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The impact of a carbon tax on manufacturing: Evidence from microdata

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ABSTRACT

We estimate the impact of a carbon tax on manufacturing plants using panel data from the UK production census. Our identification strategy builds on the comparison of outcomes between plants subject to the full tax and plants that paid only 20% of the tax. Exploiting exogenous variation in eligibility for the tax discount, we find that the carbon tax had a strong negative impact on energy intensity and electricity use. No statistically significant impacts are found for employment, revenue or plant exit.

1. Introduction

The rise of climate policy on government agendas around the world has stirred a renewed interest in the optimal design of large-scale regulation of environmental externalities. Climate change – the "ultimate commons problem" (Stavins, 2011) – is caused by anthropogenic emissions of greenhouse gases (GHG) such as carbon dioxide (CO_2) and is expected to have severe ecological and economic consequences (IPCC, 2007). Mitigating climate change will require substantial abatement of GHG emissions from all core economic sectors (Pacala and Socolow, 2004). The choice of appropriate policy instruments for each of these sectors is essential for minimizing the overall economic costs of mitigation with given technologies (static efficiency), and for stimulating technological innovations that will further reduce mitigation costs in the future (dynamic efficiency). This paper evaluates the performance of one such instrument, a tax designed to curb industrial CO_2 emissions, in a panel of manufacturing plants.

Manufacturing is a major contributor to GHG emissions around the world.[1] Since most manufactured goods are tradable, there is a risk that regulated firms will lose international competitiveness, shed part of their labor force or even exit. These concerns have been fueling vehement opposition towards regulation and left their mark on the design of the policies implemented so far. Command-and-control policies have long been the predominant form of environmental regulation in the manufacturing sector, and their impacts have been studied extensively in the context of air pollution.[2] On theoretical grounds, economists have favored market-based instruments such as taxes and tradable permit schemes because they are more efficient in both the static and dynamic senses (e.g. Montgomery, 1972; Milliman and Prince, 1989; Tietenberg,

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[1] Together with primary industry, the manufacturing sector accounts for almost 40% of GHG emissions worldwide (IEA, 2010). Total carbon emissions from the business sector in 2000 were estimated at 60.3 MtC (NAO, 2007).

[2] The literature has examined the effects of air quality regulation on air pollution (Henderson, 1996; Greenstone, 2004), industrial activity (Becker and Henderson, 2000; Greenstone, 2002), plant births and deaths (Henderson, 1996; Levinson, 1996; List et al., 2003), plant-level productivity (Berman and Bui, 2001; Gray and Shadbegian, 2003), foreign direct investment (Hanna, 2010) and market structure (Ryan, 2012).

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1990). However, empirical evidence on the impacts of market-based environmental regulation on manufacturing is scarce, especially when it comes to carbon emissions.[3] For example, the European Union Emissions Trading Scheme (EU ETS), the largest cap-and-trade system for carbon emissions worldwide, is overdue for a microeconometric evaluation (Martin et al., 2013b). While carbon taxes have been implemented in various EU countries, their rigorous evaluation has proven difficult, be it because of the lack of suitable microdata or because of the lack of a compelling identification strategy.[4]

This paper fills the void by analyzing the Climate Change Levy (CCL) package — the single most important climate change policy that the UK government has unilaterally imposed on the business sector so far (HM Government, 2006). The package consists of a carbon tax – the CCL – and a scheme of voluntary agreements available to plants in selected energy intensive industries. Upon joining a Climate Change Agreement (CCA), a plant adopts a specific target for energy consumption or carbon emissions in exchange for a highly discounted tax liability under the CCL. While the CCL package is still in place today, our analysis focuses on the first three years following its introduction in 2001, thereby avoiding overlap with the EU ETS. During the period of analysis, the CCL added 15% to the energy bill of a typical UK business (NAO, 2007) and the discount granted under a CCA amounted to 80% of the tax rate.

Given its scope and institutional context, the CCL package provides a unique opportunity to study the effects of a carbon tax in an industrialized economy. We use longitudinal data on manufacturing plants to estimate the impact of the CCL on energy use, emissions and economic performance. Our identification strategy is to compare changes in outcomes between fully-taxed CCL plants and CCA plants. A naïve difference-in-differences (DiD) estimator would likely be biased because the plants that were eligible for CCA participation could self-select their tax regime. However, plants were only eligible if they emitted pollutants subject to environmental regulation pre-dating the CCL. The variation in eligibility across plants can hence be exploited to instrument for the tax rate. We implement this idea in an IV framework where the reduced form is a DiD regression of plant outcomes on eligibility. For this approach to be consistent, it must be true that differences between eligible and non-eligible plants are not systematically related to changes in outcome variables over the treatment period. While this assumption is not testable, we show that there are no significant trend differences between eligible and non-eligible firms in the pre-treatment period. In addition, we exploit the panel structure of the dataset to control for pre-trends directly in the regression.

Firms in the control group were not only entitled to a tax discount, but they also faced a reduction target for energy consumption or carbon emissions. Although these targets could have placed binding constraints on the plant's production choices, the fact that massive over compliance occurred right from the start suggests otherwise. In fact, a large degree of flexibility was built into both the target negotiation process and the compliance review. If targets were nonetheless stringent, then our estimate represents a lower bound on the full price effect of the tax differential between the two groups of plants.

With this approach, we find robust evidence that the CCL had a strong negative impact on energy intensity, particularly at larger and more energy intensive plants. An analysis of fuel choices at the plant level reveals that this effect is mainly driven by a reduction in electricity use and translates into a negative impact on CO_2 emissions. In contrast, we do not find any statistically significant impacts of the tax on employment, revenue (gross output) or total factor productivity (TFP). In addition, we examine extensive-margin adjustments and find no evidence that the CCL accelerated plant exit. While the regression-based test we use does not have much power to detect small negative impacts on these outcomes, our results do not substantiate worries about devastating effects of the CCL on the competitiveness of UK manufacturing, which gave way to a costly exemption scheme.[5]

Over the past two decades, carbon taxes and their effects on industrial competitiveness have been a matter of political debate in many industrialized countries. By conducting the first ex-post analysis of the causal impact of such a tax on manufacturing, our study provides much-needed empirical evidence on the impacts of large-scale regulation aimed at pricing pollution. It does so in the context of climate change – an area where regulatory stringency is bound to increase in the near future – and with a focus on manufacturing, the principal engine of growth in the emerging economies and still a cornerstone of employment in post-industrial economies.

The remainder of the paper is structured as follows. Section 2 describes the CCL package in detail and reviews previous research on the tax. Section 3 describes the research design and econometric framework. Section 4 describes the data sources and summarizes the dataset used for the analysis. Section 5 reports the main results and presents several robustness checks. Section 6 examines heterogeneous impacts, aggregate effects and estimates the impact of the CCL on exit. Section 7 concludes.

2. Background

2.1. The Climate Change Levy and Climate Change Agreements

Since the 1990s the UK has adopted a series of increasingly ambitious targets for climate policy. In addition to a 12.5% reduction of GHG emissions from 1990 levels to be achieved under the Kyoto Protocol, the Blair administration promised to reduce CO_2 emissions by 19% by 2010 and by 60% by 2050.[6] When the CCL package was implemented in 2001, it constituted the single-most important policy aimed at achieving these goals.[7]

The CCL is a per unit tax payable at the time of supply to industrial and commercial users of energy. It was first announced in March 1999 and came into effect in April 2001. Taxed fuels include coal, electricity, natural gas, and non-transport liquefied petroleum gas (LPG). For each fuel type subject to the CCL, Table 1 displays the tax rates per kilowatt hour (kWh) equivalent, the average energy price in Pound Sterling paid by manufacturing plants in 2001 and the implicit carbon tax. Energy tax rates vary substantially across fuel types, ranging from 6.1% on coal to 16.5% on natural gas.[8]

While the tax establishes a meaningful price incentive for energy conservation overall, it is immediately seen that carbon contained in gas and electricity is taxed at almost twice the rate as carbon contained in coal.[9] Other fuel types were tax-exempt precisely because of their low carbon content, such as electricity generated from renewable sources and from combined heat and power. Hence, rather than a

[3] One reason for this is that most existing cap-and-trade programs regulate emissions from electric generators only – such as the much-studied SO_2 trading implemented under the US Acid Rain Program (see e.g. Ellerman et al., 2000) – and do not cover manufacturing emissions in significant ways. The RECLAIM program for NO_X emissions in California is an exception (Fowlie et al., 2012).

[4] See Bjorner and Jensen (2002) for an early microeconometric evaluation of industrial energy taxes in Denmark.

[5] A study commissioned by the UK government estimates the annual tax revenue lost due to the tax discount at £366 million or 44% of the actual CCL revenue in 2003 (Cambridge Econometrics, 2005).

[6] With the passing into law of the Climate Change Bill in November 2008, the commitment to reduce GHG emissions in the UK by at least 80% until 2050 has become legally binding.

[7] For example, the revised UK Climate Change Programme (HM Government, 2006) designated the CCL package as the top contributor of carbon savings (6.6 MtC towards an overall reduction goal of 20.8 MtC by 2010). As we explain in Section 2.3 below, such projections are highly sensitive to the assumed trajectory of baseline emissions.

[8] Tax rates were constant from 2001 until 2006 and adjusted for inflation only in April 2007.

[9] David Pearce attributed this perverse effect to historical ties between the governing Labour Party and the coal industry, which had suffered from the “dash for gas” over the 1990s and successfully lobbied for a lower tax rate on coal. Mineral oil was exempt from the tax because it was already covered by the rather unpopular ‘Fuel Duty Escalator’, a policy of automatic increases in the taxes on diesel and gasoline. Residential energy use was not taxed for fear of a possible regressive effect (Pearce, 2006).

Table 1
Taxation of energy and carbon content by fuel type.

Fuel type	Unit tax $\left[\frac{\text{Pence}}{\text{kWh}}\right]$	Fuel price $\left[\frac{\text{Pence}}{\text{kWh}}\right]$	Tax rate [Percent]	Implicit carbon tax $\left[\frac{\text{Pounds}}{\text{Ton of carbon}}\right]$
Electricity	0.43	4.25	10.1	31
Coal	0.15	2.46	6.1	16
Natural gas	0.15	0.91	16.5	30
LPG	0.07	0.85	8.2	22

Notes: Fuel prices and taxes are measured in Pence per kilowatt hour (kWh) equivalent. Average fuel prices in 2001 are based on the QFI sample (see Section 4 for details). Carbon prices taken from Pearce (2006).

pure carbon tax the CCL is a tax on energy with non-uniform rates, shaped by a mixed bag of fiscal and regulatory goals.

Similar to other European governments that had introduced energy taxes during the 1990s, the UK government set up a scheme of negotiated agreements, the CCAs, in order to mitigate possible adverse effects of the CCL on the competitiveness of energy intensive industries. By participating in a CCA, facilities in certain energy intensive sectors can reduce their tax liability by 80% provided that they adopt a binding target on their energy use or carbon emissions.

Defined either in absolute terms or relative to output, these targets were negotiated at two levels. In an 'umbrella agreement', the sector association and the government – represented at the time by the Department for Environment, Food, and Rural Affairs (DEFRA) – agreed upon a sector-wide target for energy use or carbon emissions in 2010 and on interim targets for each two-year compliance period. At a lower level, 'underlying agreements' stipulate a specific reduction to be achieved by a 'target unit', i.e. a facility or group of facilities in a sector with an umbrella agreement. DEFRA originally negotiated 44 umbrella agreements with different industrial sectors, including the ten most energy intensive ones.[10]

While the primary objective of both the CCL and the CCAs is to enhance the efficiency of energy use in the business sector, the two instruments represent fundamentally different approaches. The levy provides a price signal at roughly 15% of energy prices faced by the typical business in 2001 (NAO, 2007). If energy demand is price sensitive, the increased relative price of energy should lead to a reduction in energy consumption. In terms of CO_2 emissions, this effect could be offset in part by a shift towards more carbon-intensive fuels.

In contrast, the CCA combines a very diluted price signal of $0.2 \times 15\% = 3\%$ of energy prices faced by the typical business with quantity regulation, mostly in the form of efficiency targets. This target affects the plant only if it places a binding constraint on the trajectory of energy use during the remaining economic lifetime of the plant. If this is not the case, the plant faces weaker incentives for energy conservation than it would under the full tax rate. Moreover, since most targets are specified in terms of energy units rather than carbon emissions, there is no guarantee that even a stringent energy target leads to emission reductions.

2.2. How stringent are the targets negotiated in the CCAs?

In theory, an omniscient government can choose a combination of tax discount and reduction targets so as to induce at least as much abatement as under the full tax rate (Smith and Swierzbinski, 2007). In reality, however, the government is unlikely to have perfect information about firm-specific abatement cost, especially if firms worry that sharing this information with the government weakens their bargaining position in the target negotiations. What is more, the government might not have been willing to drive a hard bargain for fear of jeopardizing international competitiveness and exacerbating distortions in marginal abatement cost (de Muizon and Glachant, 2003; Smith and Swierzbinski, 2007). A closer inspection of the negotiation, monitoring and enforcement of CCA targets yields a number of reasons to believe that the targets did not place binding constraints on firm behavior.

First, the government may have "double counted" carbon savings from the CCA scheme (ACE, 2005). On average, CCA targets were supposed to improve energy efficiency by 11% between 2000 and 2010. This figure is well above the 4.8% improvement the government expected to occur under a "business as usual" (BAU) scenario (AEAT, 2001). However, alternative BAU scenarios were much closer to the CCA target, projecting energy efficiency of all UK industry to improve by 9.5% (DG Transport and Energy, 1999) or even 11.5% when taking into account the effect of the CCL (DTI, 2000).

Second, there was massive overcompliance with CCA targets. Combined annual carbon savings in all CCA sectors were substantially larger than the 2010 target throughout the first three compliance periods. At the end of the first compliance period in 2002, CCA sectors reported savings of 4.5 MtC — almost twice the target amount of 2.5 MtC to be achieved by 2010.[11] Consistent with this, the proportion of compliant target units was high, rising from 88% in the first compliance period to 98% and 99% in the second and third compliance periods, respectively (AEAT, 2004, 2005, 2007). CCA participants that did not meet their target could attain compliance by buying emission allowances on the UK Emissions Trading Scheme (UK ETS), a carbon market that was operational between 2002 and 2006. Allowance prices in this market remained below the implicit carbon tax rates given in Table 1.[12]

Third, the lower bound on compliance cost is zero. This is because facilities were re-certified for the reduced tax rate even if they had missed their target, provided that the sector as a whole met its target. In 2004, this was true of approximately 250 non-compliant target units (NAO, 2007).

Finally, a large degree of flexibility in both the target negotiations and the compliance review further limited the stringency of CCA targets. For instance, CCA sectors could choose their own baseline year for the target indicator. More than two thirds of all sectors chose a baseline year prior to 2000 (in some cases going as far back as 1990), allowing them to count carbon savings unrelated to the CCA towards target achievement (NAO, 2007). Furthermore, targets could be adjusted ex post to reflect a more energy intensive product mix, declining output, or other 'relevant constraints'.[13] Because of this, and for the reasons given above, it appears unlikely that the negotiated CCA targets placed binding constraints on energy use by the average CCA company.[14]

2.3. Previous evaluations of the CCL package

Several evaluations of the CCL package were conducted at different stages of its implementation. In the 2000 Regulatory Impact Assessment, the government projected that the CCL instrument alone would

[10] See online Appendix A for more details.

[11] Most of this (2.6 MtC) was due to a dramatic decline in steel production. But even without steel and three other sectors that adopted absolute targets there was substantial overcompliance, with estimated carbon savings of 3 MtC (3.9 MtC and 4.3 MtC, respectively, in subsequent compliance periods; see NAO, 2007).

[12] The allowance price fluctuated between £7 and £15 per ton of carbon (£2 and £4 per ton of CO_2 equivalent) for most of the period (Smith and Swierzbinski, 2007). This price was conditioned primarily by marginal abatement costs of 32 'direct participants' in the UK ETS who had bid emission reductions in exchange for government incentives. Trading activity in the UK ETS increased in March 2003 and March 2005 when some CCA firms bought allowances to meet their interim targets, yet this demand was not strong enough to put upward pressure on the permit price.

[13] In addition, performance in some sectors was measured against a 'tolerance band' in lieu of a fixed target. In some instances, fast growing companies that belonged to a sector with an absolute target successfully bargained for a relative target (and vice versa) as this made it easier to achieve compliance (NAO, 2007).

[14] There is, however, a sizable number of plants that are not signing up for a CCA despite being eligible. This is likely due to costs of joining a CCA other than those associated with meeting specific energy consumption targets. For example, CCA participants need to comply with more elaborate monitoring requirements and pay their sector association for the cost of negotiating the agreements. Appendix B has a more elaborate discussion of this along with a detailed analysis of CCA take-up.

achieve carbon savings of at least 2 MtC in 2010 against BAU projections (HMCE, 2000). This estimate was based on a model of business energy use maintained by the Department of Trade and Industry (DTI). An interim evaluation study, commissioned by DEFRA at the end of the second commitment period in 2004, finds evidence that the announcement of the CCL package in March 1999 reduced energy demand in the service and public sectors, but not in manufacturing (Cambridge Econometrics, 2005). The authors of the study identify this "announcement effect" as a structural break in an error correction model of quarterly energy demand (see Agnolucci et al., 2004, for more details). A series of simulation studies uses a macroeconometric model of the UK economy to assess the CCL package. An important result is "that the energy (and therefore carbon) saving and energy-efficiency targets would have been met without the CCAs" (Cambridge Econometrics, 2005, p. 7), which confirms the conclusion drawn above on the lack of target stringency. Since model simulations of the CCL package give rise to much smaller carbon savings than official estimates computed for the first compliance period (AEAT, 2004), Ekins and Etheridge (2006, p. 2079) conclude that "the CCL package as implemented [...] achieved a greater carbon reduction than a no-rebate CCL would have done by itself". They attribute this to managers becoming aware of more cost-effective efficiency enhancement projects as they started to benchmark their energy use. To be sure, the existence of such an "awareness effect" depends on whether the official carbon savings were real and not just a consequence of AEAT's (2001) pessimistic BAU scenario. In another simulation study on the impact of the CCAs on output and employment, a large effect of the CCAs on sectoral energy demand – averaging a 9.1% reduction in sectoral energy use by 2010 – is built into the model rather than estimated (Barker et al., 2007).

These assessments of the CCL package highlight two fundamental challenges in policy evaluation, namely (i) to determine a valid baseline against which to measure the impact of a policy and (ii) to attribute any measured impact to this policy in a causal fashion. In studies that use simulated trajectories of energy use as a baseline against which to measure the impact of the CCL package, the validity of the results critically depends on the counterfactual baseline being true. In econometric studies based on time series data at the sector level, it is difficult to discern the effects of the policy from that of unobserved aggregate shocks.[15]

The present study is the first evaluation of the Climate Change Levy package to use longitudinal business microdata. We address the baseline problem by comparing changes in actual firm behavior under two types of policy regimes, thus purging the effect of aggregate shocks. Moreover, we identify the causal effect of the tax by exploiting exogenous variation in the eligibility rules for the tax rebate. The next section explains our research design in detail.

3. Research design

3.1. Econometric model

We seek to estimate the effect of the CCL by comparing plants that pay the full tax rate with plants that pay just 20% of the tax by virtue of being in a CCA. We consider the estimation equation

$$y_{it} = \alpha T_{it} + x_{it}'\beta + \xi_t + \eta_i + \epsilon_{it} \tag{1}$$

where y_{it} is an outcome variable (for expositional purposes, think of energy use), T_{it} is the treatment dummy indicating that a plant pays the full rate of the tax, x_{it} is a vector of strictly exogenous covariates (including a constant), ξ_t and η_i are unobserved year and plant effects, respectively, and ϵ_{it} is a random disturbance term. Three fundamental issues need to be addressed. First, while the CCA plants in the control group receive a tax discount they are also subject to an energy consumption or efficiency target which might affect their choices. Second, participation in a CCA is voluntary but not every plant is eligible. This creates a selection endogeneity in the control group. Finally, the tax might have heterogeneous impacts among the group of treated plants.

Estimation of Eq. (1) recovers the full effect of the CCL if – as previous research has suggested – CCA targets did not impose binding constraints on firm behavior. If the converse is true, the estimated α falls short of the true price effect as control plants choose lower-than-optimal levels of energy so as to comply with their CCA target. Hence, the estimated parameter α can be regarded as a conservative estimate of the impact of the CCL. Fig. G.1 in the online appendix illustrates this point.[16]

In order to estimate α consistently, one needs to address the issue of non-random selection of plants into the control group. As we document in Section 4.2 below, CCA plants are, on average, older, larger and more energy intensive than CCL plants. Clearly, plants using large amounts of energy receive a larger absolute discount on their CCL liability which gives them a stronger incentive to join a CCA. In turn, as there are fixed costs of participating in a CCA, plants with low levels of energy use may find it more profitable *not* to join.[17] This is illustrated in Fig. G.2a of the online appendix. In principle, selection effects can be addressed by adding further control variables, but selection might in part be driven by factors not directly observable to us. For instance, given two plants that initially use the same amount of energy, the plant with the steeper marginal abatement cost schedule has a stronger incentive to join the CCA (cf. Fig. G.2b in the online appendix for an illustration).

Thanks to having panel data we can control for selection based on time-invariant unobserved heterogeneity η_i across plants by taking first differences of Eq. (1).[18] This yields

$$\Delta y_{it} = \alpha \Delta T_{it} + \Delta x_{it}'\beta + \Delta\xi_t + \Delta\epsilon_{it}. \tag{2}$$

Least-squares estimation of Eq. (2) provides an unbiased estimate of the treatment effect α if $\Delta\epsilon_{it}$ – the short-term deviation from a plant's idiosyncratic trend in energy consumption – is exogenous to the decision to join a CCA. This is not true if plants take into account their future energy consumption when deciding on CCA participation. Plants expecting to expand their energy consumption may perceive the CCA target as a binding constraint and therefore rather not join a CCA, whereas plants that expect a reduction in consumption will take the opportunity to reduce their tax liability provided that the cost of joining the CCA is not too large. As a result, plants might select themselves into treatment and control groups based on time-varying unobserved shocks to the outcome variable, causing bias in the estimate of α.

To address this issue, we adopt an instrumental variable approach based on eligibility rules for CCA participation. Econometrically, we perform a two-stage least squares estimation of Eq. (2) using the eligibility indicator ΔZ_{it} as an instrumental variable for ΔT_{it}. We also consider a reduced-form or "intent-to-treat" regression of the outcome on the instrument variable

$$\Delta y_{it} = \widetilde{\alpha}\Delta Z_{it} + \Delta x_{it}'\widetilde{\beta} + \widetilde{\xi}_t + \Delta\widetilde{\epsilon}_{it}. \tag{3}$$

[15] When the CCL package was introduced, energy markets in the UK had been undergoing important changes that entailed significant and prolonged adjustments to prices, notably declining electricity prices and increasing prices of gas and coal.

[16] The stringency of CCA targets – though relevant for the interpretation of the estimated effect as a lower bound on the full tax effect – does not affect the consistency of the estimation procedure. For example, if the targets were more stringent than the full-rate tax then our method would lead to a negative coefficient on CCA participation. This would still be a lower bound on the tax effect, albeit not a meaningful one.

[17] In personal communications, representatives of CCA sector associations pointed out multiple sources of fixed costs to us. The main cost drivers are payments to consultants or staff for doing the necessary energy accounting and administrative work as well as administrative fees charged by the sector associations.

[18] In our data we face the practical issue that some smaller plants are not sampled consecutively. In order not to throw away information on those plants we define the dependent variable in Eq. (2) as $\Delta y_{it} = y_{it} - y_{it-1}$ for $t \leq 2000$ and $\Delta y_{it} \equiv y_{it} - y_{i2000}$ for $t > 2000$ and transform the RHS accordingly. See Appendix C for details.

3.2. Instrumental variable

Eligibility for CCA participation was granted to plants engaged in polluting activities regulated under the PPC act (listed in Appendix B.1). An eligible plant is comprised of at least one installation dedicated to the PPC activity, such as a blast furnace or cement kiln. The discounted rate of the CCL applies to all energy use at this installation.[19] We define the instrumental variable Z_i as an indicator variable that equals 0 for all plants containing at least one eligible installation, and 1 otherwise. The instrument is relevant because the eligibility of a plant for CCA participation ought to be correlated with its tax regime.

Furthermore, the validity of using ΔZ as an instrument for ΔT in Eq. (2) rests on the identifying assumption that eligibility is orthogonal to shocks $\Delta\epsilon_{it}$ that occurred after 2000. This assumption deserves a careful assessment. For instance, one might worry that plants could self-select into PCC activities in order to become eligible for the CCA. Since the entire CCL package was conceived and implemented in a mere two years, and eligibility rules were established only a year before implementation (in the 2000 Financial Act) it appears unlikely that firms switched technologies in the short run just because of the CCA discount.

Moreover, if PPC regulated and non-regulated plants are subject to different trends in the outcome variables, the resulting IV estimates will be biased. In the empirical analysis to follow, we investigate this possibility by looking at pre-treatment trends but find no evidence of such differences. A visual examination of time series plots of various outcome variables (shown in Fig. 1 below) suggests no systematic differences in trends between eligible and non-eligible firms before 2001. The corresponding statistical test results are reported in panel B of Table 2 and fail to reject the hypothesis of common trends. Furthermore, our panel dataset allows us to directly control for differential trends in the outcome regressions. As we discuss in Section 5.3 below, this does not lead us to reject the hypothesis that outcomes in PPC firms and non-PPC firms followed a common trend before the introduction of the CCL. Also, the point estimates of the tax effect hardly change when controlling for pre-trends.

Finally, the exclusion restriction also rules out the possibility that mandatory public disclosure of PPC pollution in the European Pollution Emissions Register (EPER) had a direct effect on the outcome variables. While this assumption is untestable, we are not aware of any evidence that EPER reporting requirements affected firm behavior in the UK.[20] Moreover, the fact that pollution emissions in 2001 were published only in 2004 rules out any direct effects operating through the demand side.

It is worth noting that the exclusive focus on pollution intensity when eligibility was first determined left many energy intensive industries ineligible for the tax discount. For instance, textile wet processing was an eligible activity thanks to its high pollution emissions, but not so dry processing which, although energy intensive, emits no pollution regulated under PPC. Similarly, both the production and the recycling of glass containers are very energy intensive processes. However, since only the former is pollution intensive, glass container recycling was not eligible for CCA participation.[21] This institutional 'glitch' induces exogenous variation in the probability of treatment even within narrowly defined, energy intensive industrial sectors.

3.3. Heterogeneity of the treatment effect

So far the treatment effect α was implicitly assumed to be homogeneous across plants. For the case of heterogeneous responses to treatment, Imbens and Angrist (1994) have shown that, under certain conditions, the IV estimator identifies the average treatment effect on "compliers", i.e. on the subset of the treated for which a change in the instrument induces a change in treatment status. Although "compliers" need not be representative of all treated plants, an instrument based on a strict eligibility rule identifies the average treatment effect on the treated (ATT), simply because non-eligible plants cannot receive treatment (there are no "always-takers"). This result was first derived by Bloom (1984) and can be applied to our setting with only minor modifications to the interpretation.

Recall that the treatment we consider is to pay the full tax rate, and that the instrumental variable indicates whether or not a plant is eligible for an exemption from treatment. All ineligible plants must pay the full tax rate, so that only eligible plants are able to escape the treatment (i.e. there are no "never takers"). In Section D of the online appendix, we show that the IV estimator identifies the average treatment effect on the non-treated plants (the ATNT), i.e. those that apply for a tax discount when given the opportunity. We shall refer to this subpopulation in a more intuitive way as the group of "tax concerned" plants.

As we explain in more detail below, we measure eligibility using data from the EPER database. These data cover all facilities with PPC emissions above certain reporting thresholds, whereas eligibility for a tax discount was granted regardless of the amount of emissions. In Appendix D we show that this has no effect on the interpretation of our estimates as long as firms below and above the reporting threshold do not differ systematically with respect to their treatment response and their probability of being tax concerned.

4. Data

The compilation of a dataset suitable for the micro-econometric evaluation of the CCL required a major effort in terms of data collection, cleaning and matching. The result is a unique dataset that matches publicly available information on CCA participation and EPER coverage to production data from two confidential business datasets.

4.1. Data sources

The core dataset is the Annual Respondents Database (ARD) which is maintained by the Office for National Statistics (ONS) and can be accessed by approved researchers through the UK Data Service's secure access program.[22] The ARD is an annual production survey that covers about 10,000 plants in the manufacturing sector.[23] During the sample period, all plants with 250 employees or more (in some industries: 100 or more) had to report annually whereas smaller plants were included on a random basis (Barnes and Martin, 2002). The ARD provides information on the plant's age, number of employees, gross output (revenue), variable cost, capital stock, materials, and energy expenditures (inclusive of CCL payments).

[19] In addition, energy use at non-eligible installations on the same site is also taxed at the lower rate, up to a maximum of one ninth of the primary energy use at the eligible installation. Hence, it was not possible to dodge the tax by adding an installation with the sole objective to make the entire plant eligible for the discount.

[20] In the context of the US Toxic Pollution Inventory, studies have found no significant effects of public disclosure rules alone on pollution abatement, stock market returns or housing prices (Bui and Mayer, 2003; Bui, 2005).

[21] Other examples include tire production vs. recycling (retreading), mining and processing of minerals using mechanical and thermal energy, and heat-treating of metals. Eligibility rules for CCA participation were amended to include such low-pollution, energy intensive processes, but the first amendment occurred only after the end of our study period, in 2006.

[22] Office for National Statistics, Annual Respondents Database, 1973–2009: Secure Access [computer file]. 3rd Edition. Colchester, Essex: UK Data Archive [distributor], June 2012. SN: 6644.

[23] Here and in the remainder of the paper a "plant" corresponds to an ARD reporting unit. This is the lowest aggregation level for which production data is available. In 70% of all cases a reporting unit is indeed a business unit at a single mailing address — a 'local unit'. Larger business units are allowed to report on several local units combined so as to reduce compliance costs. The information linking local units to reporting units is obtained from the Interdepartmental Business Register (IDBR), which in addition provides information on plant births and deaths as well as on employment, location and industry. For more details see Criscuolo et al. (2003).

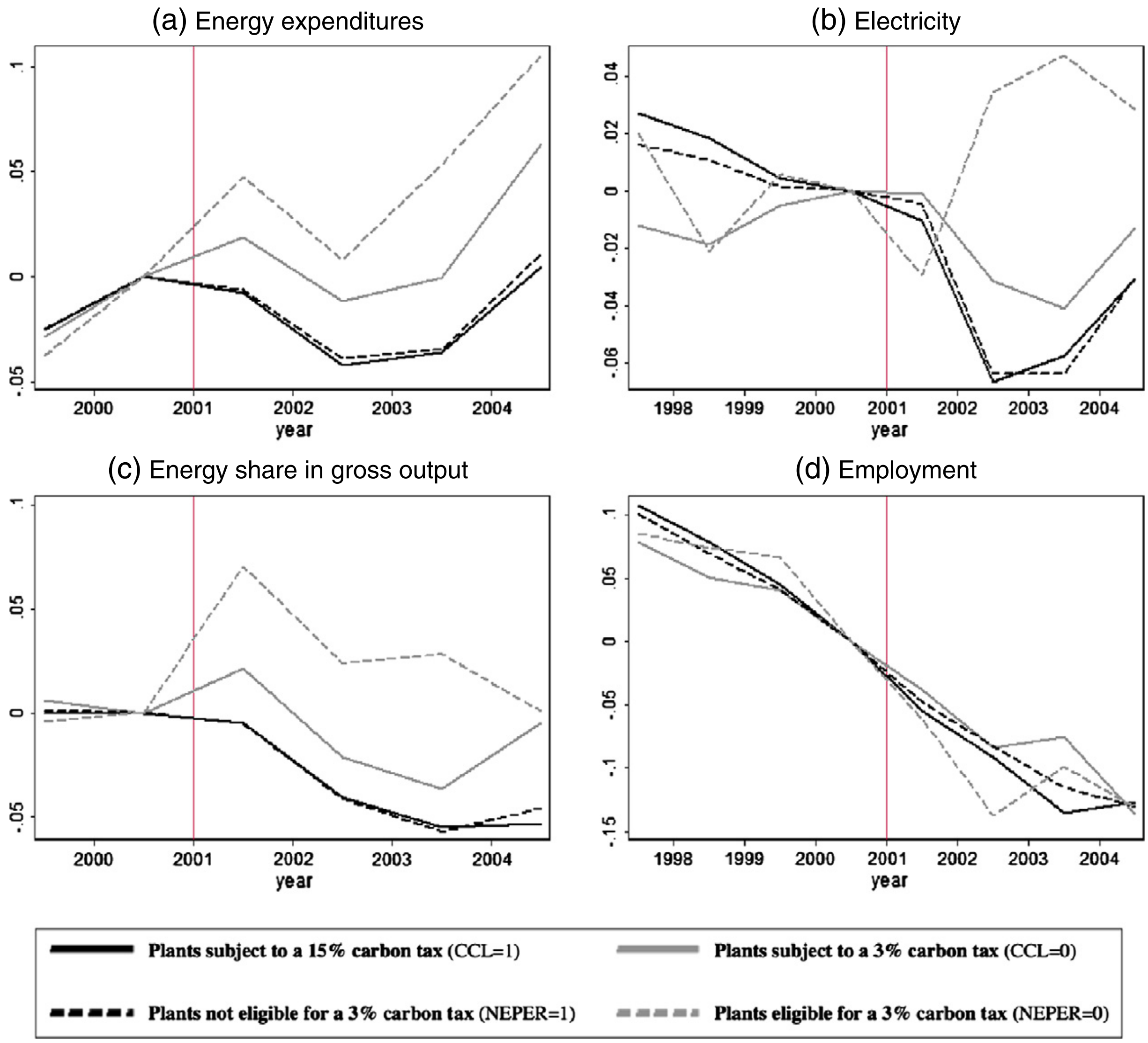

Fig. 1. Trends in outcome variables by treatment status. Notes: The graphs show the average change relative to the year 2000 ($y_{it} - y_{i2000}$) for selected outcome variables y_{it} separately for treated (CCL) and non-treated (CCA) plants and for eligible (EPER) and non-eligible (NEPER) plants. The vertical line indicates the beginning of the treatment in April 2001. The outcome variables are (clockwise): energy expenditures, electricity use, employment and the energy expenditure share in gross output.

Detailed information on energy use is taken from the Quarterly Fuels Inquiry (QFI), a quarterly survey among a panel of about 1000 manufacturing plants managed by the ONS on behalf of DTI. The survey collects data on expenditures and quantities for all relevant fuel types, including medium fuel oil, heavy fuel oil, gas oil, liquefied petroleum gas (LPG), coal (graded, smalls), hard coke, natural gas, and electricity. We have data for the period from 1993 to 2004. The majority (83%) of the observations in the QFI can be matched to the ARD without difficulty because both surveys use the same underlying government business register IDBR as their sampling frame. However, due to random sampling in the ARD we do not have ARD data for all QFI plants.[24]

We gathered information on CCA participation from both the DEFRA and HM Revenue and Customs (HMRC) websites. Lists of facilities in the original sector agreements were downloaded from DEFRA's website. The agreements stipulate the certification periods and the sector targets along with the details on the calculation of the units of energy used and carbon emissions. They also contain a list of all facilities initially covered by the CCAs. Seven agreements lack sufficient information on the facilities covered by the CCA and thus had to be excluded from the analysis.[25] The HMRC website provides, sector by sector, the list of facilities that have joined the CCA along with the date of publication.[26] The lists are regularly updated and facilities that have resigned from the CCA are removed. We merged the DEFRA and HMRC lists to obtain a complete list of facilities that pay the reduced rate of the CCL. We match this information to the ARD and QFI by combining information on a plant's postcode and the UK Company Register Number (CRN).

To construct the instrumental variable, we downloaded publicly available data from the European Pollution Emissions Register (EPER) which covers all European facilities regulated under the IPPC directive whose emissions exceed the reporting thresholds. The 2001 EPER file contains reporting thresholds and pollution discharges into air and water for 50 pollutants and covers 2397 facilities in 56 sectors of activity in the UK. We construct the instrumental variable *NEPER* as a dummy variable that equals one if a facility is not on the EPER list, i.e. it does not report emissions of any of the pollutants regulated under PPC

[24] For more details on the QFI and its combination with ARD data see Martin (2006).

[25] The craft baking sector and the meat processing sector do not contain a list of facilities. Another five sectors lack facility addresses, namely the NFU poultry meat production sector, the pig farming sector, the egg production sector, the British Poultry Meat Federation farms sector, and the British poultry meat federation processing sector.

[26] The date of publication is the date from which the CCA is applicable.

Table 2
Descriptive statistics in 2000 by tax regime and eligibility.

	(1)	(2)	(3)	(4)	(5)	(6)
	Plants subject to a 3% tax ($CCL = 0$)	Plants subject to a 15% tax ($CCL = 1$)	Diff.	Plants eligible for a 3% tax ($NEPER = 0$)	Plants not eligible for a 3% tax ($NEPER = 1$)	Diff.
A. Levels						
Energy share in gross output	−3.881	−4.386	***	−3.731	−4.340	***
ln(EE/GO)	696	3841		238	4299	
Energy expenditure	6.457	4.655	***	7.184	4.807	***
ln(EE)	696	3841		238	4299	
Electricity	16.311	15.068	***	17.516	15.193	***
ln(El)	149	368		52	465	
Employment	5.660	4.723	***	5.872	4.811	***
ln(L)	696	3841		238	4299	
Age	20.112	17.658	***	19.046	17.978	–
	696	3841		238	4299	
B. Differences						
Energy share in gross output	−0.004	0.000	–	0.012	−0.001	–
ln(EE/GO)	696	3841		238	4299	
Energy expenditure	0.030	0.025	–	0.037	0.025	–
Δln(EE)	696	3841		238	4299	
Electricity	0.012	−0.008	–	−0.003	−0.002	–
Δln(El)	149	368		52	465	
Employment	−0.017	−0.022	–	−0.017	−0.021	–
Δln(L)	696	3841		238	4299	

Notes: Summary statistics for the year 2000 (panel A) and the difference in growth rates between year 1999 and 2000 (panel B) by CCL and NEPER status. For each variable, we report the mean and the number of observations in the row below the variable mean. We report the natural logarithm for all variables except age. Columns 3 and 6 report significance levels of a *t*-test of differences in group means with unequal variance, at <1% (***), <5% (**), <10% (*). Among the 4537 ARD plants, 3743 are not eligible for the 3% reduced tax rate and pay the 15% carbon tax, and 556 are not eligible for the 3% tax rate but nevertheless benefit from the reduced rate.

legislation. A value of zero is assigned otherwise. Just like the treatment variable *T*, this variable is zero for all plants before 2001 and does not vary between 2001 and 2004. To match EPER facilities to plants in our dataset we use the same algorithm that we used for matching CCA participation data.

4.2. Descriptive statistics

Our regression sample comprises 6886 and 1079 plants in the ARD and QFI datasets, respectively.[27] Table G.1 in the online appendix reports descriptive statistics. We calculate energy intensity as the share of energy expenditures in either gross output or variable costs (the sum of expenditures on materials, energy and wages), finding a substantial amount of dispersion between plants. For example, the energy expenditure share in gross output of a plant at the 90th percentile is seven times larger than that of a plant at the 10th percentile. We report both quantities consumed and expenditures paid for the fuel variables, after aggregating up some of the variables available in the QFI to obtain the categories liquid fuels (oil, petrol, and LPG), solid fuels (coal and coke) and natural gas (firm contract, interruptible contract, tariff). Moreover, we compute the share of natural gas in the consumption of both gas and electricity, and total CO_2 emissions (in thousands of tonnes) on the basis of the fuel mix.

The regression sample starts in 1999, because this is the first year for which energy expenditure data are available in the ARD, and covers the first two target periods that lasted from 2001 until 2004. This window of analysis avoids possible complications due to (i) an overlap with the EU ETS which affected approximately 500 CCA plants from 2005 onwards, (ii) adjustments of CCA targets for the third compliance period, and (iii) new entry of sectors in 2006 following changes in the eligibility rules.

Table 2 displays the means of the main variables in the pre-treatment year 2000 (panel A) and the differences between year 2000 and 1999 (panel B), broken down by treatment and eligibility status.[28]

The treatment variable *CCL* takes a value of one if a plant pays the full tax rate and a value of zero if the plant participates in a CCA. Panel A shows that participation in CCAs is not random: CCA plants are, on average, older, larger and more energy intensive. For most of these plant characteristics, a *t*-test of equal group means for CCL and CCA plants rejects at the 1% significance level. Given this strong correlation between treatment status and observable plant characteristics, we cannot rule out that unobservable plant characteristics also influence selection.

We address selection in levels by differencing out fixed unobserved plant characteristics in Eq. (2). To mitigate bias from selection on changes in the outcome variables, we instrument the difference regression using eligibility which is presumably exogenous to innovations in the outcome variables. This assumption is more credible if we find that eligible and non-eligible plants do not follow systematically different trends in terms of the outcome variables ahead of the treatment. We examine this in Fig. 1 by plotting average changes in the main outcome variables with respect to the year 2000, both for eligible and non-eligible plants, as well as by treatment status.[29] This shows that trends were closely aligned when treatment was imminent. More formally, panel B of Table 2 reports the pre-treatment growth rates by treatment and eligibility status, along with the results of a *t*-test for group equality. The test never rejects at the 5% level, suggesting that differential pre-trends in outcome variables were not important. This mitigates concerns about changes in the outcome variables being confounded with unobserved attributes of eligible firms. Finally, selection bias might also arise if attrition rates are systematically related to treatment status. We investigate this in Section 6.3 below, finding no significant impact of the CCL on plant exit relative to CCA plants.

5. Results

5.1. Determinants of CCL status

Table 3 reports the results from various regressions of CCL status on *NEPER* and other plant characteristics. Each regression is run in both the ARD and the QFI samples. Columns 1 and 4 report the marginal effects from a probit regression of *CCL* on *NEPER* in the cross section for the

[27] To limit the effect of outliers we dropped 441 observations in the ARD and 119 observations in the QFI sample for which growth in the outcome variables were in the top and bottom 1%. We omit SIC sector 23, "Production of Fuels (cookeries, refineries)", as it is exempt from the CCL based on "The Climate Change Levy (Use as Fuel) Regulations 2001" No. 1138.

[28] See Table G.3 in the online appendix for further outcome variables not reported here.

[29] See Fig. G.3 in the online appendix for the remaining outcome variables.

Table 3
Determinants of CCL status.

Dependent variable	Probability that a plant is subject to a 15% carbon tax ($CCL = 1$)					
Sample	ARD sample			QFI sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Time period	2001	2000–2004	2001	2001	1998–2004	2001
Method	Probit	OLS	Probit	Probit	OLS	Probit
Not eligible for a 3% carbon tax ($NEPER = 1$)	0.284***	0.392***		0.440***	0.345***	
	(0.040)	(0.044)		(0.090)	(0.060)	
$\ln(\text{Gross output})_{t-1}$			0.033***			0.033
			(0.012)			0.082
$\ln(\text{Capital})_{t-1}$			−0.038***			−0.231***
			(0.010)			0.069
$\ln(\text{Energy expenditure})_{t-1}$			−0.043***			−0.105**
			(0.007)			0.044
$\ln(\text{Employment})_{t-1}$			−0.023**			0.147**
			(0.010)			0.044
Sector controls	Yes	Yes	Yes	Ye	Yes	Yes
R-squared	0.296	0.816	0.384	0.280	0.690	0.359
Observations	4027	16,876	3975	436	4619	424

Notes: Probit results report the marginal effect on the probability of being subject to the full-rate CCL. All regressions additionally include age, age squared, and region effects. Standard errors in parenthesis are robust to heteroskedasticity and autocorrelation (except probit models), and in addition pooled OLS's standard errors are clustered at the plant level. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

year 2001. The coefficients imply that a value of $NEPER = 1$ increases a plant's chances of paying the tax in full by 28.4% in the ARD sample and by 44% in the QFI sample. The results from the first-stage regression underlying the IV estimation of Eq. (2) in first differences are reported in columns 2 and 5. They corroborate that there is a robust positive and statistically significant relationship between the treatment variable and the instrument. Columns 3 and 6 display the results from a probit regression of CCL status in 2001 on various plant level controls evaluated at their 2000 levels. The coefficient estimates show that the simple correlations between CCL status and plant characteristics we found in Table 2 persist after controlling for sectoral differences. In particular, plants that were larger in terms of their capital and energy inputs prior to treatment were more likely to participate in a CCA. The coefficients on energy and gross output suggest that the same is true of more energy intensive plants. In Section B.2 of the online appendix we present further evidence pointing to size and energy intensity as the main determinants for take up among eligible plants. This is consistent with the notion that the 80% discount on the energy tax rate would allow only large and energy-intensive plants to accumulate enough tax savings to cover the fixed costs of CCA participation.

5.2. Treatment effect of the CCL

Table 4 summarizes the regression results for various outcome variables from the ARD (panel A) and the QFI (panel B). Columns 1 to 3 report, respectively, the OLS estimate of the treatment coefficient α in Eq. (2), the OLS estimate of the coefficient $\tilde{\alpha}$ in the reduced-form Eq. (3), and the average treatment effect on CCL plants obtained via IV estimation of Eq. (2).

The first two rows in panel A of Table 4 report the results for energy intensity measured as the share of energy expenditures in either gross output and variable costs, respectively. We find that the CCL caused plants to decrease their energy intensity relative to CCA plants. The point estimates from the IV regressions are −0.181 for the former measure and −0.211 for the latter. The effects are both economically and statistically significant. The importance of controlling for selection is evident from the sizable differences between the OLS and IV estimates. In particular, OLS estimation leads to an upward bias when estimating the effect of the CCL on the growth in energy intensity. This is because the OLS estimator does not correct for the self-selection of energy intensive plants into the low-tax regime, which we found in Table 3 above. As we show in Section 6.1 below, this type of plant responded more strongly to the CCL, causing bias towards zero in the OLS estimates.

In rows 3 and 4, we break down the effect on energy intensity by looking at its components. The IV point estimates of −0.095 for energy expenditure and 0.086 for gross output suggest that CCL plants both reduced energy and increased gross output so as to achieve the reductions in energy intensity reported in row 1. However, the point estimates are

Table 4
Impact of the CCL on plant outcomes.

	(1)	(2)	(3)	(4)
Dependent variables	OLS	RF	IV	Obs./plants
A. ARD variables				
Energy share in gross output	−0.023*	−0.058***	−0.181**	16,876
$\Delta\ln(EE/GO)$	(0.013)	(0.022)	(0.071)	6886
Energy share in var. costs	−0.026**	−0.067***	−0.211***	16,876
$\Delta\ln(EE/VCost)$	(0.013)	(0.022)	(0.071)	6886
Energy expenditure	−0.019	−0.030	−0.095	16,876
$\Delta\ln(EE)$	(0.013)	(0.019)	(0.062)	6886
Gross output	0.004	0.027	0.086	16,876
$\Delta\ln(GO)$	(0.011)	(0.017)	(0.054)	6886
Employment	0.010	0.026	0.082	16,876
$\Delta\ln(L)$	(0.011)	(0.017)	(0.054)	6886
Total factor productivity	0.001	0.000	0.001	16,810
$\Delta\ln(GO)$ ~ inputs	(0.006)	(0.011)	(0.033)	6851
B. QFI variables				
Electricity	−0.033	−0.069**	−0.226**	4587
$\Delta\ln(El)$	(0.022)	(0.031)	(0.109)	1079
Natural gas	−0.053	0.052	0.165	3748
$\Delta\ln(Gas)$	(0.037)	(0.044)	(0.156)	908
Natural gas share	−0.027**	0.021	0.071	4587
$\Delta(Gas/(Gas + El))$	(0.012)	(0.023)	(0.079)	1079
Solid fuels	0.174*	0.101	0.460	1563
$\Delta\ln(So)$	(0.096)	(0.156)	(0.654)	445
Solid fuels share	−0.033	0.006	0.021	4587
$\Delta(So/kWh)$	(0.022)	(0.008)	(0.026)	1079
Total kWh	−0.105***	−0.004	−0.015	4587
$\Delta\ln(kWh)$	(0.027)	(0.037)	(0.118)	1079
CO_2	−0.073***	−0.026	−0.084	4587
$\Delta\ln(CO_2)$	(0.022)	(0.030)	(0.095)	1079

Notes: The estimates come from 39 separate regressions. Columns 1 and 3 report the OLS and IV estimates, respectively, of the coefficient on the treatment variable in Eq. (2). Column 2 reports the OLS coefficient on the instrumental variable in the reduced-form Eq. (3). Column 4 reports the number of observations and plants. Dependent variables are first-differenced from 1997 until 2000 and differenced at various intervals thereafter (Δ). All regressions include age, age squared, as well as dummies for year, region and 3-digit industry code. In panel A, the total factor productivity regressions also control for labor, capital stock, and for expenditures on materials and energy. Robust standard errors reported in parenthesis are clustered at the plant level. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

imprecise and lack statistical significance at conventional levels. This reflects the fact that both variables lump together prices and quantities, which are likely to move in opposing directions and thus attenuate the effect of higher energy prices.[30] Furthermore, we obtain a positive but not statistically significant point estimate for employment of 0.082.

We derive an estimate of the CCL impact on TFP from an augmented Eq. (1) which includes the production factors capital, labor, materials, and energy. This amounts to estimating a production function where the treatment variable captures the impact of the CCL on otherwise unexplained differences in TFP.[31] The coefficients reported in row 6 are positive but small in magnitude and lack statistical significance. We thus cannot reject the hypothesis that the CCL had no effect on plant-level TFP.

The evidence in panel A clearly shows that the CCL led to substantial reductions in plant-level energy intensity compared to the CCA. While the other coefficients are estimated less precisely, the point estimates are consistent with firms substituting labor for energy and increasing output prices in response to the energy price increase. In Appendix E we show that all the qualitative results in Panel A – including the stronger response of energy intensity than energy expenditures – can be generated by a simple equilibrium model with neo-classical production functions that exhibit a sufficiently large degree of substitutability between labor and energy.

The CCL package was not part of any harmonized carbon tax scheme for Europe but a unilateral policy measure. As such, it may have had a detrimental effect on the competitiveness of UK industry. On the basis of the positive but insignificant point estimates we obtain for employment and gross output, however, we cannot reject the hypothesis that the CCL did *not* cause firms to shed jobs or lose revenue relative to CCA firms. While it seems plausible that the CCL lowered profits we cannot estimate this effect directly for lack of pertinent data. However, if profit losses were substantial they might have induced firms to shut down some plants. We examine this possibility in Section 6.3 below.

From a climate-policy perspective, it is important to know whether reductions in energy expenditures in CCL plants actually occurred, whether they corresponded to reductions in energy consumption and whether they lowered carbon emissions. For example, instead of consuming less of all fuel types CCL plants might substitute towards fuels that are cheaper but also more polluting, such as coal. More detailed information on energy use is needed to address this issue, as the energy expenditures variable lumps together changes in the *tax-inclusive* price and quantity of energy, as well as the effects of substitution between different fuel types.

Panel B of Table 4 reports results from regressions using *quantity* changes in energy consumption by fuel type which are available in the QFI sample. Although this sample is smaller than the ARD sample, we find economically and statistically significant evidence that the CCL caused plants to decrease their electricity use by 22.6%. For natural gas, solid fuels, and solid fuels as a share of total kWh consumed we obtain positive point estimates of the treatment effect.[32] However, the coefficients are not estimated with enough precision to support conclusions about interfuel substitution.

The significant decrease in electricity consumption among CCL plants translates into a decrease in carbon dioxide emissions ceteris paribus, but this could be offset by an increase in the consumption of other fuel types. The last row of Table 4 shows the impact of the CCL on total CO_2 emissions, calculated as the sum of emissions across fuel types. The CCL is associated with a significant decrease in total CO_2 emissions of 7.3% in the OLS regression. The point estimate increases slightly when going from OLS to IV, yet statistical significance is lost. We conjecture that this is due to the noisy estimates of the tax response for fuels other than electricity. In the absence of a larger sample that would enable us to estimate this effect with more precision, there are two possible ways of quantifying the effect of the CCL on carbon emissions. On the one hand, one can choose to disregard statistically insignificant coefficients altogether and conclude that the unchecked decrease in electricity consumption translates into a decrease in CO_2 emissions of equal magnitude. On the other hand, a more cautious interpretation of the results is to use the point estimate of -0.084 from the IV estimation which accounts for the possibility that some CCL plants switched into dirtier fuels such as coal. We thus conclude that the CCL – though not designed as a pure carbon tax – caused plants paying the full rate to reduce CO_2 emissions by between 8.4% and 22.6% compared to plants that paid the reduced rate.

In further regressions, reported in Section F of the online appendix, we interact the treatment indicator with year dummies so as to recover the time profile of the treatment response following the introduction of the CCL. This can reveal possible time delay in plants' responses to the treatment, or whether the treatment effect dies off after a while. We find that the tax has the largest effect on the ARD outcome variables in the first two years of treatment. While the negative impact of the CCL on electricity use is statistically significant from 2002 onwards, the point estimates for natural gas and coal are usually not statistically significant at the 5% level.[33]

5.3. Robustness checks

5.3.1. Balanced sample

Our sample is an unbalanced panel for a number of reasons: random sampling of smaller plants in the ARD, plant births and deaths, and missing responses from some plants in some years. As the set of plants in the sample changes slightly from year to year, the time profile of the treatment effect might reflect – at least in part – the changes in sample composition rather than the dynamic response to the CCL. Another potential problem with the unbalanced panel is that the results could be dominated by potentially more extreme responses of exitors. To address these concerns, we estimate the model with time interactions in a subset of "stayer" plants with observations in all years after 1999. The results are summarized in Tables G.4–G.6 in the online appendix. Since the sample size drops by about half in both samples, some of the estimated treatment effects lose statistical significance. However, the qualitative findings remain similar to the ones estimated on the full sample.

5.3.2. Controlling for pre-treatment trends

Our identification strategy relies on the (untestable) assumption that differences between eligible and non-eligible plants are not systematically related to changes in outcome variables over the treatment period. In Section 4.2, we have shown that pre-treatment trends did not differ across these groups in a statistically significant way, meaning that our estimates are unlikely to confound the impact of the treatment with pre-existing differences. To corroborate this, we include a time-invariant eligibility dummy in Eq. (2) so as to directly control for unobserved trends in the outcome variables, separately by eligibility status.[34] Tables G.7 and G.8 in the online appendix show that this yields qualitatively similar results, albeit less statistically significant ones in later years. Since the coefficients on the eligibility dummy are statistically

[30] As firms pay a higher after-tax price for energy but likely demand less of it, energy expenditures can go up or down. Moreover, the effect on revenue is dampened because the higher marginal cost tends to raise product prices while also reducing physical output.

[31] This controls for production function endogeneity arising from fixed unobserved heterogeneity across plants (Griliches and Mairesse, 1995).

[32] We report natural gas use as a share of gas and electricity only, as other fuels are less frequently used. The regressions on solid fuels are conditional on a plant using solid fuels in at least one period. In contrast, the solid fuels share is computed for all plants and takes the value of zero for plants that do not use it.

[33] A positive and significant point estimate is obtained for gas consumption in 2001. However, this result proves not robust to controlling for endogenous attrition of gas consuming plants in the logarithmic specification, which could generate this result in a spurious manner. See Appendix F for details.

[34] Like sector and region dummies, this dummy is interacted with year differences to account for time intervals of varying length in the sample. See Appendix C for details.

insignificant for all outcome variables except solid fuels, we do not include them in our preferred specification.

5.3.3. Common support regression

Despite our IV strategy there might be concern that results are driven by a fundamental heterogeneity between treated (eligible) and non-treated (non-eligible) plants. Therefore, as a robustness test we restrict the control group to a common support which is identified by the predicted probability of a plant in the control group to receive treatment.[35] We construct this common support sample by dropping plants that do not belong to the central 80% of the propensity score distribution, while also balancing the covariates between the treatment and the control group.[36] The results obtained for the common support sample are reported in Table G.9 of the online appendix. For the ARD variables in panel A this leads to slightly larger point estimates, suggesting that heterogeneity within the treated group is not a major problem. In the smaller QFI dataset, about half of the sample needs to be dropped, but this entails no qualitative change to the results.

6. Heterogeneous impacts, aggregate effects, and plant exit

6.1. The impact of the CCL in different subsamples

Our discussion so far has focused on the average effect of the CCL on non-treated plants. It is useful to know how this effect varies across plants with certain characteristics. For example, the tax impact may differ from the ATNT in industries that are very energy intensive because the levy imposes a higher cost burden on these industries. Moreover, as the political cost of job losses is high, policy-makers might be interested in the tax impact on small firms which are responsible for the bulk of total employment. Finally, the impact of the CCL on competitiveness may be particularly high for firms in sectors with high import penetration, as foreign competition prevents them from passing compliance cost on to their customers through higher output prices.

To shed light on this, we estimate the impact of the CCL separately: (i) for plants with more vs. less than 250 employees, (ii) for plants with high vs. low energy intensity and (iii) for plants with high vs. low trade intensity.[37] The first two columns of Table 5 report the IV coefficients for the split by energy intensity, defined as the share of energy expenditures in gross output. Results for the low- and high-intensity groups are reported in the odd and even-numbered columns, respectively. The IV point estimates for energy intensity and energy expenditures indicate that the average effects reported in Table 4 are due to a strong response by plants in energy intensive sectors. The point estimates in this group are −0.195 for energy intensity and −0.154 for energy expenditures, both are statistically significant at 5%. In contrast, the point estimates for the low-intensity group lack statistical significance. The point estimates for electricity consumption are similar in magnitude across groups but lack statistical significance in the low-intensity group.

In columns 3 and 4 of Table 5 we split the sample according to the trade intensity in 4-digit NACE sectors, which is computed as the value of imports and exports to non-EU countries over the total market size within the EU27.[38] This measure has been used by the EU Commission to gauge the competitiveness impact of the EU ETS on manufacturing firms. To the extent that trade intensity measures the degree of competition from non-regulated countries, it picks up the (lack of) ability of firms to pass on the cost of the CCL to their customers. The point estimates for the ARD variables obtained in the trade intensive group closely follow those obtained in the full ARD sample. In contrast, the impact on energy intensity is not statistically significant in the low-intensity group. We do not find any significant impact on employment in either of the two groups. This gives rise to two interpretations: first, that trade intensity might not be a good criterion for identifying adverse effects on competitiveness; or second, that the hypothesis which states that there are no such effects should not be rejected.

The last two columns of Table 5 report the results for the employment split. While the point estimates for energy expenditures in small plants and electricity use in large plants are negative and statistically significant at the 10% level, no clear pattern emerges from this comparison across size groups.

6.2. Aggregate effects of a carbon tax

While the micro-level approach allows for better identification of the causal impacts of the tax, from a policy point-of-view the aggregate implications of the tax matter. In this section, we compute the effect of a counterfactual carbon tax similar to the CCL but without the reduced tax rate. This exercise allows us to compare our results to studies assessing the impact of energy price changes on fuel consumption at the aggregate level.

Taking into account heterogeneous treatment effects at the plant level, the aggregate effect of the CCL on aggregate variable Y is given by

$$\Lambda_Y = \frac{\sum_i (e^{\alpha_i} - 1) Y_{i,2000}}{\sum_i Y_{i,2000}} \tag{4}$$

where the plant specific treatment effect α_i is weighted by the share of plant i in the aggregate Y. To be able to compute Λ_Y, we assume a homogenous treatment effect equal to the IV estimate among all tax concerned plants i, $\alpha_i = \hat{\alpha}_{ATNT}$. For tax unconcerned plants, we assume that treatment effects are zero because plants that do not apply for a tax discount are less likely to change their energy consumption in response to the tax itself. Finally, while all CCA plants are tax concerned by definition, there may be tax concerned plants in the non-eligible group. We predict the probability p_i that plant i is of the tax concerned type, using the probit models reported in columns 3 and 6 of Table 3. In computing the aggregate impact $\hat{\Lambda}_Y$, we weight each plant's impact by $\hat{p}_i$ and its share in the aggregate prior to treatment, i.e.

$$\hat{\Lambda}_Y = \left(e^{\hat{\alpha}} - 1\right) \frac{\sum_i \left(\hat{p}_i Y_{i,2000}\right)}{\sum_i Y_{i,2000}}. \tag{5}$$

According to this back-of-the-envelope calculation, had the CCL been applied to all plants without rebates, it would have decreased aggregate energy expenditures in manufacturing by at least 5.6% and aggregate electricity consumption by at least 13.4%.[39]

What do these estimates imply for the price elasticity of aggregate energy demand? Given that, on average, CCL plants pay $\frac{1.15}{1.03} - 1 = 11.7\%$ more for energy than CCA plants, the implicit price elasticity of energy

[35] See Blundell et al. (2004) for a framework that combines propensity score matching with a differences-in-differences estimator.

[36] Propensity scores are computed as the predicted values of a probit regression of CCL status on plant characteristics for the year 2000. We restrict the sample to the common support and verify that covariates in the resulting sample are balanced. Gross output, capital, materials, employment, the squares of these variables, as well as energy expenditures, are all balanced at the 1% confidence level.

[37] The splitting points for energy and trade intensities are defined at the 3-digit and 4-digit sector level, respectively, based on pre-treatment averages across plants in the sector. After sorting sectors in the order of decreasing intensity, we assign sectors to the high intensity group until approximately 50% of plants are assigned to this group. The remaining sectors are assigned to the low intensity group.

[38] Data on trade intensity were taken from the Impact Assessment accompanying the "Commission Decision determining a list of sectors and subsectors which are deemed to be exposed to a significant risk of carbon leakage pursuant to Article 10a (13) of Directive 2003/87/EC", of September 4, 2009. NACE is the statistical classification system of economic activities in the European Union.

[39] Respectively, $\Lambda_{EE} = [\exp(-0.095) - 1] \times 0.62 = -5.62\,\%$ and $\hat{\Lambda}_{EI} = [\exp(-0.226) - 1] \times 0.66 = -13.35\%$.

Table 5
CCL impact in different sub-samples (IV coefficients).

Dependent variables		(1)	(2)	(3)	(4)	(5)	(6)
		Energy intensity		Trade intensity		Size	
		Low	High	Low	High	Low	High
Energy share in gross output		−0.159	−0.195**	−0.115	−0.196*	−0.225	−0.141
Δln(EE/GO)		(0.159)	(0.081)	(0.098)	(0.100)	(0.190)	(0.087)
Energy expenditure		0.071	−0.154**	−0.081	−0.089	−0.292*	0.030
Δln(EE)		(0.131)	(0.072)	(0.088)	(0.084)	(0.172)	(0.077)
Employment		0.216	0.047	0.102	0.063	−0.082	0.119
Δln(L)		(0.146)	(0.054)	(0.090)	(0.068)	(0.108)	(0.089)
Electricity		−0.247	−0.233*	−0.321	−0.107	−0.059	−0.286*
Δln(El)		(0.235)	(0.138)	(0.252)	(0.110)	(0.175)	(0.161)
ARD sample	Obs.	8040	8836	8096	7871	10,145	6702
	Plants	3276	3610	3201	3213	4905	1971
QFI sample	Obs	2001	2586	1994	2318	2122	2274
	Plants	470	609	461	552	513	450

Notes: The table reports the estimated treatment effect on various plant-level outcomes and in different sub-samples, obtained from 24 separate IV regressions of Eq. (2). Energy and trade intensity samples are split according to the median defined at the 3-digit and 4-digit sector level, respectively, in 1999 or 2000. Size is defined based on employment at the respondent unit, those with 250 employees or less in 2000 or 1999 qualified as small. Robust standard errors reported in parenthesis are clustered at the plant levels. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

expenditures can be computed as $\eta_{EE} = \left|\frac{-0.052}{0.117}\right| = 0.44$. Under the assumption that the incidence of the CCL is on the buyers of energy, this implies an upper bound on the price elasticity of energy *demand* equal to $\eta_E = |-0.44-1| = 1.44$.[40] The elasticity of electricity demand can be computed in a similar fashion. Given that the CCL raised the electricity price by $\frac{0.43}{4.25} = 10.1\%$ for the average manufacturing plant (cf. Table 1), the tax differential between CCL plants and non-CCL plants is approximately $\frac{0.8\times0.43}{4.25+0.2\times0.43} = 7.9\%$. Hence the elasticity of electricity demand is given by $\left|\frac{-0.119}{0.079}\right| = 1.51$, which is slightly larger than the elasticity recovered in the ARD sample.

Both numbers are at the upper end of elasticity estimates obtained in comparable studies. For example, Bjorner and Jensen (2002) estimate the energy price elasticity at 1.37 in the pooled cross-section and 0.50 in a fixed-effects specification.[41] The reader should bear in mind, however, that we recover an estimate of a *tax-induced* price elasticity. Davis and Kilian (2011) argue that this is structurally different from elasticity estimates based on other kinds of price variation because taxes may be perceived as more persistent and hence induce larger behavioral changes. They also point to a possible additional effect of media coverage that accompanies the introduction of such taxes. Since the CCL was promoted as the UK's flagship regulation for mitigating climate change, there was ample scope for such an effect of the CCL, and our comparatively large estimates do not speak against this possibility.

Finally, notice that the IV point estimates are too large if we are underestimating the share of compliers $Pr(CCL = 0|NEPER = 0)$. This possibility could arise because we were not able to match all CCA facilities when information on the business address or name was missing or wrong. In this case, the intent-to-treat (ITT) parameter, or reduced-form coefficient, reported in column 5 of Table 4, can provide a lower bound because it does not depend on the quality of the CCA match. The ITT point estimates for energy expenditures and electricity are −0.030 and −0.069, respectively. This translates into elasticity estimates of $\left|\frac{\exp(-0.030)-1}{0.117}-1\right| = 1.25$ for energy demand and $\left|\frac{\exp(-0.069)-1}{0.079}\right| = 0.84$ for electricity demand which are both somewhat lower than the bounds derived using the simple approximation to the aggregate impact of the CCL.

6.3. The CCL and plant exit

The analysis so far has focused on how paying the full rate of the CCL affects various outcome variables in surviving plants. Rather than adjusting energy use and production at the intensive margin, there is a concern that firms might respond to the CCL by closing down plants altogether or by re-locating to non-regulated countries ("pollution havens"). After all, the substantial tax rebates granted under the CCA are intended to prevent such extensive-margin adjustments by energy intensive firms.[42]

We examine this by constructing a dummy variable *EXIT* which equals 1 in the year of exit (defined as the year following the last reported year) and 0 otherwise. To avoid recording data set attrition as plant exit, we construct *EXIT* based on the Interdepartmental Business Register (IDBR), which contains the universe of business establishments in the UK and serves as the sampling frame for the ARD and QFI data sets. If exit occurs in year t, the plant is removed from the sample in subsequent years. Note that we cannot estimate the effect of the CCL on plant exit decisions by substituting $EXIT_{it}$ for the outcome variable in Eq. (2) because we do not know whether plants that exited in pre-treatment periods would have received treatment or not.[43] Instead, we propose an IV estimator that exploits variation in pre-sample employment size. We define a dummy $SMALL_i$ which indicates that

[40] This assumption seems plausible given that fuel suppliers can easily switch between CCL and CCA firms. To test this, we employ the IV regression framework to estimate the causal impact of the introduction of the CCL on fuel prices exclusive of the tax. The results, reported in Table G.10 of the online appendix, suggest that producer prices of electricity and natural gas did not respond to the introduction of the CCL. The point estimates for less commonly used solid and liquid fuels are negative and larger in magnitude. This could indicate that suppliers of these fuels assumed part of the tax incidence, but the estimates are not very precise.

[41] Our OLS estimate in the difference equation implies an upper bound on the elasticity of 1.09 but – as we have argued above – this is biased towards zero if contracting firms select into CCAs.

[42] Loss of international competitiveness and carbon leakage have been used with some success by industry to lobby against carbon taxes or carbon pricing more generally (see Martin et al., forthcoming), for the case of permit auctions in the EU ETS. Virtually all European governments that levy taxes on energy use or carbon emissions (i.e. Denmark, Finland, Germany, Netherlands, Sweden and the UK) have also granted exemptions or partial tax rebates to industries carrying a high tax burden.

[43] If we assigned all plants that exit prior to treatment to the control group, the estimated treatment effect would be biased. To see this, recall that the differences-in-differences estimator of an exogenous treatment T is identified from the sample equivalent of the expression

$$\alpha = E[Y_{it}|T_i = 1, T_{it} = 1] - E[Y_{it}|T_i = 1, T_{it} = 0]$$
$$-E[Y_{it}|T_i = 0, T_{it} = 1] + E[Y_{it}|T_i = 0, T_{it} = 0]$$

where T_{it} indicates the treatment period and $T_i = 1$ indicates that a plant belongs to the treatment group. In the case of exit, by construction we have no exit in the treatment group, i.e. $E[EXIT_{it}|T_i = 1, T_{it} = 0] = 0$. As a consequence, even in the case of an exogenous exit probability $\rho > 0$ which is constant across plants and time periods (i.e. $\alpha = 0$), this estimator is upwardly biased, since $\hat{a} = \rho - 0 - (\rho - \rho) = \rho > 0$. This problem is aggravated in the IV estimator as we would falsely assign $NEPER = 1$ to some exiting plants that would have been listed in EPER had they survived until 2001.

employment at the plant was below the median in 1997. Using data from 1998 onwards, we estimate the probit regression

$$\Pr(EXIT_{it} = 1) = \Phi(\alpha CCL_{it} + SMALL_{i1997} + x_{it}'\beta). \qquad (6)$$

This allows for fixed differences in the exit propensity between small and large plants and, since employment size and treatment status are strongly correlated (see Table 2), *SMALL* may also, to a large extent, control for fixed heterogeneity between treatment and control groups. Moreover, we use the interaction of $SMALL_i$ with a post-treatment dummy $\mathbf{I}_{\{t>2000\}}$ to instrument for CCL_{it}. The idea behind this is (i) to use the fact that size influenced the decision to participate in a CCA and (ii) to rely on variation in size prior to our sample period so as to preserve the exogeneity of the instrument. The estimated coefficient α has the interpretation of a local average treatment effect (LATE).

Since all the information needed to estimate Eq. (6) is available from the IDBR, we implement these regressions at the local unit level (see footnote 23 above). Table 6 reports the results from probit and IV probit models, along with the corresponding reduced-form (RF) and first-stage (FS) results. In each of the exit regressions, the coefficient on *SMALL* is positive and significant, confirming the already well-documented empirical regularity that smaller firms are more likely to exit. The simple probit model yields a positive and significant coefficient estimate on *CCL* which implies a 5.9% increase of the exit probability at the average CCL plant. Notice that this effect is not necessarily causal. In fact, the positive coefficient is consistent with a reverse-causality explanation according to which, plants that anticipate to exit in the near future do not sign a CCA because the tax savings this generates over the remaining lifetime of the plant do not cover the fixed costs of certification to be paid upfront. Once we instrument for *CCL* status, the point estimate becomes statistically insignificant, as foreshadowed by the insignificant coefficient estimate on the instrument obtained in the reduced form. The first-stage regression coefficients show that our instrument is strongly correlated with CCL status. In sum, we find no evidence that the CCL had an impact on plant exit decisions. This finding is robust to the inclusion of industry controls and to splitting the sample by either energy or trade intensity as in Section 6.1 above.[44]

Our analysis has focused on exit decisions at the local unit level whereas the bulk of the variables used in Section 5 are only available at a slightly higher level of aggregation (the 'reporting unit' or 'plant'). Since employment (and only employment) is available at both levels of aggregation, we re-estimate a version of Eq. (2) using employment data at the local unit level in order to verify that the results obtained at the reporting unit level are robust. Table 7 reports estimates of the CCL impact on employment in the full sample and when the sample is split according to energy and trade intensities, or size (defined as above at the reporting unit level). Our preferred specification includes a trend coefficient for the treatment group (NEPER × year diff) because we find it to be statistically significant for the high trade intensity group.[45] As before, we do not find evidence of a detrimental effect of the CCL on employment, regardless of which way the data are cut.

Table 6
Exit regressions.

	(1)	(2)	(3)	(4)
	Probit	RF	FS	IV
Plants subject to a 15% carbon tax	0.059***	0.000	0.018***	−0.007
(CCL = 1) or SMALL × I{t > 2000}	(0.005)	(0.002)	(0.001)	(0.084)
SMALL	0.036***	0.037***	−0.001***	0.037***
	(0.001)	(0.001)	(0.000)	(0.001)
Observations	679,240	679,240	679,240	679,240

Notes: The table reports the results of probit (column 1) and IV probit (column 4) regressions of exit at the local unit level, along with reduced-form and first-stage regressions (columns 2 and 3, respectively). SMALL is a dummy indicating that employment at the plant was below the median in 1997. Coefficients in columns 1 and 4 are reported in terms of marginal effects w.r.t. the probability of exit, evaluated at the mean of the explanatory variables. The sample period ranges from 1998 to 2004. All regressions include year dummies, age and age squared. Standard errors are clustered at the local unit level. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

7. Conclusion

There is a growing consensus that climate policy should aim to regulate GHG emissions efficiently across a broad range of economic sectors. While curbing industrial emissions must be an integral part of any such policy, there is surprisingly little empirical evidence on the impacts of large-scale regulations of industrial GHG emissions — let alone using market-based instruments. In this paper we have provided the first micro-econometric evaluation of a carbon tax on the manufacturing sector. Unlike simulation-based evaluations, our approach does not require making assumptions about counterfactual – "baseline" – trends in the outcome variable of interest. Instead, we compare changes in outcomes both over time and between plants that were subject to different tax rates. The "baseline" is hence given by the contemporaneous outcomes of plants that faced lower tax rates by virtue of being in a CCA. Our estimates of the impact of the CCL are thus purged of confounding factors that affect plant performance at the level of the economy, the region and the sector. Since we also control for self-selection into CCAs by exploiting exogenous variation in CCA eligibility rules, we interpret our estimates as the causal effect of the CCL on plant outcomes.

We find robust evidence that the price incentive provided by the CCL led to larger reductions in energy intensity and electricity use than the energy efficiency or consumption targets agreed under the CCA. The tax discount granted to CCA plants has been justified as a means of preventing energy intensive firms from losing competitiveness in international product markets due to the unilateral implementation of the tax and to the lack of international harmonization. Although this has been widely argued, we find no discernible impact on employment, gross output or productivity across groups, and we cannot reject the hypothesis that the CCL had no impact on plant exit.

Our results show that the introduction of a moderate tax on energy encourages electricity conservation and helps to reduce energy intensity in the manufacturing sector. This is in contrast to previous research that attributed substantial carbon savings to the CCA scheme on the basis of comparisons with counterfactual baseline emissions (Ekins and Etheridge, 2006; Barker et al., 2007; AEAT, 2004).[46] While our research design arguably produces a more credible estimate of the effect of the CCL, it is clear that this effect is additional to any effect the CCA targets may have had on firm behavior.

Our study constitutes a first step towards building an evidence base that informs policymakers about the impacts of climate change policies on industry. As more such policies are being implemented across countries, and as business microdata are becoming more abundant and easier to access, we expect that researchers will exploit the variation in policies and institutional settings to make important contributions to this evidence base. In the context of climate change policy in the UK, there are several issues that deserve attention in future research. First,

[44] Table G.11 in the online appendix reports reduced-form and first-stage results for the robustness checks. The coefficient estimates for the full sample with 2-digit sector dummies – reported in columns 1 and 2 – are virtually identical to the ones in Table 6. When the sample is split by energy or trade intensity – columns 2 through 5 – the coefficient estimates for the reduced form remain unchanged and the first-stage estimates change only in insignificant ways.

[45] If we had a panel of year-to-year changes only (i.e. year diff = 1), the trend would be the coefficient on a time-invariant NEPER dummy. See Appendix C for details.

[46] This finding contrasts as well with results obtained by Bjorner and Jensen (2002) who investigate the consequences of a similar policy package in Denmark and obtain a positive effect of negotiated agreements on energy efficiency. Apart from institutional differences between the British and the Danish policy packages, the discrepancy might be owed to differences in the research design as these authors do not control for selection into negotiated agreements based on time-varying unobservables.

Table 7
CCL impact on employment at local units.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Energy intensity		Trade intensity		Size	
		Low	High	Low	High	Small	Large
Plants subject to a 15% carbon tax ($CCL = 1$)	−0.011	−0.027	0.013	−0.008	0.012	−0.120	0.056
	(0.045)	(0.117)	(0.042)	(0.064)	(0.074)	(0.074)	(0.057)
Trend of plants not eligible for a 3% carbon tax ($NEPER \times$ year diff)	0.003	0.006	0.001	−0.004	0.016**	0.000	−0.014
	(0.005)	(0.008)	(0.005)	(0.007)	(0.008)	(0.006)	(0.010)
Observations	972,213	467,629	480,847	444,149	410,646	917,447	29,759
Plants	207,971	102,970	100,207	92,157	90,671	186,281	6682

Notes: Columns display IV estimates of the impact of the CCL on log employment at the local unit level for different samples. The dependent variable is first-differenced from 1996 until 2000 and differenced at various intervals thereafter. NEPER is a dummy variable that equals one if a facility is not on the EPER list. Energy and trade intensity samples are split according to the median defined at the 3-digit and 4-digit sector level, respectively, in 1999 or 2000. Size is defined based on employment at the respondent unit, those with 250 employees or less in 2000 or 1999 qualified as small. All regressions include age, age squared, year dummies, a full set of region-by-year and 3-digit sector-by-year dummies. Robust standard errors reported in parenthesis are clustered at the plant level. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

it seems important to gain a better understanding of how plants achieved the substantial reductions in energy use that we measure. This will require gathering more qualitative information on the key drivers of energy conservation — be they technical, economic or managerial. This information could lead to the design of more sophisticated policy instruments. From a political economy point-of-view, an analysis of the bargaining over CCA targets and of compliance behavior of individual CCA facilities will provide valuable insights regarding the design of negotiated agreements. Finally, given the long-term nature of climate change, an important open question is whether a moderate energy tax such as the CCL can stimulate much-needed innovation to bring about substantial carbon reductions in the future.

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Disclaimer

Appendix. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2014.04.016.

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