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February 2024

Centre for Climate Change Economics and Policy Working Paper No. 431
ISSN 2515-5709 (Online)

Grantham Research Institute on Climate Change and the Environment
Working Paper No. 406
ISSN 2515-5717 (Online)

Economic and Social Research Council

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Suggested citation:
Basaglia P, Isaksen ET, and Sato M (2024) *Carbon pricing, compensation and competitiveness: lessons from UK manufacturing*. Centre for Climate Change Economics and Policy Working Paper 431/Grantham Research Institute on Climate Change and the Environment Working Paper 406. London: London School of Economics and Political Science

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

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This version: February 8, 2024

Abstract

Carbon pricing is often paired with compensation to carbon-intensive firms to mitigate carbon leakage risk. This paper examines the causal impacts of compensation payments for indirect carbon costs embodied in electricity prices. We use confidential UK administrative microdata to exploit firm-level inclusion criteria in both difference-in-differences and regression discontinuity frameworks. Our findings suggest that compensated firms increased production and electricity use relative to uncompensated firms, with no significant effect on energy intensity. While compensation lowers leakage risk, it also implies large forgone opportunity costs of public funds and increased mitigation costs of meeting national emission targets.

Keywords: carbon pricing, compensation schemes, competitiveness, electricity consumption

JEL codes: Q52, Q58, Q4, Q41, H23

First version: January 2019. We are grateful to Frank Venmans, Koichiro Ito, Andreas Lange, Moritz Drupp, Grischa Perino, Thomas Sterner, John Van Reenen, Luca Taschini, Aurelien Saussay, Yang Zheng, Francesca Diluiso, Andreas Gerster, Stefan Lamp, Gregor Singer as well as audiences at the 24th/25th EAERE Conference, 7th IZA Workshop on Environment and Labor Markets, 6th LSE/Imperial/King's Workshop in Environmental Economics, 5th FSR Climate Annual Conference, 13th International Workshop on Empirical Methods in Energy Economics, USITC workshop on Trade and Environment, Cambridge CEENRG Seminar, Umeå CERE Seminar series, TED University sustainability Seminar series, and LSE PRINZ seminar for helpful comments and feedback. We wish to thank Amy Richards and Paul Irving at the (former) UK Department for Business, Energy & Industrial Strategy for their invaluable help and insights. Piero Basaglia acknowledges support from the DFG under Germany's Excellence Strategy (EXC 2037 and CLICCS) project no. 390683824, contribution to the Center for Earth System Research and Sustainability (CEN) of the University of Hamburg. Misato Sato gratefully acknowledges support from the Grantham Research Institute on Climate Change and the Environment, at the London School of Economics, and the Economic and Social Research Council grants Centre for Climate Change Economics and Policy (CCCEP) (ref. ES/R009708/1) and PRINZ (ES/W010356/1). Elisabeth Isaksen and Misato Sato also acknowledge support from the Research Council of Norway (grant no. 295789). For the purpose of open access, the authors have applied a Creative Commons attribution (CC BY) licence to any Author Accepted Manuscript version arising.

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1 Introduction

Policies to establish a carbon price have proliferated in recent years. Currently, 73 of such initiatives collectively cover 23% of global emissions (The World Bank, 2023). While carbon pricing is considered an essential part of the solution to achieving a cost-effective decarbonization of the economy, there is a long-standing concern that carbon price incentives are being compromised by the concessions offered to industry (Fischer and Fox, 2007; Sterner and Muller, 2008; Rosendahl, 2008). For example, the EU Emissions Trading System (EU ETS) gives energy-intensive sectors free allocation of allowances for the direct costs of carbon emissions. Additionally, some countries also compensate electro-intensive firms for the indirect carbon costs embodied in electricity prices. These cost containment measures make the policy more politically acceptable and are increasingly justified on grounds of alleviating carbon leakage risk[1] (Sato et al., 2022), thus target energy-intensive manufacturing firms operating in regional or global markets with limited ability to pass through carbon costs to consumers (Ganapati, Shapiro and Walker, 2020).

By shielding firms from the full carbon cost, however, such compensation may compromise efficient carbon price incentives to decarbonize industrial production and consumption. Studies have shown that adjusting free allocation volumes over time can create incentives for polluters to emit more in the present to obtain more free allocations in the future (Rosendahl, 2008)[2] contrary to earlier claims that market outcomes and efficiency are independent of how allowances were allocated (Montgomery, 1972).[3] Compensation linked to current production volumes essentially provides an implicit production subsidy and dampens the carbon price signal (Fischer and Fox, 2007; Fowlie, Reguant and Ryan, 2016; Meng, 2017), also limiting carbon cost pass through to consumers and foregoing demand side substitution. This means that to achieve the overall emission reduction targets, the mitigation burden shifts elsewhere (to other sectors or towards greater emissions intensity improvements), which means carbon prices and overall costs rise. This perverse production incentive effect has been high-

[1]Carbon leakage is often defined as a policy-induced relocation of emissions to countries with more lenient carbon policies.

[2]This is known as "output-based" allocation and is in contrast to allocation based on historic output or emissions known as "grandfathering" or "ex-ante" allocation.

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

lighted in the literature (Fischer, 2001; Demailly and Quirion, 2008; Böhringer, Carbone and Rutherford, 2012; Fischer and Fox, 2011) but downplayed in policy debates arguably due to the lack of robust empirical evidence.

This paper contributes to the literature by empirically examining UK manufacturing firms' responses to an indirect carbon cost compensation scheme. Starting in 2013, the EU ETS allows participating states to partially shield electro-intensive firms from the indirect carbon cost induced by emissions trading, due to carbon cost pass-through in the power sector (European Commission, 2020*b*). This is expected to continue, for example, Germany, France and Poland have committed to compensating in total an estimated €27.5 billion, €13.5 billion and €10 billion, respectively, between 2021 and 2030 (European Commission DG Competition, 2022). Given the large fiscal implications involved and number of countries compensating indirect carbon costs, there is surprisingly little empirical evidence on their impacts.[4] Of the Member States providing compensation the UK's compensation was relatively generous because electricity prices reflect relatively high carbon costs induced not only by the EU ETS but also the carbon price floor implemented in 2013 that more than tripled the cost of power sector emissions.

We combine two quasi-experimental research designs, namely a difference-in-difference (DiD) design with inverse propensity score weighting and a "fuzzy" regression discontinuity (RD) design. The two methods complement each other by addressing different types of potential selection biases, and by providing different types of treatment estimates. In both approaches, we exploit the variation caused by the UK eligibility rules for receiving the compensation to identify effects. To be eligible for the program, a firm first needs to operate in a 4-digit NACE industry that is deemed eligible for compensation.[5] Second, eligible firms need to document that the *firm's* overall electricity costs as a share of gross value added (GVA) amounts to at least 5%, where calculations are based on historical values.[6] Third, the firm needs to apply to the compensation scheme,

[4]The UK, Germany, Belgium, the Netherlands, Greece, Lithuania, Slovakia, France, Finland, Luxembourg, Poland, Romania Spain, and Norway all provide monetary compensation to electro-intensive firms for higher indirect carbon costs induced by the EU ETS in 2020 (European Commission, 2020*b*). The total compensation distributed in 2017 by EU countries (for indirect costs incurred in 2016) amounted to €694 million (European Commission, 2018).

[5]NACE refers to the industry standard classification system used in the European Union.

[6]Note that this criteria is calculated at the firm level, i.e., the legal entity, and not at the plant level. This means that for firms operating multiple plants where some are very electro-intensive

documenting that it meets the two eligibility criteria. The second and third requirements imply that there are likely both compensated and uncompensated firms operating plants in the same narrowly defined industries, which we can exploit 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 assessed at the firm level, the amount of compensation paid is calculated at the plant level and is linked to the plant's output. Compared with firm-level analysis, more disaggregated plant-level data are advantageous because firms may operate multiple plants across different sectors. In the analysis, we are comparing similar plants belonging to compensated and non-compensated firms to isolate the effects of compensation for indirect carbon costs, going well beyond previous analysis relying on cross-sectoral or cross-country variation (Ferrara and Giua, 2022).

As a first step, we develop a static conceptual framework to elucidate how compensation payments affect firms' adaptation to indirect carbon costs. The compensation payout is based on historical output multiplied by an electricity intensity benchmark, but if an installation significantly extends (reduces) its production, then baseline output can be increased (reduced) to reflect the capacity or production changes. Our framework illustrates how, analogous to output-based free allocation in emissions trading, firms receiving compensation for the indirect carbon costs embodied in electricity prices face weaker incentives to contract output, while the incentives to improve electricity intensity of production remain intact. As a consequence, the overall electricity use is expected to increase for compensated firms compared to uncompensated firms.[7]

Our empirical analysis delivers three key results. First, in line with our theoretical prediction, we find that compensated plants increased production relative to non-compensated plants. Results from the DiD estimation show that compensation led to an increase in the sales of own goods by around 16% in the

while others are not, there may be electro-intensive plants belonging to firms that do not pass the 5% eligibility test.

[7] Analytical models on this topic tend to compare one allocation approach over another e.g. Hagem, Hoel and Sterner (2020); Fowlie, Reguant and Ryan (2016). Our model instead compares the effect of treatment on compensated firms with that on non-compensated firms.

post-treatment period (2013–2015). These results are supported by the fuzzy RD design, where we find a 30% increase in own sales for the compensated plants, with an estimated lower bound of 26% (reduced form estimate). Second, our results point to an increase in electricity use (measured in physical units) of around 22% as a result of compensation accompanied by no significant changes in electricity intensity. Relatedly, we additionally document an increase in carbon emissions of approximately 22% for compensated plants vis-á-vis their uncompensated counterparts.[8] Finally, we find that energy intensity (scaled by sales) did not experience any significant changes in both the DiD and RD designs. Overall, we find robust evidence in line with our theoretical predictions that incomplete carbon price internalization created by output-based compensation provisions for carbon and energy-intensive industries weakens incentives to reduce output and hence overall energy consumption. Our DiD findings exhibit robustness across a range of tests, including variations in the time frames used to compute p-scores, industry-specific effects defined at different digit levels, sample trimming based on electricity intensity to mitigate the influence of outliers, considering different time horizons in the estimations, extended post-treatment periods, and the utilization of diverse proxies for production and energy usage. Additionally, our results from the RD design are robust to multiple bandwidth selections and alternative functional forms (Lee and Lemieux, 2010).

Our findings provide several important policy implications for carbon pricing in the UK and elsewhere where free allocation, compensation and exemptions remain commonplace (European Commission, 2020*a*; The World Bank, 2023).[9] While carbon leakage may have been limited,[10] industry compensation represents a substantial forgone carbon tax revenue that could be employed towards driving forward the transition to net zero. We find robust evidence that compensation encourages firms to increase production and thereby pollute more, shifting the mitigation burden elsewhere in the economy where emissions abatement may be costlier (Martin et al., 2014). Moreover, output-based

[8]Due to the small sample size, the effects of compensation on electricity use in physical units, carbon emissions and the associated measures of electricity intensity are only produced in the DiD design.

[9]Even in the EU where the Carbon Border Adjustment Mechanism (CBAM) will be introduced in 2026 to reduce the risk of leakage, free allocation is scheduled to continue until 2035 (Morgado Simões, 2023).

[10]Given large volumes of free allocation, it is not surprising that studies on the EU ETS find limited evidence to support leakage (Naegele and Zaklan, 2019; Verde, 2020)

compensation to industry also limits cost pass through, thus also hindering mitigation through demand-side response (Quirion, 2009). Our results hence underscore the need for complementary measures to encourage consumers to substitute away from energy- and carbon-intensive goods.

Our paper contributes to a broader literature on the incentives effects of industry compensation in climate policy. Free allocation in emissions trading and the distortions that can arise from specific designs of free allocation rules have been extensively studied. For example, *ex-ante* free allocation based on historic activity can generate large windfall profits (Laing et al., 2014) and over-allocation (Martin et al., 2014), and lead to early action problems, distorting investment decisions or reducing incentives to phase out inefficient technologies (Sterner and Muller, 2008; Venmans, 2016) but can be rectified through benchmarking (Neuhoff, Martinez and Sato, 2006; Zetterberg, 2014); closure provisions create incentives to delay exit (Verde, Graf and Jong, 2019); combining free allocation with activity thresholds create incentives to artificially inflate output in low-activity installations (Branger et al., 2015). Our empirical analysis particularly complements literature on output-based free allocation that primarily uses theoretical and modelling approaches and highlights perverse production incentives while improving leakage outcomes (Fischer, 2001; Fischer and Fox, 2007; Demailly and Quirion, 2008; Böhringer, Fischer and Rosendahl, 2014). Rosendahl and Storrøsten (2015) show that output-based allocation (OBA) in general gives stronger incentives to improve abatement technology due to a higher permit price but the effects of OBA is heterogeneous across types of firms and sectors. Finally, research has shown that opportunity costs of compensation are high in part because they are coarsely or ill-targeted (Martin et al., 2014; Fowlie and Reguant, 2022).

Some papers have explored other carbon cost compensation measures including refunding of emission payments (Martin et al., 2014; Hagem, Hoel and Sterner, 2020), and relatedly, exemptions and rebates for energy taxes (Ito, 2015; Gerster and Lamp, 2023).[11] On the compensation scheme for indirect carbon costs, to our knowledge, there is only one other empirical analysis (Ferrara and Giua, 2022), but their empirical approach using firms in other countries or sec-

[11] In contrast to exemptions for energy- and carbon-related taxes, the CO_2 price compensation scheme is designed in a way that aims to restore some of the incentives created by the initial carbon pricing policy. We would therefore expect the mechanism and impacts to differ from an exemption scheme.

tors without compensation as controls is problematic.[12] We are the first paper to rigorously examine the effects of indirect carbon cost compensation.

Our study also complements and expands the knowledge base on how carbon pricing affects carbon and energy-intensive firms (Martin, De Preux and Wagner, 2014; Petrick and Wagner, 2014; Aldy and Pizer, 2015; Klemetsen, Rosendahl and Jakobsen, 2020; Marin and Vona, 2021; Dechezleprêtre, Nachtigall and Venmans, 2023; Colmer et al., 2023)[13], including the specific papers on the UK Carbon Price Floor (Abrell, Kosch and Rausch, 2022; Leroutier, 2022). The latter studies examine the direct impact of the policy on decarbonizing the UK electricity sector, while we study the indirect effects of carbon pricing via higher electricity prices, as well as how these indirect costs are mediated through a compensation scheme.

The remainder of the paper is structured as follows. We first lay out a simple conceptual framework to characterize the compensation scheme's impact on firms in Section 2. We then give some essential policy background on the UK carbon pricing and compensation scheme, introduce the data, and provide descriptive statistics in Sections 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 carbon abatement, before we conclude in Section 7.

2 Conceptual framework

Here we use a simple framework to characterize the theoretical predictions of manufacturing plants' behavior in the presence of indirect carbon costs with and without compensation, drawing inspiration from Hagem, Hoel and Sterner (2020) and Fowlie, Reguant and Ryan (2016).

[12]To distinguish the compensation scheme's causal effect from other factors unrelated to the program is difficult under this choice of control group. The countries self-selecting into giving out compensation are likely to be different from other countries in terms of observable and unobservable factors. The sectors selected for compensation have been assessed as energy intensive and at high risk of relocation, hence likely to be different from non-eligible sectors.

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

Suppose that production causes direct carbon emissions from the combustion of fossil fuels (e_i is the emission intensity or emissions per unit of output q_i for firm i) as well as indirect carbon emissions through the use of electricity (el_i is the 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 reducing production (q_i) and/or by lowering the respective intensities – by installing abatement equipment to lower e_i or electricity saving technology to lower el_i. Suppose that firms face two types of carbon costs. First, firms pay a *direct carbon cost* that depends on the output, q_i, the emission intensity, e_i, and an equilibrium emission permit price, τ, (or more generally, the monetized damages associated with an additional tonne of carbon emissions). Second, firms face an *indirect carbon costs* via carbon embodied in electricity prices that is a function of output, q_i, the electricity intensity, el_i, and the electricity price, $p_{el}(\tau_{el})$. Note that the electricity price is a function of the carbon tax levied on the power sector: $p_{el}(\tau_{el})$.[14]

We consider a sector that consists of firms indexed by i=1,....,n with each firm producing quantity q_i of a homogeneous good, operating in perfectly competitive global markets where all firms are price takers.[15] We apply the standard assumptions that marginal costs of production is positive and increasing: $c_i' > 0$, $c_i'' > 0$ and abstract from exit and entry decisions. The profit of a single plant is given by:

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

where p is the product price, $c_i(q_i)$ is 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 $\varphi_i(q_i, e_i(y_i), \tau)$ indicates the direct carbon costs and the parameter $\psi_i(q_i, el_i(z_i), p_{el}(\tau_{el}))$ indicates 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 units

[14] Assuming 100 % pass-through of carbon taxes in the power sector, the full carbon cost associated with electricity generation is born by the users of electricity.

[15] This assumption is in line with the UK Government's underlying assumption of UK firms being unable to pass through domestic carbon taxes to product prices.

of production multiplied by an industry-specific emission intensity benchmark, $\bar{e}_j$ i.e. output-based allocation. Thus, direct carbon costs (φ) to the firm will be:

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

(i) No compensation for indirect carbon costs

Under no compensation for indirect carbon costs, the cost of electricity consumption (ψ) to the firm will be:

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

where τ_{el} is the carbon price faced by electricity generators.[16] 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 Equation (4) expresses the marginal cost of reducing electricity use through output reductions, and the left-hand side of Equation (5) expresses the marginal cost of reducing electricity use through technology investments.

(ii) Compensation for indirect carbon costs

If compensation is introduced to offset the indirect carbon cost component of electricity prices, based on industry-specific electricity intensity benchmarks

[16] Assuming complete cost pass-through in the electricity sector, the carbon price faced by UK power plants will be equal to: $\tau_{el} \equiv \tau$ + Carbon Price Support.

(denoted by $\bar{el}_j$) and baseline output subject to dynamic updating,[17] then it follows that the cost of electricity consumption (ψ) to the firm will be:

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

where $\bar{el}_j$ is the electricity intensity benchmark for industry j (tCO_2/tonne) and A is the aid share.[18,19]

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 w.r.t. the electricity-saving investment z_i is the same regardless of the compensation payments for the indirect carbon costs. 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 w.r.t. output, q_i, has changed relative to no compensation: (4) $\neq$ (7). From (7) we see that the marginal cost of lower electricity use through output reductions is no longer equal to the

[17]Both assumptions match how compensation payments for higher electricity costs induced by the EU ETS are calculated across Member States, where baseline output is updated on a quarterly basis and electricity consumption efficiency benchmarks (in MWh/tonne of output and defined at Prodcom 8 level) are defined as the product-specific electricity consumption per tonne of output achieved by the most electricity-efficient methods of production for the product considered (EU 2012/C 158/04).

[18]Over the time frame considered in this paper, the EU Commission recommendations state that aid intensity should not exceed 85% of the eligible costs incurred in 2013, 2014 and 2015 and 80% of the eligible costs incurred in 2016 (EU 2012/C 158/04).

[19]In the case of complete pass-through of the power sector carbon price τ_{el} to electricity prices, $A_i = 1$, and $el_i = \bar{el}_j$, the compensation per tonne received by the firm would equal the increased electricity cost per tonne due to the higher τ_{el}. If instead $el_i < \bar{el}_j$, compensation payments per unit of output will be larger than the carbon price-induced increase in the electricity price.

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 increases the cost of reducing electricity use through output reductions, as reduced production leads to lower compensation payments – this marginal loss of compensation via reduced output equals $\bar{el}_j \cdot \tau_{el} \cdot A_i$. Hence, the firm's marginal cost of reduced output exceeds the social cost of reduced output. While higher electricity prices induced by carbon pricing in the power sector make production more costly, compensation payments make production less costly.

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 contract 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 contract less vis-á-vis uncompensated plants*

These predictions compare the effects of output-based compensation on treated and non-treated firms, in contrast with the predictions in previous papers, which compare the effects of output-based compensation on treated firms vis-á-vis other allocation methods such as auctioning or grandfathering (e.g. Fowlie, Reguant and Ryan, 2016; Rosendahl, 2008).

In the following sections, we empirically test these theoretical predictions by applying a difference-in-difference and regression discontinuity design to the UK indirect carbon cost compensation scheme. In the next section, we describe the research design 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, so it is essential to understand how the relevant policies were rolled out.

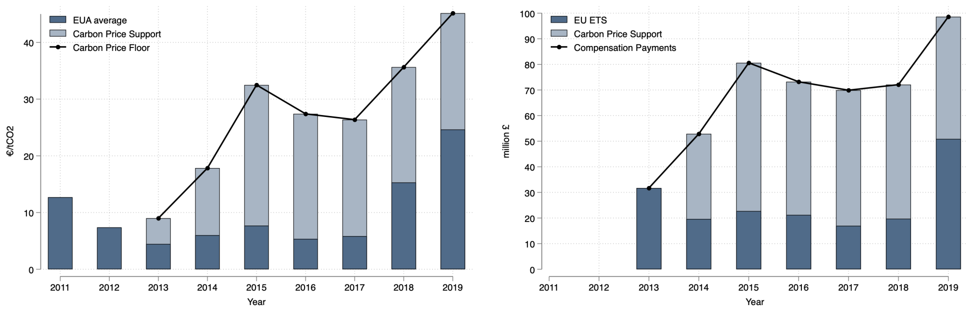
In 2005, a EU-wide carbon price was introduced for the manufacturing and power sectors with the introduction of the EU ETS. The carbon price can affect manufacturing firms in two ways. First, regulated firms have to purchase and surrender EU Allowances (EUAs) for each tonne of CO_2 emitted in the previous 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 on to consumers (Sijm, Neuhoff and Chen, 2006; Fabra and Reguant, 2014; Hintermann, 2016). To prevent carbon leakage, the ETS Directive gives free allocation to leakage-exposed sectors to limit their exposure to direct carbon costs. Since 2013, "the 2012 Guidelines" also allowed EU ETS countries to grant State aid to compensate selected electro-intensive industries for indirect carbon costs (European Commission, 2020*b*).

In the UK, in addition to the EU ETS, a Carbon Price Floor was unilaterally introduced on April 1 2013, applying only to electricity generation and immediately raising the carbon price faced by UK power plants. The initial idea of the policy was to first set the desired carbon price floor (path) and then stipulate the tax needed to top up the EUA price with the Carbon Price Support (CPS). From 2016, however, the UK Government decided to freeze the CPS at £18/tCO_2, which meant that the policy effectively functioned as an additional tax on carbon emissions that came on top of the EUA price. As seen from Panel (a) in Figure 1, CO_2 prices faced by power plants were 2-5 times larger than the EUA price.

The UK CPS was expected to accelerate the decarbonization of the UK power sector and came in response to the general concern in the years leading up to phase III of the scheme that the EUA price was too low (UK BEIS, 2019); in 2012, the average allowance price was around €7/tCO_2. But simultaneously, it spiked substantial concerns about leakage and loss of competitiveness of UK

Figure 1: Carbon prices and compensation payments in the UK

(a) Carbon Price Floor (€/tCO2) **(b)** Indirect carbon cost compensation (£m)



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, 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.

electro-intensive manufacturing firms vis-á-vis competitors abroad.[20,21] To mitigate the potential adverse effects on domestic firms and win political support, the CPS was accompanied by a compensation scheme for the additional costs it entailed. This was meant to start in 2013, but was only approved by the EU Commission in March 2014, when it came into effect. This was combined with another compensation introduced in January 1, 2013 for the indirect carbon costs induced by 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 were the compensations. Panel (b) in Figure 1 summarizes the annual payments made by the UK government for compensation for EU ETS and CPS indirect carbon costs, demonstrating an upward trend in correlation with the rise in the price of EUA allowances in more recent years.

[20]Even before 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).

[21]Grubb and Drummond (2018) quantify the relative contribution of various components to UK industrial prices. They stipulate that costs induced by the CPS and the EU ETS accounted for approximately 25% of the industrial electricity price in 2016. Cambridge Econometrics (2017) report a lower number: As a proportion of the industry electricity price in 2016, the indirect EU ETS carbon cost and the CPS amounted to around 9%.

Eligibility

We exploit a discontinuity in the eligibility rules for indirect carbon cost compensation to test the effect of the compensation on firms' economic and environmental outcomes. Eligibility for compensation for the indirect costs of both the EU ETS and the Carbon Price Support was based on two criteria. First, the firm needs to manufacture a product in the UK within an eligible sector defined by the 4-digit NACE code. The European Commission selected a list of eligible sectors with a high risk of carbon leakage.[22] Appendix Table A.1 lists the 15 eligible industries according to the 4-digit NACE code (European Commission, 2012).[23]

With the aim of a more targeted compensation scheme, the UK Government also imposed a second eligibility criteria: a firm needs to show that its indirect carbon costs (the combined costs of EU ETS and the Carbon Price Support) would amount to 5% or more of its gross value added. Specifically, this so-called 5 % filter test was calculated in the following way:

$$\frac{\text{electricity consumption (MWh) x price impact (£/MWh)}}{\text{Gross Value Added (£)}} \geq 5\%, \qquad (9)$$

where electricity consumption and gross value added (GVA) are average values for the period 2005-2011, and the price impact was set to £19/MWh in real 2007 prices. As calculations were based on historical values, there was a limited ability for firms to adjust consumption or production to ensure that they were eligible for compensation. Both electricity costs and GVA had to be calculated at the aggregate legal entity level, i.e., the firm. For multi-plant firms, this implied that parts of the electricity use and GVA might stem from activity unrelated to the manufacture of the eligible product(s). If these activities were less energy intensive, it would lower the firm's average electricity intensity, and hence make it harder to meet the eligibility criteria.

Even if a firm meets the two criteria, it also needs to submit an application

[22] Generally, the compensation schemes need to comply with the principles set out in the Environmental and Energy Aid Guidelines and the ETS State and Guidelines adopted by the European Commission. The first set of guidelines states that Member States are allowed to partially compensate large electricity users for the indirect costs of taxes on energy products, when those taxes have the same aim and effect as the ETS carbon allowance price. The criteria for choosing eligible firms and calculating compensation levels need to be the same as those in the ETS State aid Guidelines.

[23] This list was subsequently revised down from 15 to 10 in 2020.

to receive the compensation. Crucially for identification, the multiple criteria implies that we might have three types of firms within a narrowly defined eligible industry: (i) firms that passed the 5% filter test and received compensation, (ii) firms that *would* pass the 5% filter test, but did not apply, (iii) firms that did not pass the 5% filter test. This makes it possible to exploit within-industry variation to estimate impacts of the compensation scheme.[24]

Compensation calculation

While the 5% test requires a calculation at the aggregate *firm* level, the amount of compensation is calculated based on *installation* level data.[25,26] Compensation payments based on installation-level data are calculated using the following formula:

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

The baseline output corresponds to the average production of the eligible product in tonnes per year at the installation over the reference period 2005–2011. However, if an installation significantly extended its production, the baseline output could be increased in proportion to the production extension. Also, if an installation significantly reduced its production, the aid would be reduced

[24] In addition to the criteria listed, 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, as defined by the 8-digit Prodcom classification. Additionally, a firm is also granted compensation if it can demonstrate that it failed the 5% test because of the inclusion of business activity that did not relate to the manufacture of the eligible product(s).

[25] In the compensation scheme an installation was defined as a stationary technical unit where one or more activities associated with the manufacture of the eligible product are carried out.

[26] It is then possible that two plants have the exact same electricity intensity (el/GVA) but only one of the plants are eligible for compensation because the plant's owner firm passes the eligibility test. The ineligible plant might be part of a multi-plant firm, where the other plants are less energy intensive. Generally, we would expect that firms with a secondary industry code that makes them eligible are less likely to receive compensation compared to firms with a primary industry code that is eligible.

according to a stepwise function.[27] Payments to firms are made quarterly, and firms were required to inform the UK Government quarterly of any significant increases or reductions in their production. There is hence a degree of dynamic updating of the baseline, which means that compensation payments can potentially be affected by a firm's recent production.

3.2 Data sources

To examine the indirect effect of carbon pricing on manufacturing, we combine several data sources at the firm and plant levels, primarily confidential microdata from the UK secure data lab. While the disaggregated data offers rich detail, it also poses challenges for analysis due to the relatively small sample size because some data sources are surveys.

Compensation schemes: A list of firms that received compensation for indirect carbon costs between 2016 and 2019 is publicly available from the Department for Energy Security and Net Zero (DESNZ)[28] website. We assume that the same firms also received compensation for the years 2013 to 2015.[29] There were in total 59 firms that received compensation in 2016 for the indirect costs induced by the EU ETS and the Carbon Price Support.

Economic data: We use plant-level data[30] on employment and economic outcomes from restricted microdata maintained by the Office for National Statistics (ONS). Our core dataset is the Annual Business Survey (ABS), which is

[27] If production was reduced by less than 50%, there would be no reduction in the aid amount. If reduced between 50% and 75%, an installation would only receive 50% of the aid amount. If reduced by 90% or more, and installation would not receive any compensation. Conditional on eligibility, there may be perverse incentives around the thresholds to artificially inflate production especially during economic downturns in order to receive full compensation as documented in the case of ETS free allocation by Branger et al. (2015).

[28] Formerly Department for Business, Energy & Industrial Strategy (BEIS)

[29] While information on which firms received compensation before 2016 is not publicly available, we were told in conversations with the former Department for Business, Energy & Industrial Strategy (BEIS) that it is safe to assume that the list of firms are approximately the same as for 2013-2015.

[30] A "plant" corresponds to a "reporting unit", which holds the mailing address for the business and is the unit for which businesses report their survey data to the UK Office for National Statistics. A reporting unit represents an aggregation level that is more granular than an "enterprise unit" (which may be subdivided into several reporting units) and more aggregated than a "local unit" (which may be combined to form one reporting unit to reduce compliance costs). It is the lowest aggregation level for which most business data are available. Within our sample, around 16% of compensated enterprise units represent multi-plant firms. For more details see Criscuolo, Haskel and Martin (2003).

an annual survey of businesses covering production, construction, distribution, and service industries. ABS is the largest business survey conducted by the ONS and covers around 62,000 plants. The sample design is a stratified random sample using three stratification variables: employment, geography, and the 4-digit Standard Industrial Classification (SIC) code. From the ABS, we collect information on SIC codes, employment, sales of own goods, production value, turnover, gross value added (GVA), and energy expenditures for the period 2005 to 2019.[31,32] Monetary values are adjusted for inflation, with 2010 serving as the base year, based on official inflation statistics.

Energy and Electricity use: To examine how electricity use is impacted by carbon pricing and the compensation scheme, we collect detailed information from the Quarterly Fuels Inquiry (QFI). The QFI provides quarterly information on the value and the quantity of fuels used by a small sub-sample of UK manufacturing plants. Before 2008 the survey covered around 1200 plants, while after 2008, the survey only covered around 600 plants. The survey is maintained by the ONS on behalf of the DESNZ. Unfortunately, this data is not available beyond 2015. Observations are aggregated to the annual level and then linked to the ABS. Because the QFI covers a smaller sample than the data on economic variables and is not available beyond 2015, to have sufficient power to test some of our hypotheses, we rely on reported energy costs from the ABS as a proxy (see section 3.4).

Electricity related indirect emissions: To calculate indirect carbon emissions embodied in electricity, we combine detailed electricity use in physical units from the QFI with emission factors provided by the UK DESNZ.[33]

3.3 Descriptive statistics

Table 1 is based on plant-level microdata from the ABS and the QFI and shows summary statistics by compensation status. The sample is restricted to manu-

[31] This includes a period of economic turmoil following the 2016 EU Referendum in the UK. We show in robustness tests that the results are consistent including 2016-2019.

[32] The ABS was merged with the names of compensated firms by first manually matching the compensated firms names with Bureau van Dijk's Orbis data to obtain the Company Registration Number (CRN), which can then be linked to the company IDs in the confidential data (Enterprise Reference Number).

[33] Government conversion factors for company reporting of greenhouse gas emissions can be found here.

facturing industries (SIC 7-33).

To test our first hypothesis on the effect of compensation on production, we use sales of own goods as our main dependent variable and proxy for production volumes, and other proxies including total output, GVA, and total turnover in robustness checks (see Panel A). Given that protecting jobs is a frequently used argument to justify compensation, we also examine the effects of the compensation scheme on employment but regard this outcome as less tightly linked to production volumes. Comparing compensated and non-compensated plants, we see from Panel A in Table 1 that compensated plants are larger than the non-compensated manufacturing plants in terms of both production, employment, and gross value added. We also see that there is a limited number of compensated plants in our sample, ranging from 70 to 119 depending on the variable of interest. By contrast, the number of non-compensated manufacturing plants is between 8,976 and 16,180 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) and energy purchases as a share of sales (Panel A), we also provide results for a wide range of intensity measures in robustness checks.

To test the third hypothesis on the effects on electricity consumption, we focus on electricity use in kWh (Panel B) as the variable that is closest to what we would like to test. However, due to the smaller sample size in the QFI (34 compensated plants and 739 non-compensated plants), we also examine the effects on the energy purchases variable from the larger ABS (Panel A) even if this variable is a proxy for electricity use.

Comparing compensated and noncompensated plants, we see that compensated plants are larger, use more energy, and are more energy-intensive than non-compensated plants. Clearly, we need to account for this selection bias in our estimation in order to recover causal estimates of the compensation scheme.[34] Table 1 also highlights the challenge we face in terms of sample size, with the limited number of compensated plants in our sample relative to the number of noncompensated manufacturing plants particularly for the QFI sample.

[34] Additional descriptive evidence on our key outcome variables, including plots showing the development in variables over time, are provided in Appendix B.

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	14770	3.264***
	(1.506)		(2.223)		(0.211)
Total output	10.36	112	7.208	15503	3.149***
	(1.457)		(2.182)		(0.207)
Total turnover	10.38	112	7.303	15713	3.073***
	(1.436)		(2.167)		(0.205)
Production value	10.98	70	7.157	8976	3.819***
	(1.219)		(2.483)		(0.297)
GVA (Market Prices)	16.04	118	13.37	15463	2.662***
	(1.447)		(2.074)		(0.191)
Employment	5.157	119	2.925	16180	2.233***
	(1.161)		(1.694)		(0.156)
Productivity (turnover / employment)	5.432	118	4.345	15708	1.087***
	(0.806)		(0.900)		(0.0831)
Energy purchases (£)	6.583	99	3.275	15272	3.308***
	(1.791)		(2.232)		(0.225)
Energy purchases /Sales	-3.124	118	-3.898	14284	0.773***
	(0.879)		(0.893)		(0.0825)
Energy purchases /Output	-3.192	120	-4.035	15039	0.843***
	(0.868)		(0.931)		(0.0853)
Energy purchases /Turnover	-3.288	121	-4.118	15278	0.830***
	(0.989)		(0.917)		(0.0837)
Energy purchases /Production	-3.052	77	-3.989	8590	0.937***
	(1.094)		(1.014)		(0.116)
Energy purchases /GVA	-8.854	119	-10.15	14934	1.299***
	(1.175)		(1.169)		(0.108)
Energy purchases /Employment	1.789	109	0.241	15310	1.547***
	(1.324)		(1.184)		(0.114)
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)
Electricity use / Output	6.005	30	4.891	707	1.114***
	(0.942)		(1.197)		(0.221)
Electricity use / Turnover	5.839	33	4.785	726	1.054***
	(1.196)		(1.230)		(0.219)
Electricity use / Production	6.145	25	5.224	321	0.921***
	(0.967)		(1.125)		(0.232)
Electricity use / GVA	0.530	32	-0.992	720	1.522***
	(1.306)		(1.331)		(0.240)
Electricity use / Employment	11.53	31	9.712	728	1.819***
	(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	15248	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)

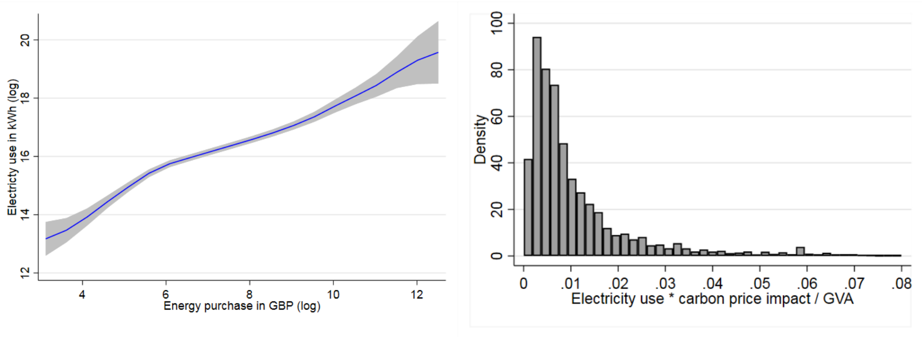
Notes: The table shows summary statistics at the plant level for the period 2005–2011, which is the baseline period used to determine eligibility for the compensation scheme. All variables are in logs. The sample is restricted to manufacturing industries (SIC 7-33). N refers to the number of plants. Source: ABS and QFI. *See Section 3.4 for details.

3.4 Using predicted electricity use to calculate the eligibility criterion

One key data challenge we face is the limited availability of plant-level data on electricity consumption. The QFI is a relatively small sample and data on electricity use in kWh is only available for a small subset of plants (Table 1, Panel B) 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 up to 2019. Panel (a) in Figure 2 shows the strong and positive relationship between electricity use and total energy purchases. The raw correlation ranges from 0.91 to 0.93, depending on sample restriction (see Table C.1 in Appendix C where we detail the procedure used to make out-of-sample predictions of electricity consumption).[35]

Figure 2: Predicting electricity consumption from energy purchases

(a) Electricity - Energy correlation | **(b)** Plants by the electricity cost share



Notes: Panel (a) plots the correlation between log electricity use and log energy purchase in 2011, with 95% confidence interval and local smoothing. Panel (b) shows the distribution of plants by the electricity cost share, using the formula outlined in Equation 9 and predicted electricity use. Data source: the Annual Business Survey (ABS) and the Quarterly Fuels Inquiry (QFI). The population is restricted to plants in SIC 7-33 industries.

Predicted electricity consumption is then used to calculate the electricity cost intensity for all plants in the sample, to evaluate the eligibility criterion described in Equation 9. As we will see in Section 4, having a measure of the electricity cost intensity is important in the empirical strategies we use (as a matching variable in the DiD estimation and as the running variable c_i in the RD design). Note that we do not use predicted electricity use, or any variable de-

[35] In robustness tests, we use energy purchases directly to calculate an energy cost intensity, and instead infer the likely cut-off value; see Appendix D.

rived from predicted electricity use, as an outcome variable in the main analysis presented in Section 5.

Panel (b) in Figure 2 plots the distribution of the calculated electricity intensity criteria based on Equation 9. We see that most firms' intensity is much lower than 5%. There is also no detectable bunching right above the 5% criterion, which suggests that plants are not able to manipulate the running variable c_i (see Section 4.2 for more details). A McCary test also gives no indication of bunching at the 5% eligibility cut-off.

4 Empirical Strategy

Faced with challenges around selection bias and sample size, our approach to examining the indirect impacts of carbon pricing via electricity prices on manufacturing firms with and without compensation schemes in place is the following. Acknowledging that no single approach can adequately overcome all threats to identification, we pursue two empirical strategies: i) a difference-in-differences (DiD) strategy with inverse propensity score weighting and industry-specific time trends, and ii) a "fuzzy" regression discontinuity (RD) design, where we exploit the discontinuous jump in the probability of receiving compensation at the eligibility thresholds. We then compare the results from the two strategies.

4.1 Difference-in-differences

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

$$y_{ijt} = \beta_1 Comp_{ijt} + X'_{ijt} + \gamma_i + \delta_{jt} + \varepsilon_{ijt}, \tag{11}$$

where y_{ijt} is a placeholder for a relevant plant-level outcome (e.g., production, electricity use, or electricity intensity). X'_{ijt} is a vector of plausibly exogenous covariates, γ_i are firm-specific fixed effects, and ε_{ijt} is the idiosyncratic error term. The main identifying assumption is that, in the absence of compensation payments, the compensated and uncompensated firms would have followed parallel trends in the outcome variable. One potential threat to identification is industry-specific shocks. By including industry-specific time dummies, δ_{jt}, we absorb time-varying shocks at the 3-digit industry level, which means that identification is based on variation within narrowly defined industries.[36]

However, there is still the possibility of selection bias within industries across treated and non-treated groups, such as systematic differences in electricity intensity. To account for such within-industry differences in observables, we combine the DiD design with inverse propensity score weighting. Specifically, we use a propensity score estimator to reweight plants in Equation 11 to reflect the differences in the probability of getting compensation. We estimate the propensity score ($\hat{p}$) based on a proxy of the pre-treatment electricity intensity, and lagged values of the outcome variable. On the former, we estimate the propensity score based on an electricity intensity measure that is as similar as possible to the eligibility criteria (see Equation 9 in Section 3.1) where electricity intensity is defined relative to firm-level GVA. As mentioned, due to the small sample size of the QFI, where electricity use is reported, we instead use predicted electricity use to calculate the eligibility criteria; (Section 3.4). The propensity score is calculated separately for each 3-digit SIC industry, based on the period 2005-2011. These years correspond to the period used by the UK Government to calculate the electricity cost share, which again determines whether a plant passes the 5% filter test. The propensity score estimates are then transformed into weights and used in panel regressions. Specifically, we weight each compensated plant by $1/\hat{p}$, and weight each uncompensated plant by $1/(1-\hat{p})$. This allows us to recover an estimate of the average treatment effect (ATE) of compensation on the outcome of interest (Imbens, 2004).[37]

To verify if pre-treatment trends are parallel and to examine how the treatment unfolds over time, we also estimate a dynamic version of the DiD with leads and lags. Specifically, we interact the treatment variable, $Comp_{ijt}$, with time dummies, where we use the year before the first treatment year as the ref-

[36]We also show effects for 2 digit industries in robustness checks; see Section 5.1.1. Due to the small sample size, there is a trade-off between accounting for detailed industry-specific trends and ensuring that we have sufficient observations to recover precise estimates.

[37]This approach avoids discarding non-matching observations, retaining a larger estimation sample and hence greater statistical power for inference. See e.g., Guadalupe, Kuzmina and Thomas (2012) for a similar approach.

erence 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 with the following regression:

$$y_{ijt} = \sum_{m=0}^{M} \beta_{-m} Comp_{ijt-m} + \sum_{k=1}^{K} \beta_{+k} Comp_{ijt+k} + X'_{ijt}\beta_2 + \gamma_i + \delta_{jt} + \varepsilon_{ijt}, \quad (12)$$

where lead m captures potential deviations in the pre-treatment m years before treatment and lag k captures the effect of the policy k years after the start of the treatment.

Even if pre-treatment trends are parallel, and we ensure that any differences in initial electricity intensity are accounted for, there might still be a component of non-random self-selection into the compensation scheme that influences the development in production, energy use, and financial performance in the post-intervention period. For example, as firms applying to the compensation scheme will likely incur fixed costs in preparing the necessary accounting and administrative work, firms with lower levels of electricity use (but still above the eligibility threshold) might find it too costly to apply. While in principle selection effects can be addressed by adding additional (time-varying) control variables and matching on additional pre-treatment observables, selection might in part be driven by unobserved factors. It is therefore difficult to fully account for potential self-selection effects.

4.2 Fuzzy regression discontinuity design (DiDiD-IV)

In an alternative empirical approach, we take advantage of thresholds that influence the eligibility for treatment to identify causal effects.[38] In our setting, we can exploit that there is a change in the probability of treatment at two eligibility thresholds: (i) the industry code, and (ii) electricity costs are at least 5% of GVA over a baseline period. While these two thresholds may not perfectly determine whether a firm gets compensation, they still create a discontinuity in the *probability* of treatment.

The intuition behind a fuzzy RD is related to the instrumental variable strat-

[38] 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. By contrast, a fuzzy RD allows for a smaller jump in the probability of assignment to treatment at the threshold (Imbens and Lemieux, 2008).

egy, and the fuzzy RD can be estimated using two-stage least squares. When $comp_{ijt}$ is a dummy that indicates if firm i in industry j receives compensation payments in year t, then the first stage, reduced form, and the second stage are:

First stage:

$$comp_{ijt} = \pi_1 \underbrace{post_t \times \mathbb{1}\{c_i \geq c_0\} \times \mathbb{1}\{elig_j = 1\}}_{Instrument} + X'_{ijt}\beta + \gamma_i + \mu_{ijt} \tag{13}$$

Reduced form:

$$y_{ijt} = \pi_2 \underbrace{post_t \times \mathbb{1}\{c_i \geq c_0\} \times \mathbb{1}\{elig_j = 1\}}_{Instrument} + X'_{ijt}\beta + \gamma_i + e_{ijt} \tag{14}$$

Second stage:

$$y_{ijt} = \beta_1 \widehat{comp}_{ijt} + X'_{ijt}\beta_2 + \gamma_i + \varepsilon_{ijt}, \tag{15}$$

where $post_t$ is equal to 1 for the year 2013 and onwards and 0 otherwise, $\mathbb{1}\{elig_j = 1\}$ indicates if a plant operates in a 4-digit industry eligible for compensation, and $\mathbb{1}\{c_i \geq c_0\}$ indicates if a plant's electricity intensity is above the eligibility cut-off c_0 (Equation 9 in Section 3.1). c_i is often referred to as the "assignment" or "running" variable. When the running variable exceeds the cut-off value, c_0, it induces a change in the probability of a plant receiving compensation. In our context, higher electricity intensity increases, by definition, the likelihood that a plant i will be closer to the cut-off. If the compensation scheme matters, this will induce a change in the outcome variable, y_{ijt} at the cutoff.

As not all plants that are eligible receive compensation payments, the change in the outcome variable at the cut-off needs to be rescaled by the jump in the probability of treatment, i.e.,: $\beta_1 = \frac{\pi_2}{\pi_1}$. The estimate corresponds to β_1 in the second stage estimation (Eq. 15). Using a 2SLS framework, we can estimate a weighted local average treatment effect (LATE) for the compensated firms, where the weights reflect the *ex-ante* likelihood that plant i is near the threshold (Imbens and Lemieux, 2008). This represents a LATE for a small subgroup of the sample composed of highly electricity-intensive firms close to the 5% cut-off and is therefore not directly comparable to the ATE, which is evaluated based on the entire population of plants. As shown in Figure 2b, the 5% threshold is in the right tale of the electricity intensity distribution. Therefore, the subsample of observations used for estimating the LATE represents a small group of highly electricity intensive plants.

Note that the increased probability of receiving compensation as the electricity intensity crosses the eligibility cut-off ($\mathbb{1}\{c_i \geq c_0\}$) only applies to plants operating in eligible industries ($\mathbb{1}\{elig_j = 1\}$). The instrumental variable (IV) is hence the interaction between these two indicator variables. By including $post_t \times \mathbb{1}\{elig_j = 1\}$ and $post_t \times \mathbb{1}\{c_i \geq c_0\}$ in the vector of covariates X'_{ijt}, we allow for eligible industries and plants with an electricity intensity above the cut-off c_0 to develop differently over time.[39] By exploiting variation along three dimensions (pre and post, eligible and non-eligible industries, above and below the electricity cut-off), the empirical strategy could also be interpreted as a difference-in-difference-in-difference (DiDiD) combined with instrumental variables (IV).

A causal interpretation of β_1 relies on several identifying assumptions. First, the probability of treatment has to jump at the cut-off, c_0. 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 running variable, c_i, which in our case is the industry code and the electricity cost share. The latter is based on historical electricity consumption and gross value added and is therefore difficult to manipulate. A McCary test also shows no sign of bunching around the threshold value (see Section 3.4). Industry codes are assigned to plants and should in principle not be manipulable. Third, we must assume monotonicity, i.e., that crossing the threshold cannot simultaneously cause some units to get compensation and others to move out of the compensation scheme.

Beyond these identifying assumptions, one obvious threat to identification is the small sample size, especially the small number of compensated plants included in the QFI. Given the limited number of observations close to the threshold in our data, we are forced to increase the bandwidth. This introduces the possibility of increased bias, given that a wider bandwidth increases the likelihood of systematic differences between firms positioned above and below the cut-off.

[39] We allow for several different functional forms of $post_t \times \mathbb{1}\{c_i \geq c_0\}$ in our regressions; linear and 2nd degree polynomial distance from the cut-off and equal-sized bins on each side of the cut-off. To control for $post_t \times \mathbb{1}\{elig_j = 1\}$, we combine $post_t$ with a dummy variable indicating if the 2-digit SIC industry is eligible for compensation. We include the control at the 2-digit level, as including industry-specific trends at the 3 or 4 digit level is too demanding and leaves us with very little identifying variation.

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 (Equation 11) using data from the ABS and QFI, respectively. We additionally report p-values from a mean comparison test of lagged outcomes categorized by treatment status to present corroborative evidence on the robustness of the parallel trend assumption after IPW. To recall, the ABS sample is larger than the QFI but energy purchase is used as a proxy for electricity consumption. 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. In all regressions, the sample is restricted to manufacturing industries (SIC 7-33) and plants with at least one observation in the post-treatment period.

First in terms of production, in line with Prediction 1, our results indicate that compensation led to an increase in our main proxy indicator "sales of own goods" by around 16% in the post-treatment period. This estimated effect is based on a comparison of compensated and non-compensated plants with similar electricity intensity and sales figures in the pre-treatment period; see column (1) in Table 2. The estimated treatment effect is robust across a number of tests which are presented in Section 5.1.2. In other words, our results suggest that compensation is doing its job in combating the displacement of production and carbon leakage that could arise from climate policy induced electricity price differentials. Interestingly, we do not find any significant effect on employment (cf. Table E.3), productivity or GVA (cf. Figure F.1). In other words, our results fail to support claims that carbon pricing or higher energy costs lead to job losses.

In terms of electricity intensity, both our results using the QFI (electricity use/sales, Table 3 column 2) and ABS (Energy purchase/sales, Table 2 column 3) that the difference between compensated plants and non-compensated plants is not statistically significant. This is in line with Prediction 2.

As production is higher, we find broadly that overall electricity consumption is also higher for compensated firms, broadly in line with Prediction 3. In other words, the compensation is dampening the effect of the carbon price signal on discouraging energy use and therefore emissions. Estimates using actual electricity use data from the QFI (Table 3 column 1 and 3) indicate that compensation increased electricity use by 22%, and electricity-related carbon emissions by 23%. Instead when using energy purchases data from the ABS as a proxy, we find a positive effect that is not statistically significant (Table 2, column 2).

Table 2: Average treatment effects of compensation. 2010–2015.

	Source: ABS		
	Sales of own goods (1)	**Energy purchases** (2)	**Energy intensity** (3)
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 pre-treatment: compensated	11.135	7.346	-3.147
Mean outcome pre-treatment: 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 Equation 11. Dependent variables are in logs. 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. We drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS) and Quarterly Fuels Inquiry (QFI). See reference list for full citation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Average treatment effects of compensation. 2010–2015.

	Source: QFI		
	Electricity use (1)	**Electricity intensity** (2)	**Indirect CO_2 emissions** (3)
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 pre-treatment: compensated	17.305	5.936	23.491
Mean outcome pre-treatment: 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 Equation 11. Dependent variables are in logs. 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. We drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS) and Quarterly Fuels Inquiry (QFI). See reference list for full citation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.1.1 ATEs over time

We also present the dynamic version of the DiD (Figure G.1, which plots the annual DiD coefficients estimated from Equation 12 and shows how treatment effects unfold over time. It also shows the validity of parallel pre-treatment trends leading up to 2013 when compensation was first paid out (for indirect costs incurred in 2012) – the same year as the introduction of the UK Carbon Price Support in the UK power sector. Figure G.1, Panel (a) shows that difference in production levels between compensated and non-compensated firms emerged already in 2013, but grew more in 2014. Figure G.1, 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 information both from the ABS and the QFI is available. Nevertheless, ensuring comparability across results for different variables comes at the expense of shrinking the estimation sample size. Tables 4 and 5 provide additional results for outcome variables that are available beyond that period to corroborate our findings from Table 2. The corresponding results for employment are presented in Table E.3 in the Appendix.

Table 4: ATEs of compensation on sales. 2010–2019.

	Sales of own goods				
	2015	2016	2017	2018	2019
Compensation	0.156** (0.0638)	0.164** (0.0763)	0.126* (0.0705)	0.147** (0.0701)	0.144** (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 pre-treatment (Treat)	11.135	10.867	10.831	10.744	10.743
outcome pre-treatment (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

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. We 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 5: ATEs of compensation on energy intensity (energy purchases/sales). 2010–2019.

	Energy intensity				
	2015	2016	2017	2018	2019
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 pre-treatment (Treat)	-3.147	-3.087	-2.750	-3.089	-3.090
outcome pre-treatment (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 the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. We drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.1.2 Robustness checks for DiD estimation

Our DiD results are robust to a number of tests. To mitigate concerns about how the global financial crisis might affect the computation of our p-scores, and our estimates accordingly, we show that our coefficients are robust to the use of an alternative time horizon to compute our p-scores ranging from 2010 to 2012 (see Appendix E.6). We also show how our results change when we trim the sample by dropping plants with an electricity intensity based on Eq. (9) below different thresholds to ensure that our results are not driven by sample trimming decisions (see Appendix E.4). Additionally, Appendix E.3 shows how our results change when incorporating industry-specific effects at a broader sectoral level (2-digit level), thereby trading off some precision in the identification strategy to expand our estimation sample. Finally, Tables E.9 - E.11 in the Appendix provide a set of alternative estimations relying on different proxies for production and energy intensity from the ABS sample. These findings are summarized in Figures F.1 - F.2 in the Appendix which provides a graphical comparison of the estimated effects across the array of robustness tests across all outcome variables.

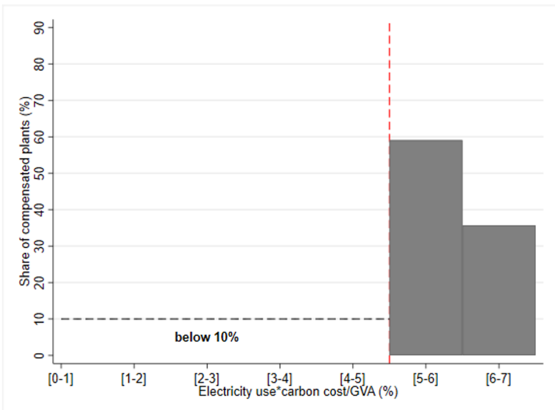
5.2 Fuzzy RD estimates of the local average treatment effects

Turning now to the RD estimation, we start by presenting the graphical evidence and estimated coefficients of the first stage and the reduced form, before turning to the instrumental variable estimates (second stage). Note that we present estimates from the first stage, reduced form and second stage using different functional forms as controls.

5.2.1 First stage and reduced form

Figure 3 illustrates the first stage, showing the share of compensated plants for different intervals of the electricity cost intensity. The sample is restricted to eligible industries, and averages within each bin are based on data from the period 2005–2011. Predicted electricity use is used to calculate the electricity cost intensity. As expected, we observe a sharp discontinuous jump in the share of compensated plants as we cross the eligibility cut-off; for plants with an electricity cost intensity between 5-6%, over half of the plants receive compensation payments. The exact height of the bars located to the left of the threshold is suppressed due to confidentiality reasons, but the share of compensated plants is below 10% for those bins.

Figure 3: Share of compensated plants by electricity cost share. 2005-2011



Notes: Figure shows the share of compensated plants by the electricity cost share (electricity use*carbon price impact/GVA), 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. Data source: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI). See reference list for full citation.

Table 6, Panel A, reports the estimated first stage based on Equation 13, where we include both eligible and non-eligible industries as well as firm- and sector-year - specific fixed effects.[40] The estimated coefficients reflect the probability of receiving compensation payments if the 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.

Table 6: Local average treatment effects of compensation. 2010–2015. Fuzzy RDD.

	Source: ABS		
	Sales of own goods (1)	**Energy purchases** (2)	**Energy intensity** (3)
Panel A: First stage	0.879***	0.879***	0.879***
	(0.101)	(0.101)	(0.101)
Panel B: Reduced form	0.264**	0.209	-0.0562
	(0.125)	(0.187)	(0.134)
Panel C: Second stage	0.301**	0.238	-0.0639
	(0.131)	(0.199)	(0.156)
Panel D: OLS	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 regression discontinuity design. Dependent variables are given by the table headings and are measured in logs. Standard errors are clustered at the firm level. The sample estimation period is 2010-2015. Cutoff value: 0.05. Bandwidth: cutoff 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-specific year fixed effects. Data source: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 4 shows graphical evidence of the discontinuous jump in our outcome variables at the threshold value, i.e., the reduced form effect. The RD plots are based on polynomial regressions over quantile-spaced bins, where we

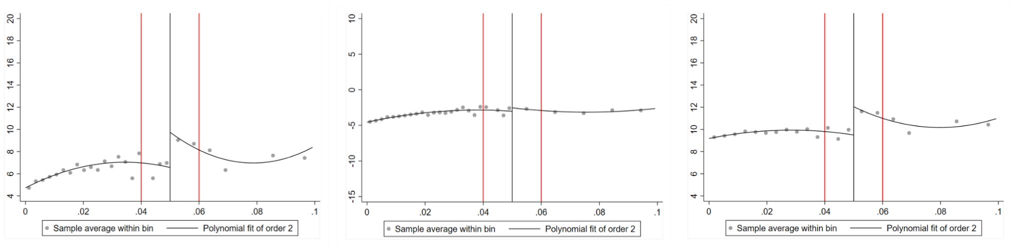
[40] Sector-year fixed effects are included at the 2-digit level. Including this at the 3-digit level of disaggregation was not possible due to issues of sample size.

follow Calonico, Cattaneo and Titiunik (2015) to determine the optimal number of bins. Each dot represents a local mean for each bin. The figure shows a jump in sales of own goods (cf. Panel (a)) and electricity consumption (proxied by energy purchases, (cf. Panel (c)) at the 5% eligibility cut-off, indicating that the compensation had an effect on these outcomes.

Table 6, Panel B, reports the reduced form coefficients estimated based on Equation 14. The coefficients represent a lower bound of the effect of the compensation scheme (in the RDD sample) as not all plants that meet the eligibility criteria receive compensation. The reduced form estimates could be interpreted as "intention to treat", which has the advantage that they do not rely on the exclusion restriction for unbiasedness. A statistically significant jump in outcome is observed for sales of own goods (0.26) but not for the other outcome variables. Additional results based on alternative specifications, different samples, and different outcome variables are presented in Appendix H.

Figure 4: RD Plot based on quantile spaced number of bins. 2013-2015.

(a) Sales of own goods (log) **(b)** Energy Intensity (log) **(c)** Energy purchases (log)



Notes: Figure shows data-driven regression discontinuity plots using polynomial regression based on quantile-spaced numbers of bins. Optimal number of bins has been selected following Calonico, Cattaneo and Titiunik (2015). Cutoff: 0.05. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI). See reference list for full citation.

5.2.2 Main RD estimates

We now rescale the jump in (reduced form) outcomes by the jump in the (first stage) treatment probability to obtain the second stage estimates around the cutoff. A causal interpretation of the findings relies on the assumption that crossing the 5% eligibility threshold only impacts plants via the probability of receiving compensation and reflects a LATE. Due to the smaller sample size around the threshold, our RD estimates are only based on the ABS sample.[41] For our main

[41]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

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, Cattaneo and Farrell (2020). More details on this procedure can be found in Section H.3 in Appendix H.

Table 6 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 noncompensated plants. The effect on electricity consumption, proxied by energy purchases, is positive and large (24%) but not statistically significant, while we find a negative and non-significant effect on energy intensity. Overall, these findings are in line with our three predictions and DiD results and provide additional evidence that compensation for higher electricity prices particularly boosts production volumes for the compensated. Overall, while pointing towards the same general conclusions, compared to our ATEs, the RD estimates are larger in magnitude, suggesting that as expected, the effects of compensation tend to be larger for more electricity-intensive plants.

5.2.3 Robustness checks

We additionally perform a number of robustness tests to further investigate the validity of our baseline RD findings. Specifically, we produce RD estimates with different assumptions on the functional form where we amend Equations 13, 14, and 15 by additionally accounting for the linear (see Table H.1 in the Appendix) and quadratic (see Table H.2 in the Appendix) distance of each observation from the threshold (cf., Section 4.2). We also examine the robustness of our main estimations with different bandwidth choices and generate a distribution of estimated effects across different estimation window sizes (see Section H.4 in Appendix H).

5.3 Comparing the DiD and RD estimates

The balance of evidence from DiD and RD approaches is summarized in Figure 5. On the whole, results from both strategies indicate that the compensation

due to disclosure concerns.

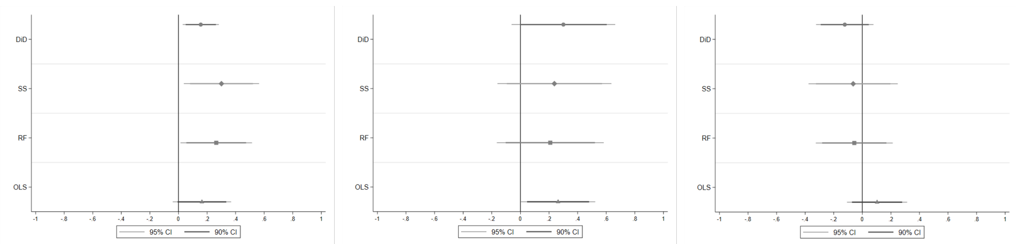
scheme had a positive impact on sales and energy consumption with no detectable significant improvements in energy intensity. However, the magnitude of the treatment effect estimates differs between the two approaches, with the local average treatment effects (LATEs) estimated by the RD approach being larger than the average treatment effects (ATEs) estimated by the DiD approach.

One first reason for this difference in magnitude is that the two strategies focus on different populations with the RD approach focusing on a few plants around the discontinuity threshold in the electricity intensity distribution (see Panel (b) in Figure 2). This means that the RD approach may be interpreted as the treatment effect of the compensation scheme for plants that are most likely to be affected by the policy. In contrast, the DiD approach estimates an average effect of the compensation scheme that is representative for the broader population of manufacturing plants, regardless of their relative position in the electricity intensity distribution.

Another reason for the difference in magnitude may be linked to the identification strategy used in each approach. Our DiD approach combined with IPSW assumes that the weighted treatment and control groups are comparable in all other respects except for 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. This means that the RD approach is better able to control for unobservable factors that may affect the outcomes of interest.

Figure 5: Comparing ATEs and LATEs across ABS outcome variables.

(a) Sales of own goods **(b)** Energy purchases **(c)** Energy intensity



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). See reference list for full citation.

6 Discussion on policy implications

Assessments of the effectiveness of anti-leakage policies typically focus on whether there is 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 7 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 higher sales, calculated based on our DiD estimates of the average treatment effect (Table 2), is in the ballpark of £2 billion per year. The increase in production led to an increase in electricity use, cumulatively amounting to 2.35 TWh (or 2.5% of total annual industrial electricity consumption). The associated annual increase in indirect CO_2 emissions due to this greater electricity use amounts to approximately 1.5 million tonnes CO_2. Given that total emissions are capped under the ETS, this increase in emissions must be offset by emission reductions elsewhere in the system. This implies additional abatement costs for non-compensated plants and sectors, resulting from the compensation treatment.

Table 7: Costs and benefits

	Total
Number of compensated firms	59
Estimated value of increased production	£2,000 million / year
Estimated value of increased GVA	£232 million / year
Estimated forgone reduction in electricity use	2.35TWh / 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 (cf. Section 3.1). We calculate increases in production and indirect emission 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 firm-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 F.1. We obtain cumulative values by multiplying the estimated mean firm-level increases by the total number of compensated firms. *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.

The foregone reductions in indirect carbon emissions are valued at 36 to 377 million £ per year, depending on the CO_2 price assumption used. The upper bound estimate uses current official recommendations on the social cost of carbon (SCC) from the UK government[42] while the lower bound estimate uses the average EUA clearing prices as an alternative market-based proxy for the cost of a tonne of CO_2. Given the ETS cap, these estimates reflect abatement value achieved elsewhere in the system. They offer insights into the potential trade-offs in societal benefits stemming from the implementation of output-based compensation schemes alongside carbon pricing.

The substantial increase in production indicates that the compensation scheme has contributed to shielding energy-intensive firms from higher electricity costs by acting as an implicit production subsidy. When comparing the magnitudes to the direct annual cost of the scheme of around 72 million £ (cf. Section 3.1), each pound of compensation on average has yielded more than one pound in production value (proxied by sales) and GVA. Yet the collateral increase in indirect emissions among compensated energy-intensive firms is sizable, corresponding to around 4.3% (1.3%) of annual industrial (nationwide) emissions from electricity use.

Therefore, in line with empirical studies that find limited evidence of carbon leakage from the EU ETS due to generous free allocation (e.g. Naegele and Zaklan, 2019), our results indicate that the indirect carbon compensation scheme is working, insofar as production displacement and carbon leakage is being discouraged. However, the known downsides of preventing leakage through an output-based compensation have also materialized. Compensation dampens the carbon price signal which is intended to reduce emissions by discouraging the production of CO_2 intensive goods. It creates perverse incentives on the supply side to artificially inflate output, resulting in higher emissions compared to a scenario without compensation.

Interestingly, indirect cost compensation had no statistically significant effect on employment (cf. Table E.3), suggesting that increased electricity prices due to carbon pricing have not led to the displacement of workers in electro-intensive sectors. We also do not find any significant effect on GVA, which is a

[42] Under current guidelines, the UK government recommends using a social costs of carbon (SCC) per tonne of carbon dioxide emitted of £241 (in 2020 £) for policy appraisal and evaluations. See here for further details.

proxy for value added, or on productivity (cf. Figure F.1). The scheme also did not hamper technological improvements in terms of increased energy efficiency in compensated firms vis-á-vis non-compensated firms.

While the ETS cap ensures its environmental effectiveness, our calculations show that indirect CO_2 cost compensation has implications for both economic efficiency and distributional outcomes from carbon pricing. It is likely that compensation to electro-intensive sectors increases the overall compliance cost for meeting mitigation goals. As compensation targets energy-intensive sectors, to meet the overall cap, abatement responsibilities in the EU ETS would shift toward sectors with relatively lower energy intensity – due to the so-called *waterbed effect* (cf. Perino, 2018). This shift represents an adjustment in the distribution of the compliance costs associated with reducing CO_2 emissions. While we do not directly observe sectoral-level marginal abatement costs, sectors with lower energy intensity may find it costlier to implement emissions reduction measures compared to energy-intensive sectors. As the abatement burden shifts to these sectors, the cost-effectiveness of the emissions reduction program may diminish and increase overall compliance costs for the cap-and-trade system (Martin et al., 2014). In the context of an inter-jurisdictional cap-and-trade system, this additionally implies that unilateral compensation schemes have the potential to shift the distribution of abatement responsibilities across countries, effectively redistributing not only carbon abatement costs but also the local health co-benefits associated with reduced emissions of air pollutants from CO_2 combustion (e.g., Cushing et al., 2018; Banzhaf, Ma and Timmins, 2019; Hernandez-Cortes and Meng, 2023).

Additionally, under output-based compensation, theory predicts that producers will not pass on the full CO_2 cost to product prices (Quirion, 2009). Without the full CO_2 cost pass-through, incentives along the production and consumption chain to substitution away from energy-intensive goods are dampened. This suggests the need for supplementary consumption-based measures to encourage mitigation through demand-side substitution. For example, embodied carbon standards, green procurement, and climate excise contribution are discussed in the literature (Grubb et al., 2022).

Finally, using ETS auction revenue to compensate energy-intensive companies for higher carbon costs comes at the trade-off of other climate-related investments or redistributing climate policy costs to the public through alternative revenue recycling schemes (such as lump sum transfers), which could contribute to enhancing the public acceptability of carbon pricing schemes (Baranzini et al., 2017; Douenne and Fabre, 2022). The need to consider the opportunity costs of public funds devoted to compensation schemes becomes more salient with the anticipated substantial future payments driven by the recent surge in carbon prices within the EU ETS. These forthcoming payments are expected to lead to substantial transfers that disproportionately benefit a select few energy-intensive firms and their capital owners, underscoring pivotal equity implications in the distribution of climate policy costs.

7 Conclusion

Governments pursuing ambitious climate policies encounter a complex challenge characterized by a delicate balancing act. On one hand, they must incentivize emission reduction efforts, and on the other, they must mitigate the risk of carbon leakage and competitive disadvantage for domestic industries. This conundrum necessitates the deployment of comprehensive strategies. One approach is to pair carbon pricing with schemes that compensate energy-intensive firms for higher carbon costs or electricity prices. Such policies may help obtain political buy-in from industry and alleviate adverse economic effects. At the same time, a carbon cost containment measure by its nature is likely to delay industrial decarbonization.

While the downsides of output-based free allocation or compensation have been known, perhaps they have been downplayed due to the lack of empirical evidence. We use UK microdata and idiosyncrasies in the eligibility criteria to examine the impact of indirect carbon cost compensation on firms output, electricity use, electricity intensity, and emissions.

We find robust evidence that as intended, compensation limits carbon leakage. It does so by attenuating the carbon price signal and discouraging energy-intensive firms from reducing production, electricity use and emissions. Our back-of-the-envelop calcluations suggest that each pound of compensation yields more than a pound in production value and GVA, but the increase in indirect emissions among compensated energy-intensive firms is also sizable.

In the context of a cap-and-trade scheme, these findings carry important implications for the distribution of mitigation burdens across sectors. Dampening incentives to limit supply from energy-intensive sectors means that to achieve the overall ETS cap, mitigation shifts elsewhere (to other sectors or towards greater emissions intensity improvements) which implies allowance prices and overall costs would rise (Martin et al., 2014).

Compensation for indirect carbon costs as well as free allocation is, however, likely to prevail for some time.[43] Free allocation in the EU ETS is also set to continue until 2028 (European Parliament , 2021) even after the introduction of the Carbon Border Adjustment Mechanism (CBAM) to reduce leakage risk for EU exporters because the proposed CBAM targets imports only. Indeed, free allocation continues to be the default anti-leakage policy across emission trading schemes worldwide, not least because it is hugely advantageous for obtaining political buy-in for carbon pricing from industry (Sato et al., 2022). Our results help make these difficult trade-offs faced by policy makers more explicit, by quantifying the increased production by energy and emission intensive firms due to compensation payments.

[43]The UK has committed to continued compensation to 2025 (Department for Business, Energy & Industrial Strategy, UK, 2022) while international CO_2 price differences prevail, and industrial carbon neutral technologies are not yet widely available. In Europe, several governments have already committed compensation payments until 2030.

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Online supporting material

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

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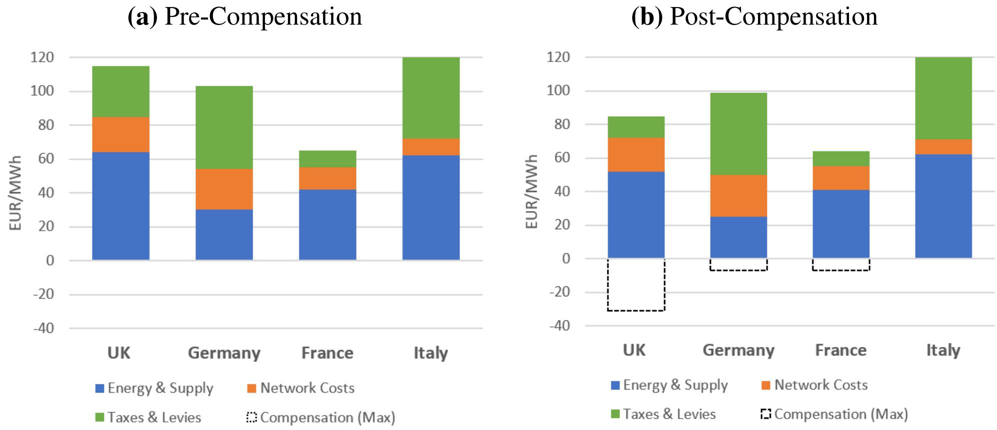
Contents

Appendix A Research context

A.1 Electricity prices in the UK and continental Europe

Electricity prices are kept low in continental Europe, often through discounts or exemptions for industrial users. For example, in Germany, the regulatory approach taken to recover network and policy costs protects electro-intensive industries by recovering costs primarily from domestic and commercial users. By contrast, in the UK these costs are spread relatively evenly across all electricity consumers. In France, the industry has been able to collectively negotiate long-term contracts for lower electricity prices, whereas the UK market has no collectively negotiated contracts and few contracts with a duration beyond a couple of years ahead. Higher levels of interconnection on the continent also allow policy choices to lower industrial electricity prices. For example in Italy, the government facilitated large energy-intensive companies to purchase cheap electricity from neighboring countries in exchange for investments in expanding interconnection capacity. Furthermore, wholesale price differences between the UK and continental Europe are driven by differences in fossil fuel prices, renewable penetration, and the merit order effect (Grubb and Drummond, 2018).

Figure A.1: Industrial electricity prices, pre-and post-compensation for selected EU countries in 2016. EUR/MWh.



Source: Adapted from *Scenario S2* in Grubb and Drummond (2018). Carbon price compensation covers EU ETS costs and costs induced by the UK carbon price floor/tax.

A.2 Eligible 4-digit industries

Table A.1: Eligible industries

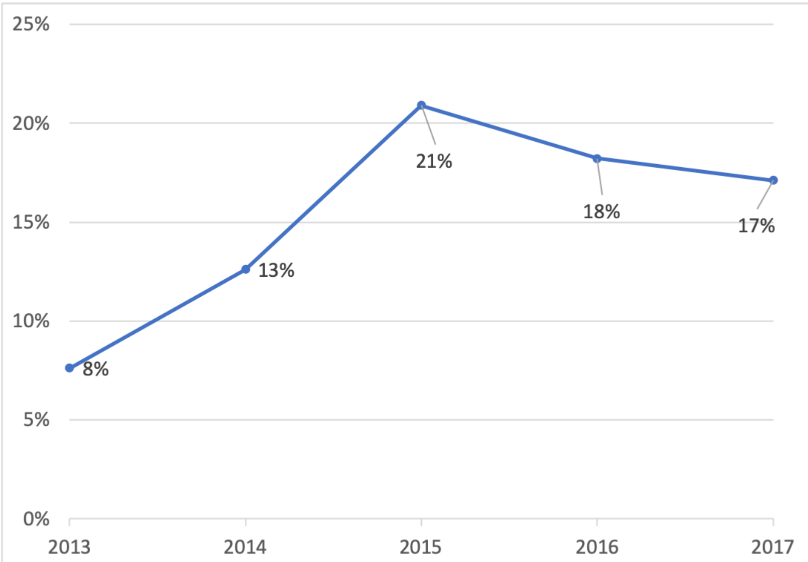
Industry	NACE Rev. 1
Mining of Iron Ore	1310
Mining of chemical and fertiliser minerals	1430
Preparation and spinning of cotton-type fibres	1711
Manufacture of leather clothes	1810
Manufacture of pulp*	2111
Manufacture of paper and paperboard	2112
Manufacture of other inorganic basic chemicals	2413
Manufacture of other organic basic chemicals	2414
Manufacture of fertilisers and nitrogen compounds	2415
Manufacture of plastics in primary forms*	2416
Manufacture of man-made fibres	2470
Manufacture of basic iron and steel and of ferro-alloys	2710
Aluminium production	2742
Lead, zinc and tin production	2743
Copper production	2744

Note: For industries noted by $*$, only a subset of products are eligible for compensation. *Source:* European Commission (2012)

Appendix B Additional descriptive material

B.1 Magnitude of compensation payments

Figure B.1: Compensation payments as a share of electricity prices. 2013-2017.



Notes: Own calculations based on compensation formula and average electricity prices from the UK Department for Business, Energy & Industrial Strategy (2018), Table 3.1.4: Prices of fuels purchased by manufacturing industry in Great Britain.

B.2 Descriptive evidence for outcome variables

Figures B.2 and B.3 provide additional descriptive evidence for our key outcome variables. The raw mean trends exhibit a steady significant decrease in energy intensity for the average plant in the sample, following the introduction of the UK Carbon Price Floor in 2013 as can be seen in Panel (d), Panel (e), and Panel (f) in Figure B.2. Although the raw pre-post mean comparison already provides some exploratory evidence, it does not necessarily capture the causal effect of the regulation, as there are many possible channels that could plausibly explain the observed drop in energy intensity. Additionally, there has been a remarkable decrease in production levels as shown by Panel (a), Panel (b), and Panel (c) in Figure B.2 both in the late 2000s and in 2016. These drops coincide respectively with the global financial crisis in 2008-09 and the EU membership Referendum, that took place in the UK in 2016 (Brexit). Descriptive evidence from Figure B.3 indicates that there has been a tendency to increase electricity consumption and electricity intensity among compensated plants vis-á-vis uncompensated plants (that do not exhibit any trend deviation) following 2013.

Figure B.2: Raw average trends in key outcome variables from ABS over time, by year. 2005-2019.

(a) Sales of own goods (log)

(b) Total output (log)

(c) Turnover (log)

(d) Energy/Sales (log)

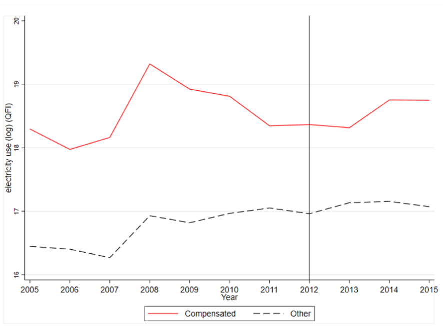
(e) Energy/Output (log)

(f) Energy/Turnover (log)

Notes: Figures plot the average values of key outcomes variable over time by treatment status. Data sources: Annual Business Survey (ABS). The vertical line indicates the year before the carbon price floor was introduced.

Figure B.3: Raw average trends in key outcome variables from QFI over time, by year. 2005-2015.

(a) Electricity use (log)



(b) Electricity/Sales (log)

(c) Electricity/Output (log)

(d) Electricity/Turnover (log)

Notes: Figures plot the average values of key outcomes variable over time over time by treatment status. Data sources: Quarterly Fuels Inquiry (QFI). The vertical line indicates the year before the carbon price floor was introduced.

Appendix C Predicting electricity consumption

Table C.1: Simple correlation between electricity use and energy purchases. 2005–2011

	Electricity (log)	Electricity (log)	Electricity (log)
Energy purch. (log)	0.905***	0.926***	0.920***
	(0.0112)	(0.0135)	(0.0228)
R^2	0.645	0.670	0.669
Obs	3583	2324	805
Sample	QFI, all SIC	eligible SIC, 2 digit	eligible SIC, 4 digit

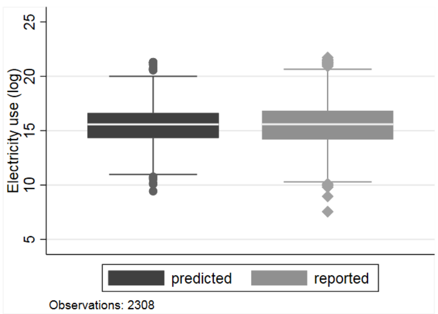
Notes: Table shows correlations between electricity consumption and energy purchases. Both variables are in logs. Electricity use is only available for plants part of the QFI. Data source: ARDx and QFI. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Correlation between electricity use and energy purchases, employment, and turnover. 2005–2011

	Electricity (log)
Energy purch. (log)	0.567***
	(0.0237)
Employment (log)	0.316***
	(0.0400)
Turnover (log)	0.148***
	(0.0323)
Constant	8.030***
	(0.0740)
R^2	0.740
Obs	3582
Sample	QFI, all SIC

Notes: Table shows the correlation between predicted and electricity use and observed energy purchases. Regression include industry dummies at the 4-digit level. Data source: ARDx and QFI. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.1: Comparing predicted and reported electricity use.



Notes: Box plot shows the distribution of predicted electricity use and reported electricity use. Sample is restricted to units with reported electricity use. Data source: ABS and QFI.

Appendix D Using energy purchases to calculate the running variable

As an alternative to using predicted electricity consumption to calculate the second eligibility criteria, we have also tried to use infer the electricity intensity cut-off value, X_0, using total energy purchase in GBP. Total energy purchases and GVA are available from the Annual Business Survey (ABS), hence avoiding the problem om limited coverage of the Quarterly Fuels Inquiry (QFI). Energy intensity is defined as energy purchases divided by GVA, and the sample is restricted to the eligible industries listed in Table A.1. A graphical analysis reveals there is a clear jump in the probability of a plant receiving compensation when the energy intensity is above 12 %. Results are available upon request.

Appendix E Additional results: DiD

E.1 Complementary results based on the period 2010–2019

Table E.1: ATEs of compensation on sales. 2010–2019.

	Sales of own goods				
	2015	2016	2017	2018	2019
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 pre-treatment (Treat)	11.135	10.867	10.831	10.744	10.743
outcome pre-treatment (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

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. We 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 E.2: ATEs of compensation on energy intensity (energy purchases/sales). 2010–2019.

	Energy intensity				
	2015	2016	2017	2018	2019
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 pre-treatment (Treat)	-3.147	-3.087	-2.750	-3.089	-3.090
outcome pre-treatment (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 the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. We drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E.2 DiD Results for employment

Table E.3: ATEs of compensation on employment. 2010–2019.

	Employment				
	2015	2016	2017	2018	2019
Compensation	0.0355	0.0292	0.0122	0.0230	0.0239
	(0.0437)	(0.0451)	(0.0482)	(0.0492)	(0.0503)
Obs	669	923	1106	1337	1492
N compensated	28	40	42	43	43
N other	139	176	184	205	208
Energy intensity 05-11 (Treat)	0.035	0.035	0.035	0.035	0.035
Energy intensity 05-11 (Control)	0.035	0.037	0.037	0.036	0.036
p-value (mean-comparison test)	0.920	0.637	0.660	0.699	0.698
outcome pre-treatment (Treat)	5.401	5.306	5.295	5.246	5.245
outcome pre-treatment (Control)	5.363	5.108	5.130	5.116	5.117
p-value (mean-comparison test)	0.744	0.086	0.151	0.255	0.260

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. We drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E.3 Alternative definition of industry-year fixed effects

Table E.4: ATEs of compensation on sales *within 2-digit industries*. 2010–2019.

	Sales of own goods (effects at 2-digit level)				
	2015	2016	2017	2018	2019
Compensation	0.146***	0.185**	0.193***	0.195***	0.190***
	(0.0536)	(0.0720)	(0.0741)	(0.0707)	(0.0724)
Obs	925	1189	1409	1709	1881
N compensated	31	41	44	45	45
N other	185	216	225	250	252
Energy intensity 05-11 (Treat)	0.045	0.039	0.040	0.038	0.038
Energy intensity 05-11 (Control)	0.035	0.035	0.035	0.034	0.034
p-value (mean-comparison test)	0.001	0.170	0.155	0.233	0.239
outcome pre-treatment (Treat)	9.538	10.729	10.718	10.669	10.667
outcome pre-treatment (Control)	11.003	10.876	10.862	10.810	10.813
p-value (mean-comparison test)	0.000	0.266	0.273	0.269	0.254

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 2-digit SIC code level and are weighted by the inverse propensity score. We 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 E.5: ATEs of compensation on energy intensity (energy purchases/sales) *within 2-digit industries*. 2010–2019.

	Energy intensity (effects at 2-digit level)				
	2015	2016	2017	2018	2019
Compensation	-0.0906	0.0808	0.130	0.114	0.108
	(0.0622)	(0.0961)	(0.104)	(0.0806)	(0.0814)
Obs	1097	1447	1778	2080	2304
N compensated	39	49	54	56	56
N other	261	319	342	356	365
Energy intensity 05-11 (Treat)	0.035	0.039	0.039	0.034	0.034
Energy intensity 05-11 (Control)	0.037	0.037	0.037	0.037	0.037
p-value (mean-comparison test)	0.576	0.596	0.528	0.260	0.278
outcome pre-treatment (Treat)	-2.999	-2.953	-2.937	-3.133	-3.133
outcome pre-treatment (Control)	-2.973	-3.026	-3.013	-3.029	-3.038
p-value (mean-comparison test)	0.814	0.437	0.410	0.236	0.274

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 2-digit SIC code level and are weighted by the inverse propensity score. We drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E.4 Alternative sample trimming

Table E.6: ATEs of compensation on sales *with different trimming*. 2010–2019.

	Sales of own goods (Intensity >0.005)				
	2015	2016	2017	2018	2019
Compensation	0.136**	0.158**	0.130**	0.114*	0.115*
	(0.0653)	(0.0665)	(0.0651)	(0.0643)	(0.0644)
Obs	914	1252	1552	1875	2081
N compensated	33	50	62	63	63
N other	199	249	277	309	315
Energy intensity 05-11 (Treat)	0.031	0.030	0.030	0.031	0.031
Energy intensity 05-11 (Control)	0.026	0.027	0.027	0.026	0.026
p-value (mean-comparison test)	0.123	0.255	0.189	0.070	0.062
outcome pre-treatment (Treat)	10.441	10.276	10.227	10.208	10.194
outcome pre-treatment (Control)	10.673	10.474	10.449	10.396	10.386
p-value (mean-comparison test)	0.233	0.254	0.204	0.277	0.270

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. We drop plants with an electricity intensity based on Eq. (9) below 0.005. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E.7: ATEs of compensation on energy intensity (energy purchases/sales) *with different trimming*. 2010–2019.

	Energy intensity (Intensity >0.005)				
	2015	2016	2017	2018	2019
Compensation	-0.144	-0.0380	0.0105	0.0401	0.0322
	(0.0908)	(0.0974)	(0.102)	(0.0962)	(0.0933)
Obs	997	1427	1789	2140	2371
N compensated	34	56	71	72	72
N other	234	299	331	363	371
Energy intensity 05-11 (Treat)	0.028	0.028	0.028	0.028	0.028
Energy intensity 05-11 (Control)	0.029	0.028	0.030	0.029	0.028
p-value (mean-comparison test)	0.785	0.920	0.432	0.665	0.757
outcome pre-treatment (Treat)	-3.249	-3.094	-3.108	-3.108	-3.118
outcome pre-treatment (Control)	-3.242	-3.254	-3.238	-3.252	-3.275
p-value (mean-comparison test)	0.969	0.124	0.198	0.152	0.117

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. We drop plants with an electricity intensity based on Eq. (9) below 0.005. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E.8: ATEs of compensation on QFi variables *with different trimming*. 2010–2015.

	Electricity use	**Electricity intensity**	**Carbon Emissions**
Compensation	0.240***	0.185	0.243***
	(0.0831)	(0.186)	(0.0818)
Obs	472	712	490
N compensated	15	16	15
N other	76	128	79
Industry effects digit	1	1	1
Industry effects-year FE	Yes	Yes	Yes
EUTL-year-Industry effects	No	No	No
Energy intensity 05-11 (Treat)	0.036	0.033	0.036
Energy intensity 05-11 (Control)	0.034	0.031	0.032
p-value (mean-comparison test)	0.700	0.546	0.329
outcome pre-treatment (Treat)	17.299	5.901	23.512
outcome pre-treatment (Control)	17.360	5.960	23.562
p-value (mean-comparison test)	0.715	0.692	0.796

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 1-digit level and are weighted by the inverse propensity score. We drop plants with an electricity intensity based on Eq. (9) below 0.005. Data sources: Quarterly Fuels Inquiry (QFI).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E.5 Alternative proxies for production and electricity intensity

Table E.9: ATEs of compensation on total output. 2010–2019.

	Total output				
	2015	2016	2017	2018	2019
Compensation	0.147*	0.185**	0.156*	0.150*	0.157*
	(0.0798)	(0.0868)	(0.0895)	(0.0884)	(0.0902)
Obs	528	710	862	1105	1230
N compensated	26	37	40	41	41
N other	99	125	134	162	165
Energy intensity 05-11 (Treat)	0.035	0.034	0.035	0.034	0.034
Energy intensity 05-11 (Control)	0.036	0.037	0.037	0.037	0.037
p-value (mean-comparison test)	0.586	0.338	0.403	0.350	0.351
outcome pre-treatment (Treat)	10.960	10.752	10.721	10.651	10.650
outcome pre-treatment (Control)	11.116	10.873	10.839	10.778	10.778
p-value (mean-comparison test)	0.307	0.454	0.472	0.422	0.419

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. We 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 E.10: ATEs of compensation on turnover. 2010–2019.

	Turnover				
	2015	2016	2017	2018	2019
Compensation	0.141*	0.158**	0.159**	0.192**	0.195**
	(0.0721)	(0.0761)	(0.0778)	(0.0796)	(0.0803)
Obs	550	755	907	1151	1270
N compensated	25	37	39	41	41
N other	105	133	141	168	170
Energy intensity 05-11 (Treat)	0.035	0.035	0.035	0.034	0.034
Energy intensity 05-11 (Control)	0.036	0.036	0.036	0.035	0.035
p-value (mean-comparison test)	0.725	0.603	0.681	0.722	0.722
outcome pre-treatment (Treat)	11.120	10.936	10.906	10.831	10.830
outcome pre-treatment (Control)	11.130	10.946	10.915	10.850	10.859
p-value (mean-comparison test)	0.954	0.954	0.957	0.906	0.858

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. We 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 E.11: ATEs of compensation on energy/output. 2010–2019.

	Energy/Output				
	2015	2016	2017	2018	2019
Compensation	-0.0615	0.0728	0.0601	0.00245	0.00502
	(0.128)	(0.141)	(0.141)	(0.132)	(0.125)
Obs	648	896	1145	1371	1524
N compensated	24	39	43	45	45
N other	150	179	201	217	223
Energy intensity 05-11 (Treat)	0.036	0.035	0.035	0.035	0.035
Energy intensity 05-11 (Control)	0.035	0.036	0.038	0.037	0.036
p-value (mean-comparison test)	0.982	0.858	0.341	0.463	0.754
outcome pre-treatment (Treat)	-3.062	-2.966	-2.976	-2.952	-2.949
outcome pre-treatment (Control)	-3.042	-3.033	-3.051	-3.058	-3.078
p-value (mean-comparison test)	0.907	0.555	0.478	0.312	0.220

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. We 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 E.12: ATEs of compensation on energy/turnover. 2010–2019.

	Energy/Turnover				
	2015	2016	2017	2018	2019
Compensation	-0.0445	0.0134	0.0476	0.0319	0.00431
	(0.101)	(0.116)	(0.146)	(0.138)	(0.135)
Obs	705	983	1232	1455	1609
N compensated	27	39	44	45	46
N other	162	206	224	238	241
Industry effects digit	3	3	3	3	3
Energy intensity 05-11 (Treat)	0.030	0.029	0.036	0.036	0.036
Energy intensity 05-11 (Control)	0.035	0.036	0.033	0.032	0.032
p-value (mean-comparison test)	0.075	0.009	0.145	0.113	0.042
outcome pre-treatment (Treat)	-3.300	-3.254	-3.019	-3.029	-2.998
outcome pre-treatment (Control)	-3.123	-3.128	-3.135	-3.170	-3.177
p-value (mean-comparison test)	0.181	0.221	0.268	0.180	0.090
outcome pre-treatment (Control)	-3.042	-3.033	-3.051	-3.058	-3.078
p-value (mean-comparison test)	0.907	0.555	0.478	0.312	0.220

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. We drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E.6 Alternative calculations of p-scores

In our main results, we estimate propensity score weights based on the period 2005-2011. These years correspond to the time frame used by the UK Government to calculate the electricity cost share, which again determines whether a firm passes the 5% filter test (cf. Section 3.1).

In Table E.13, we show that our main results are robust to the use of an alternative time horizon to compute our p-scores ranging from 2010 to 2012, which mitigate potential concerns about how the global financial crisis might affect the p-score estimation.

Table E.13: ATEs of compensation on sales *using different p-scores*. 2010–2015.

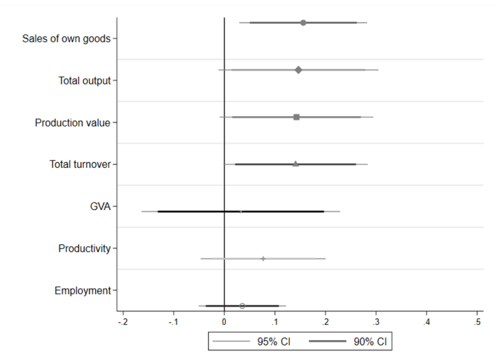
	ABS			QFI	
	Sales	**Energy intensity**	**Electricity use**	**Indirect Emissions**	**Electricity intensity**
Compensation	0.121*	0.0762	0.166*	0.177*	0.118
	(0.0646)	(0.0839)	(0.1000)	(0.0981)	(0.173)
Obs	474	629	489	464	630
N compensated	20	24	13	13	16
N other	84	137	84	78	113
Industry effects digit	3	3	1	1	1
Industry effects-year FE	Yes	Yes	Yes	Yes	Yes
Energy intensity 05-11 (Treat)	0.034	0.031	0.043	0.042	0.035
Energy intensity 05-11 (Control)	0.038	0.036	0.034	0.034	0.033
p-value (mean-comparison test)	0.311	0.100	0.091	0.092	0.535
outcome pre-treatment (Treat)	11.213	-3.099	17.200	23.389	5.999
outcome pre-treatment (Control)	11.218	-3.076	17.329	23.598	6.032
p-value (mean-comparison test)	0.977	0.844	0.468	0.240	0.834

Notes: Table shows the coefficient β_1 estimated from the DiD equation. Dependent variables are in logs. Standard errors are clustered at the firm level. All regressions include year x industry fixed effects at the 3-digit SIC code level (ABS variables) or 1-digit level (QFI variables) and are weighted by the inverse propensity score. We drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

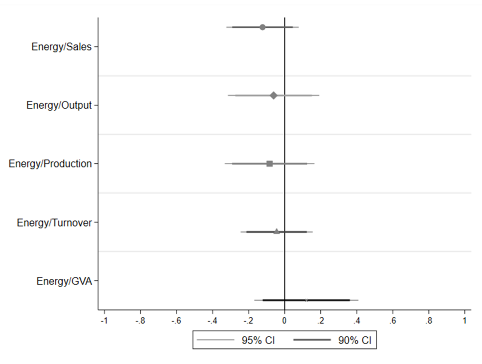
Appendix F Graphical comparisons of DiD estimates

Figure F.1: Comparison of DiD estimates (1)

(a) Production and employment variables



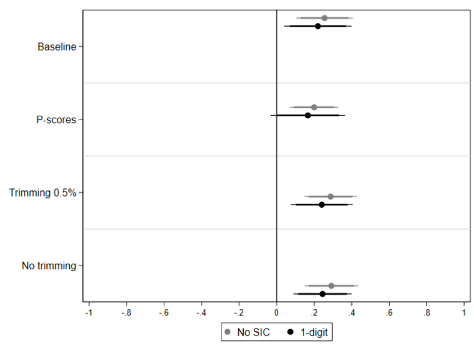
(b) Energy intensity proxies



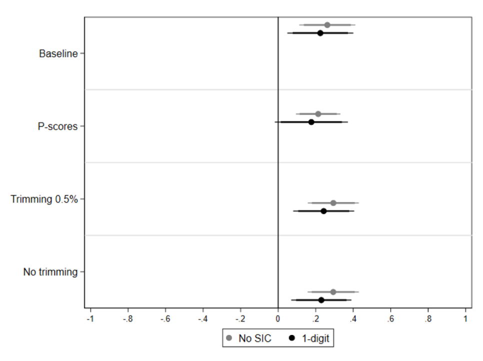
Notes: All regressions include year x industry fixed effects at the 3-digit SIC code level (ABS variables) or 1-digit level (QFI variables) and are weighted by the inverse propensity score. The post-treatment period is 2013–2015. We drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).

Figure F.2: Comparison of DiD estimates (2)

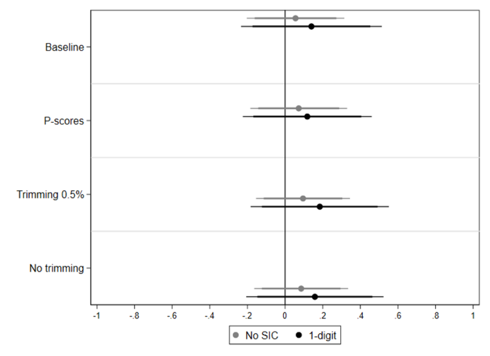
(a) Electricity use



(b) CO_2 emissions



(c) Electricity intensity



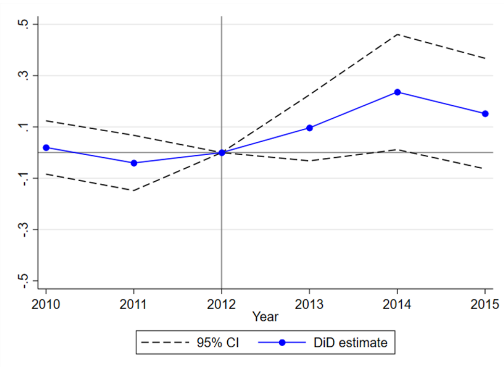
Notes: All regressions include year x industry fixed effects at the 3-digit SIC code level (ABS variables) or 1-digit level (QFI variables) and are weighted by the inverse propensity score. The post-treatment period is 2013–2015. We drop plants with an electricity intensity based on Eq. (9) below 0.01 (*Baseline*) and 0.005 (*Trimming 0.5%*) as well as re-estimate the baseline model with no sample trimming. *P-scores* refers to the alternative specification leveraging the pre-period 2010-2012 to compute p-scores (cf. Section E.6). Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).

Appendix G Dynamic difference-in-differences results

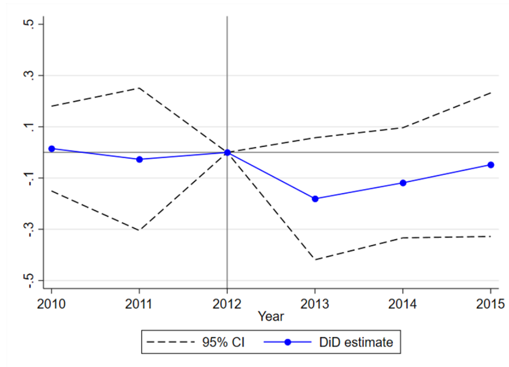
Figure G.1 below presents the dynamic version of the DiD approach, which plots the annual DiD coefficients estimated from Equation 12 and shows how treatment effects unfold over time for two key ABS outcome variables: (a) sales of own goods and (b) energy intensity (scaled by sales). The figures related to alternative outcome variables are subject to disclosure restrictions, and access to them is available exclusively through the ONS secure lab. For additional information on access requirements, please refer to the details provided here.

Figure G.1: Treatment effects of compensation, by year. 2010-2015.

(a) Sales of own goods (log)



(b) Energy intensity (log)



Notes: Figures plot the coefficients $\sum_{m=0}^{M} \beta_{-m}$ and $\sum_{k=1}^{K} \beta_{+k}$ estimated from equation 12. 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).

Appendix H Robustness Checks: Fuzzy RDD

H.1 Alternative functional specifications

Table H.1: LATEs of compensation. Fuzzy RDD controlling for linear distance from the cut-off.

	Sales of own goods	**Energy purchases**	**Energy Intensity**
Panel A: First Stage	0.899***	0.924***	0.924***
	(0.0891)	(0.0848)	(0.0848)
Panel B: Reduced Form	0.254**	0.170	-0.0725
	(0.125)	(0.220)	(0.150)
Panel C: Second Stage	0.282**	0.184	-0.0784
	(0.129)	(0.227)	(0.167)
Panel D: OLS	0.160	0.274*	0.124
	(0.107)	(0.139)	(0.115)
Observations	253	252	249
N Compensated	20	20	20
N Other	49	48	47
F statistics	101.69	118.73	118.63
Functional form	Linear	Linear	Linear

Notes: Tables show the coefficients estimated from the first stage, reduced form, and second stage of the fuzzy regression discontinuity design. Dependent variables are given by the table headings and are measured in logs.Each stage of the estimation includes include $post_t \times \mathbb{1}\{elig_j = 1\}$ and $post_t \times \mathbb{1}\{c_i \geq c_0\}$ and firm-level fixed effects (see Section 4.2 for more details). Standard errors are clustered at the firm level. The sample estimation period is 2010-2015. Cutoff value: 0.05. Bandwidth: cutoff value +/-0.007. Bandwidth refers to the range of electricity intensity values (electricity use * carbon cost / GVA) used to restrict the sample. Data source: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table H.2: LATEs of compensation. Fuzzy RDD controlling for quadratic distance from the cut-off.

	Sales of own goods	**Energy purchases**	**Energy Intensity**
Panel A: First Stage	0.923***	0.917***	0.918***
	(0.103)	(0.104)	(0.104)
Panel B: Reduced Form	0.273*	0.0591	-0.211*
	(0.140)	(0.174)	(0.124)
Panel C: Second Stage	0.296**	0.0644	-0.230
	(0.138)	(0.186)	(0.144)
Panel D: OLS	0.150	0.254*	0.107
	(0.110)	(0.137)	(0.131)
Observations	253	252	249
N Compensated	20	20	20
N Other	49	48	47
F statistics	79.91	77.79	77.87
Functional form	Polynomial	Polynomial	Polynomial

Notes: Tables show the coefficients estimated from the first stage, reduced form, and second stage of the fuzzy regression discontinuity design. Dependent variables are given by the table headings and are measured in logs.Each stage of the estimation includes include $post_t \times \mathbb{1}\{elig_j = 1\}$ and $post_t \times \mathbb{1}\{c_i \geq c_0\}$ and firm-level fixed effects (see Section 4.2 for more details). Standard errors are clustered at the firm level. The sample estimation period is 2010-2015. Cutoff value: 0.05. Bandwidth: cutoff value +/-0.007. Bandwidth refers to the range of electricity intensity values (electricity use * carbon cost / GVA) used to restrict the sample. Data source: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

H.2 RD Results for employment

Table H.3: LATEs of compensation on employment with different functional forms. 2010–2015. Fuzzy RDD.

	Employment		
Panel A: First stage	0.879***	0.899***	0.923***
	(0.101)	(0.0891)	(0.103)
Panel B: Reduced form	0.0612	0.0693	0.0245
	(0.125)	(0.0837)	(0.0858)
Panel C: Second stage	0.0696	0.0771	0.0265
	(0.0890)	(0.0893)	(0.0916)
Panel D: OLS	0.0371	0.0290	0.0228
	(0.0557)	(0.0569)	(0.0522)
Observations	256	256	256
N Compensated	20	20	20
N Other	50	50	50
F statistics	75.51	101.77	79.82
Functional form	Bins	Linear	Quadratic

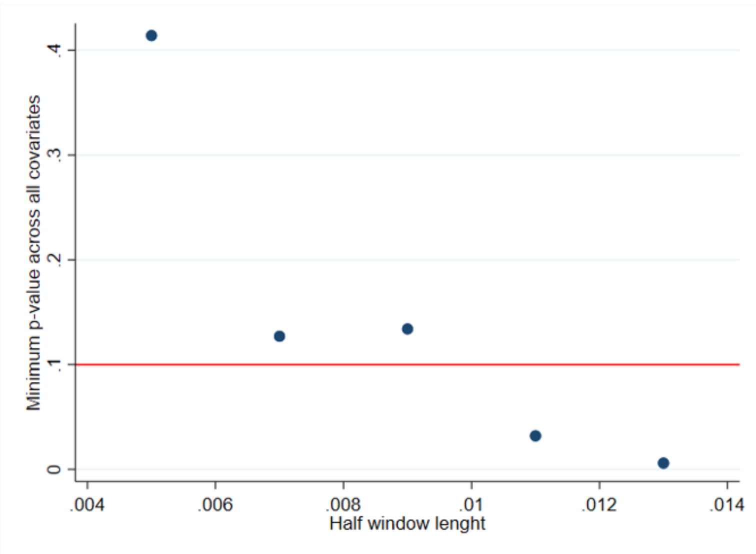
Notes: Tables show the coefficients estimated from the first stage, reduced form, and second stage of the fuzzy regression discontinuity design. Dependent variables are given by the table headings and are measured in logs.Each stage of the estimation includes include $post_t \times \mathbb{1}\{elig_j = 1\}$ and $post_t \times \mathbb{1}\{c_i \geq c_0\}$ and firm-level fixed effects (see Section 4.2 for more details). Standard errors are clustered at the firm level. The sample estimation period is 2010-2015. Cutoff value: 0.05. Bandwidth: cutoff value +/-0.007. Bandwidth refers to the range of electricity intensity values (electricity use * carbon cost / GVA) used to restrict the sample. Data source: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

H.3 RD bandwidth selection procedure

We implement the window-selection procedure based on balance tests for RD designs under local randomization introduced by Calonico, Cattaneo and Farrell (2020). Specifically, this procedure involves constructing a sequence of nested windows around the RD cutoff and undertaking binomial tests for the running variable and hypothesis tests for a set of covariates. Then, the selected window is the largest window around the cutoff such that the minimum p-value of the balance test is larger than 0.10. To produce Figure H.1, we select proxies for production levels and energy intensity (i.e., sales of own goods and electricity scaled by sales as a measure of intensity, respectively) as covariates and focus on the pre-treatment period to select the largest inference window where local randomization is assumed to hold where we can empirically show that the distribution of observed covariates does not change discontinuously at the threshold to a significant extent.

Here, we report the selection of covariates that produced the most conservative (or lowest) p-values in our runs and opt for an optimal window of +/- 0.007 from the cutoff. As the choice of covariates bears an arbitrary component, we run the same procedure outlined above with a different selection of covariates, and the resulting p-value for the window length that we selected (+/-0.007) ranges from around 0.14 (as shown below) to around 0.5 (when we include other production values such as total output, turnover, and production value). We then test the extent to which our results are affected by different bandwidth choices in the following section. Nevertheless, due to the limited sample size around the threshold, we face a trade-off between moving closer to the threshold where the assumption of local randomization becomes increasingly more plausible and model precision. See Section 4.2 for more details on our RD setting.

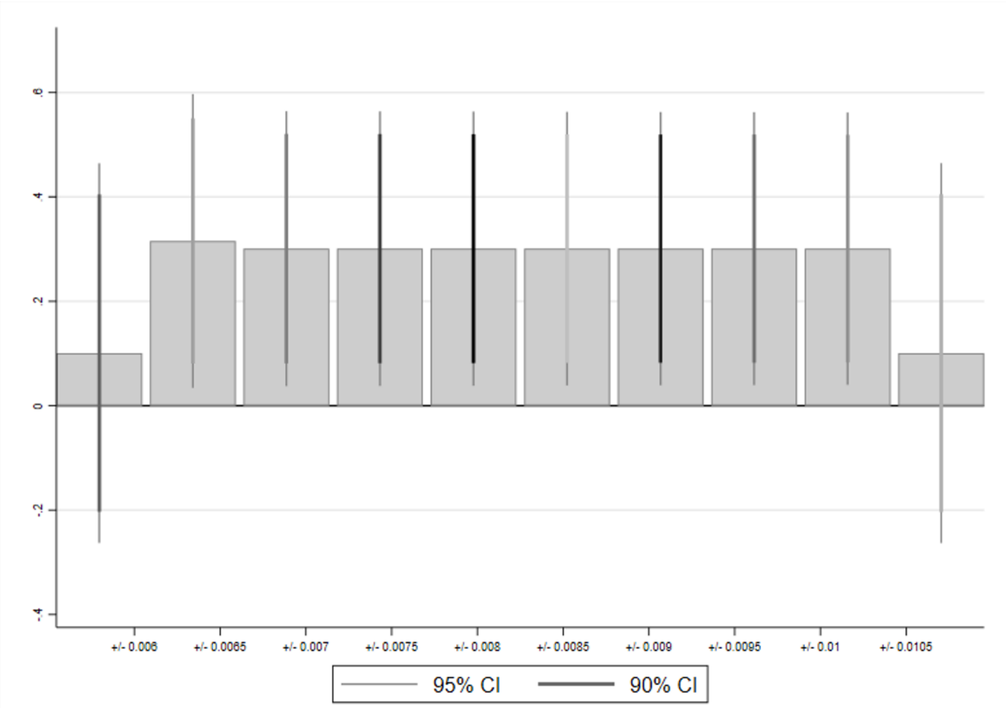
Figure H.1: RD bandwidth selection procedure



Notes: Figure plots the minimum p-value of a balance test following Calonico, Cattaneo and Farrell (2020).

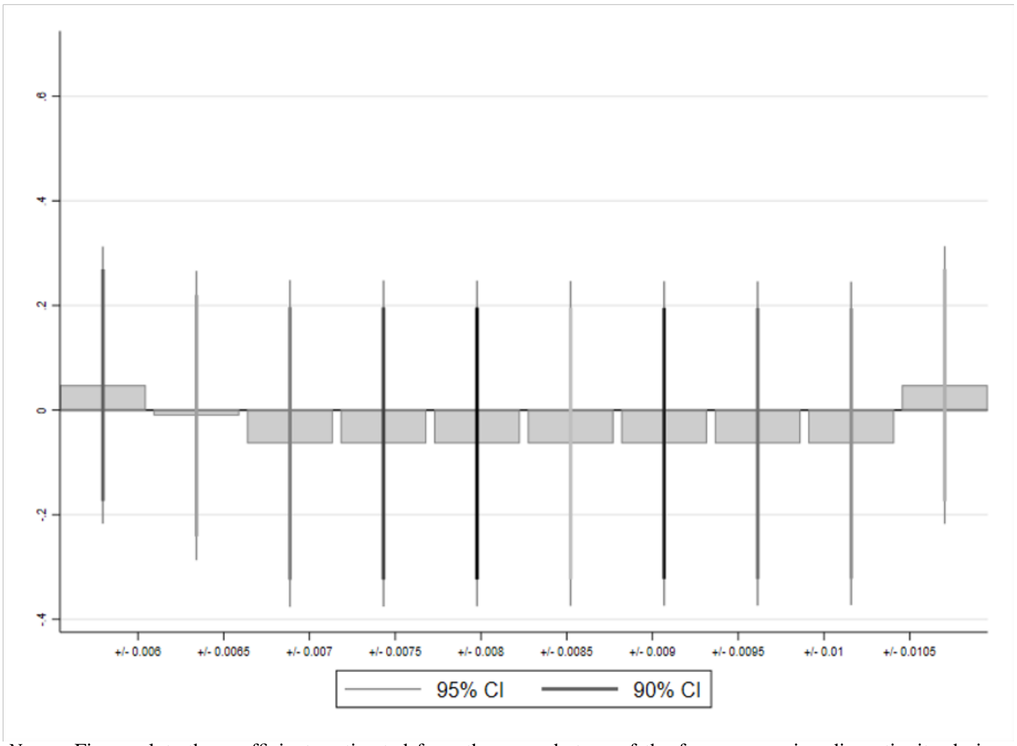
H.4 Alternative bandwidths

Figure H.2: Comparing LATEs on sales across different bandwidths. 2010-2015.



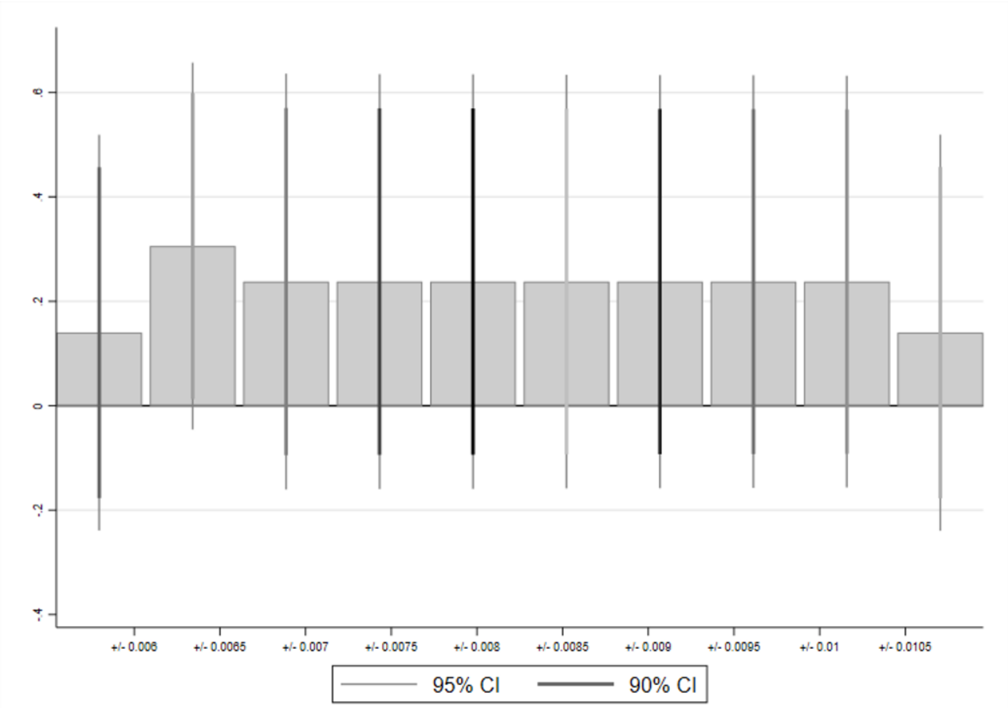
Notes: Figure plots the coefficients estimated from the second stage of the fuzzy regression discontinuity design. Dependent variables are indicated in the caption and are measured in logs. Each stage of the estimation includes include $post_t \times \mathbb{1}\{elig_j = 1\}$ and $post_t \times \mathbb{1}\{c_i \geq c_0\}$ and firm-level fixed effects (see Section 4.2 for more details). Standard errors are clustered at the firm level. The sample estimation period is 2010-2015. Cutoff value: 0.05. Bandwidth: cutoff values range from +/-0.006 to +/-0.0105 with a 0.0005 step-wise increase in the estimation window from left to right. Bandwidth refers to the range of electricity intensity values (electricity use * carbon cost / GVA) used to restrict the sample. Data source: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).

Figure H.3: Comparing LATEs on energy intensity (scaled by sales) across different bandwidths. 2010-2015.



Notes: Figure plots the coefficients estimated from the second stage of the fuzzy regression discontinuity design. Dependent variables are indicated in the caption and are measured in logs. Each stage of the estimation includes include $post_t \times \mathbb{1}\{elig_j = 1\}$ and $post_t \times \mathbb{1}\{c_i \geq c_0\}$ and firm-level fixed effects (see Section 4.2 for more details). Standard errors are clustered at the firm level. The sample estimation period is 2010-2015. Cutoff value: 0.05. Bandwidth: cutoff values range from +/-0.006 to +/-0.0105 with a 0.0005 step-wise increase in the estimation window from left to right. Bandwidth refers to the range of electricity intensity values (electricity use * carbon cost / GVA) used to restrict the sample. Data source: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).

Figure H.4: Comparing LATEs on energy purchases across different bandwidths. 2010-2015.



Notes: Figure plots the coefficients estimated from the second stage of the fuzzy regression discontinuity design. Dependent variables are indicated in the caption and are measured in logs. Each stage of the estimation includes include $post_t \times \mathbb{1}\{elig_j = 1\}$ and $post_t \times \mathbb{1}\{c_i \geq c_0\}$ and firm-level fixed effects (see Section 4.2 for more details). Standard errors are clustered at the firm level. The sample estimation period is 2010-2015. Cutoff value: 0.05. Bandwidth: cutoff values range from +/-0.006 to +/-0.0105 with a 0.0005 step-wise increase in the estimation window from left to right. Bandwidth refers to the range of electricity intensity values (electricity use * carbon cost / GVA) used to restrict the sample. Data source: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).