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Economic Policy Analysis

Carbon costs and industrial firm performance:

Evidence from international microdata

Carbon costs have had a limited effect on the performance of industrial firms. In carbon leakage-sensitive sectors, rising carbon costs modestly reduce employment, while investments are ramped up and losses and exit probabilities are not noticeably increased.

This follows from a new study based on data on approximately 3 million firms from 32 countries in the period 2000–2019. Shadow prices of fossil energy are used to measure carbon costs stemming from the broad mix of climate and energy policies.

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Carbon costs and industrial firm performance: Evidence from international microdata

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Abstract

A central concern in climate policy making is that unilateral increases in carbon costs would adversely affect firm performance and competitiveness. Using comprehensive microdata, this paper provides first international firm-level evidence on the joint performance effects of carbon policies. Shadow prices of fossil energy sources are employed as an integral and internationally comparable measure of carbon costs. We evaluate the impact of carbon costs on various performance outcomes, considering up to 3.1 million firms from 32 countries and 15 competitiveness-prone industrial sectors in the period 2000–2019. We find little evidence of adverse performance effects for an average firm, except for economically modest employment reductions. However, there is considerable effect heterogeneity. Performance effects appear to be most pronounced in carbon leakage sectors and EU countries. Specifically, significant employment reductions are concentrated in capital-intensive firms and small firms in leakage sectors, mainly in mining, cement, and basic metals. In leakage sectors, we further observe a significant ramping up of investments in large and capital-intensive firms and productivity improvements in small firms. In all subsamples, profitability and exit probabilities are hardly affected by carbon costs.

Keywords: Carbon costs, climate policy stringency, shadow price, firm performance, competitiveness, shift-share instrument

JEL codes: D22, H23, Q41, Q48, Q52, Q58

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1 Introduction

National and international targets for carbon emission reduction demand stringent policies that increase the costs of emitting carbon (IPCC, 2018; Stiglitz et al., 2017). From an economic perspective, carbon costs could be introduced through explicit carbon prices, i.e. a carbon tax or emissions trading system (ETS), or instruments that create an implicit price, such as fossil excise taxes, subsidies, standards, and restrictions.

A central concern among policymakers, however, is that increases in carbon costs would adversely affect economic activity and deteriorate competitiveness of domestic firms (Stiglitz, 2019; Gillingham and Stock, 2018). This mainly applies to industrial firms, as they tend to be energy- and trade-intensive and have limited abilities to pass through carbon costs to customers.[1] Another concern is carbon leakage: when carbon costs are raised unilaterally, firms might relocate to less-regulated jurisdictions. This would decrease the effectiveness of unilateral climate policies with respect to the goal of achieving global emission reductions. Whether or not these concerns are warranted remains an open question (Köppl and Schratzenstaller, 2022).

Evaluations of single carbon pricing policies find little evidence of adverse effects, but the results seem hard to generalize (Köppl and Schratzenstaller, 2022). No strong adverse effects are found for the EU ETS so far (Dechezleprêtre et al., 2023; Naegele and Zaklan, 2019; Verde, 2020) and for taxes like the UK's 2001 fossil fuel taxes, which corresponded to up to GBP 31 per tonne of CO_2 equivalent (tCO_2e) or a 15% rise in average energy costs (Martin et al., 2014a). However, these evaluations are often based on non-random and non-representative subsamples. Typically small and low-emitting firms close to the regulatory thresholds or firms from a single country are considered. Perhaps more importantly, explicit carbon prices have been persistently low and include a wide range of exemptions and compensation measures (Verde, 2020; Köppl and Schratzenstaller, 2022).[2] To date, about four-fifths of global carbon emissions are not explicitly priced (World Bank, 2023) and the global average carbon price was below USD 2.40/tCO_2e in 2020 (Dolphin et al., 2020). By contrast, *implicit* carbon prices, such as taxes levied on fossil energy, have been the main determinant of carbon costs (OECD, 2015; Carhart et al., 2022; Sen and Vollebergh, 2018). Given the current policy discussions on intensifying climate policies, it is important to examine potential effects using an encompassing measure of carbon costs, which exhibits more substantial variation and to ensure that results are widely applicable.

Model-based simulations of major carbon price shocks find relatively small production losses but quite serious carbon leakage effects (Carbone and Rivers, 2017). While these

[1] The issue of competitiveness is less important in other sectors due to a low exposure to international competition (transport and built environment) or ability to nearly fully pass through carbon costs to customers (electricity sector, see e.g. Fabra and Reguant (2014) and Hintermann (2016)).

[2] This complexity around carbon pricing poses a technical problem, which may explain the current lack of international empirical evaluations (Köppl and Schratzenstaller, 2022). We coin this the *'Swiss cheese problem'*: explicit carbon taxes tend to be limited to *slices* of the entire cheese (the total tax base of global carbon emissions), with typically low weight (tax rate), and they include many *holes*, i.e. exemptions or conditionalities. This makes it hard to judge the weight of the cheese, i.e. how heavy the effective cost burden is, and how this affects firm performance. Bottom-up calculations of effective carbon prices (Dolphin et al., 2020) necessarily abstract from this problem by focusing on specific slices and making various assumptions and shortcuts. Typically, the holes are neglected, implying upward biases in the estimates. Another way that many carbon pricing schemes resemble Swiss cheeses: they tend to get hollowed out more the more CO_2 is being released from within.

estimates provide useful policy insights, they represent aggregated long-run effects, which are surrounded by a great deal of uncertainty. Results strongly depend on model assumptions and may hide the 'pain' incurred in the short-run and for specific subsectors or individual firms. The lack of knowledge about such micro-level effects can be highly important to policy making. For instance, in the Netherlands, policymakers have been assigning a central role to anecdotal evidence from case studies and interviews in informing about micro-level effects of carbon pricing policy (PwC, 2019b,a, 2020, 2022).

This paper aims to provide policy-relevant evidence on the joint effects of carbon policies on firm performance around the world using granular microdata and shadow prices to capture the joint stringency of policies. For this reason, we combine two unique data sources. We use production data for a wide range of countries and sectors over time to construct shadow prices of fossil energy sources. These shadow prices provide an integral measure of the carbon costs implied by the mix of climate and energy policies, which allows for economically sound international comparisons. We link this measure to rich international microdata on industrial firms' production activity and performance to evaluate carbon cost impacts. This allows us to contribute to prior single-policy and single-country studies in two main ways.

Firstly, we answer the call for generalizable estimates of carbon cost effects from cross-country empirical analyses (Köppl and Schratzenstaller, 2022). This study provides the first comprehensive international evidence on the impact on industrial firm performance. We employ the largest global microdataset available, BvD Orbis Historical, which –combined with carbon cost data– has a substantial coverage in both the cross-sectional and time dimensions. It spans the years 2000–2019 and includes up to 3.1 million firms of different types (small and large, capital-intensive and capital-extensive, etc.), from all industrial sectors in a large set of countries. The 32 countries covered by our dataset represent 75% of global cumulative greenhouse gas emissions over the sample period, whereas the EU countries represent 10%. Using granular firm-level data on domestic production activity and performance, we are able to accurately identify treated firms and determine carbon cost effects. Moreover, the microdata facilitate isolating the effects of carbon costs from relevant (unobserved) confounding events and elaborately testing effect heterogeneity across firms. These are considered crucial elements in advancing the literature (Martin et al., 2014a; Marin and Vona, 2021; Dechezleprêtre et al., 2023).

Secondly, shadow prices are used to consistently measure the carbon costs implied by a broad scope of climate and energy policies. This is important because carbon costs stem from complex country- and sector-specific policy mixes and designs. The costs that policies impose on firms are not only determined by direct carbon taxes, but also – often predominantly – by other policies, such as fuel excise taxes, subsidies, emission standards, and technology restrictions. For instance, fuel excise taxes typically function as a carbon tax on fossil fuel consumption (Sen and Vollebergh, 2018); requirements to add a certain percentage of clean fuels to fossil sources also drive up costs; and Vollebergh and Van Der Werf (2014) illustrate the implied costs of emission standards. As such, carbon price signals can be provided explicitly and implicitly in a very similar manner.

Brunel and Levinson (2016) review the approaches to measuring environmental and climate policy stringency. They argue that an ideal measure for international analyses would capture policies in an integrated and consistent manner, but available measures mostly fall short of this ideal.[3] Shadow prices seem to come close to the ideal, as they provide a microeconomic-founded measure of private sector compliance costs which is consistent across countries, sectors, and over time (Van Soest et al., 2006). The shadow price of a dirty production input, such as coal, oil, or natural gas, represents the total cost for a profit-maximizing or cost-minimizing firm of using one additional unit of this input instead of other inputs while keeping output constant. As such, shadow prices facilitate a quantification of all policy-induced costs related to fossil energy inputs.[4] Thus, by using shadow prices, we can consistently evaluate the effects of increasing policy *stringency*. This complements the prior evidence on effects of firm *participation* in a carbon pricing scheme (Verde, 2020). The average estimated shadow prices more than doubled over the sample period and are far larger than estimates of explicit carbon prices (Dolphin et al., 2020). The relatively substantial levels and large variation in shadow prices represent a fruitful basis for testing the potential effects of increasingly ambitious climate policies.

Robust fixed effects instrumental variable estimations show little evidence of adverse performance effects of carbon cost increases for an average firm in competitiveness-prone industrial sectors, except for economically modest employment reductions. However, we find considerable effect heterogeneity. Performance effects tend to be most pronounced in carbon leakage sectors and EU countries. Specifically, significant employment reductions are concentrated in capital-intensive firms and small firms in leakage sectors, mainly in mining, cement, and basic metals. In leakage sectors, we further observe a significant rise of investment expenditures in large and capital-intensive firms and productivity improvements in small firms. In all subsamples, profitability and exit probabilities are hardly influenced by carbon cost increases. Our results from international microdata and an integrated carbon cost measure corroborate the prior evidence of limited adverse effects for the EU ETS (Dechezleprêtre et al., 2023) and energy price increases (Marin and Vona, 2021).

The rest of the paper is organized as follows: Section 2 describes the methodology and the data. The baseline results are presented and discussed in Section 3.1. Section 3.2 examines longer-term effects and effect heterogeneity is examined in Section 3.3. Section 4 concludes.

[3] A large stream of literature proxies policy stringency by country-wide environmental policy indices, counts of environmental laws and policies, public spending, or emissions and energy use (Brunel and Levinson, 2016; Dechezleprêtre and Sato, 2017). These measures often do not focus on carbon costs. Moreover, these measures are typically matched with readily available data on listed firms, which implies that policy exposure is not identified. Exceptions are Dechezleprêtre et al. (2022) and Trinks et al. (2022b), who use survey data to identify policy exposure of listed multinationals. A limitation of the latter studies is the low external validity, given that listed firms represent only a tiny fraction (roughly 1%) of firms globally (Gopinath et al., 2017) and the limited availability of comprehensive and granular country-by-country reporting about production activity and emissions.

[4] This sets our approach apart from recent carbon price estimates, which serve a different purpose. Carhart et al. (2022) calculate country-level 'comprehensive carbon prices' for the highest-emitting countries to track countries' progress towards carbon pricing. Dolphin et al. (2020) quantify explicit carbon prices at the IPCC activity level to map the instruments that explicitly target carbon emissions. Alternatively, energy prices have also been used directly to evaluate the effects of climate policy stringency on firm performance or industrial competitiveness (Aldy and Pizer, 2015; Marin and Vona, 2021).

2 Methods and materials

2.1 Baseline model

The impact of carbon costs on firm performance outcomes is estimated using fixed effects instrumental variable (FE-IV) panel regression models. The fixed effects estimator exploits the within-firm variation to eliminate biases stemming from unobserved constant differences across firms. The models additionally control for time-varying factors that are well-established performance drivers and for shocks common to all industrial firms, like global financial or energy crises. As such, the aim is to come as close as possible to a causal interpretation of the effects. Note in this respect that the integral measure of carbon costs circumvents major omitted variable biases that could occur in analyses of individual or partial policy measures. Equation (1) presents the baseline model:

$$Firm\ performance_{isct} = \beta Carbon\ costs_{sct} + Controls'_{isct}\gamma + \alpha_i + \delta_t + \epsilon_{isct} \quad (1)$$

where $Firm\ performance$ is the outcome variable of firm i in sector s, country c, and time (year) t; $Carbon\ costs$ is the shadow price measure[5] that shows variation at the sector-country-time level and for which an IV is used to correct for simultaneity (Section 2.3); β is the coefficient of interest; $Controls$ is a vector of time-varying control variables; α_i and δ_t capture firm- and time fixed effects;[6] and ϵ is the error term. Errors are two-way clustered at the sector and country level to allow for unmeasured correlation between firms within the same sector or country and over time. This is to avoid false precision in the estimates of the independent variable of interest, carbon costs, which exhibits variation at the sector-country-year level.[7] The model specification is assessed in Table SM.1 and Table A.1 lists the definition and data source for all variables.

We examine six measures of *firm performance*, which together provide a detailed view on carbon cost effects. The measures reflect typical policy concerns, namely potential production losses, tax base erosion, and relocation. We follow the literature in the choice of the variables and their operationalization. The first three outcome variables primarily capture the responses in firms' production structure, whereas the last three variables

[5]In Eq. (1), we use the shadow prices themselves rather than shadow prices net of market prices for three reasons: (1) the estimation of net shadow prices requires a minimum number of degrees of freedom, which reduces the time variation (Althammer and Hille, 2016; Van Soest et al., 2006), (2) our FE-IV panel regression model controls for price differences across sectors and countries, and (3) Sato et al. (2019) show that energy price variation over time predominantly stems from regulatory differences rather than differences in wholesale prices across sectors and countries.

[6]Note that, since the dataset records firms by sector and country over the full sample period, the firm fixed effects absorb sector- and county-specific time-invariant factors. We test models that additionally include interacted sector-time fixed effects, which eliminate any potential influence of subsector-specific variation over time. We find qualitatively similar results, but the significance level of the employment effect decreases (see Table SM.1). We further examine models with sector and country fixed effects instead of firm fixed effects. These models leverage the cross-sectional variation in our dataset but at the expense of biases stemming from unobserved time-invariant differences between firms across countries and sectors.

[7]This follows current best practices of robust inference. For our case of few unequally sized clusters, the Wild cluster bootstrap is recommended (Cameron et al., 2008; Djogbenou et al., 2019; MacKinnon et al., 2023) and increasingly being used in the environmental economics literature (Barron and Torero, 2017; Isaksen et al., 2019). Specifically, we apply the Wild Restricted Efficient bootstrap (WRE) (Davidson and MacKinnon, 2010; Roodman et al., 2019) with 999 replications. Note that the bootstrapped confidence intervals need not be symmetric around the coefficient estimate (Roodman et al., 2019). We tested if statistical inference changes when using alternative error clustering regimes (Table SM.2).

reflect firms' economic performance. First, we investigate *sales* to assess how the total production value changes due to higher carbon costs. We further examine net *investment*, measured as the change in tangible fixed assets, which is a proxy for future growth and provides indications about firms' locational decisions and carbon leakage (Aus dem Moore et al., 2019). *Employment* effects are examined using data on the number of employees. With regard to economic performance, we first investigate *productivity*, operationalized as total factor productivity (TFP) based on the method by Ackerberg et al. (2015). This method has become increasingly a standard in the literature as it accounts for endogenous factor inputs and solves functional dependence problems of prior TFP measurement approaches.[8] We further examine a widely used measure of financial performance, namely *profitability*, measured as return on assets. Lastly, we test effects on firms' probability of *exit*. This variable is operationalized as a binary variable which equals one in the years after the firm has stopped reporting financial information, following Martin et al. (2014a) and Marin and Vona (2021).[9] Note that while this indicator is not informative about the exact reason for exit (firm relocation, bankruptcy, etc.), it precisely reflects policymakers' interest into the risk of firms ceasing their economic activity – for whatever reason.[10] The robustness to alternative specifications of the outcome variables is tested in Table SM.3.

The baseline model (Eq. (1)) includes a set of *controls* to isolate carbon cost effects. We follow the financial economics literature, which documents the theoretical importance of the factors *size* (log total assets), *leverage* (ratio of total debt to total assets) (Margolis et al., 2009; Trinks et al., 2022a; Waddock and Graves, 1997), and country-level factors of which the main time-varying factor is *GDP per capita*. Performance outcomes, like profitability and market value, are typically higher in larger and less-leveraged firms. Larger firms also tend to face less financial constraints (Bartram et al., 2022). In addition, size and leverage are associated with firm sustainability efforts and carbon emissions (Chava, 2014; Galama and Scholtens, 2021; Trinks et al., 2020). Variation in countries' income levels can affect the performance of incumbent firms. In robustness analyses in Table SM.1, we tested alternative specifications by excluding controls or including more controls to assess potential bad control problems. Our estimates are qualitatively similar. When controls are excluded, effects tend to be lower, yet not significantly. This provides an indication that potential bad control issues are minor and, if at all, would generally have biased our predominantly insignificant baseline estimates upwards.

2.2 Microdata on firm performance

We employ the largest global microdataset available, Bureau van Dijk (BvD) Orbis Historical, which includes harmonized production and financial information on the population of all public and private firm accounts worldwide (Gopinath et al., 2017; Kalemli-Özcan et al., 2022). Key advantages of these data are the extensive coverage over time and firms, the high match rate with the shadow price data, and the ability

[8] The specification of output and inputs (see Table A.1) closely follows a.o. Kalemli-Özcan et al. (2022). The sensitivity of the effect on TFP to alternative input specifications is assessed in Table SM.3.

[9] For employment and exit, Eq. (1) is estimated using the linear probability model. This is because of technical reasons and the strictly positive counts, but also to be in line with related literature (Commins et al., 2011; Dechezleprêtre et al., 2023; Marin and Vona, 2021) and to aid the interpretation of the coefficients. We tested that IV Poisson and IV probit models yield qualitatively similar results.

[10] Other relocation indicators used in prior sector-level studies are trade measures, such as net imports (Aldy and Pizer, 2015; Hille, 2018). The lack of consistent firm-level data, however, prevents us from examining these indicators.

to accurately identify treated firms. The latter is important because impact evaluations of national policies on firm performance are complicated by the fact that firm-level accounts may span activities in multiple countries or sectors. The Orbis Historical dataset includes rich information on both consolidated and unconsolidated accounts. The consolidated accounts reflect the situation of parent firms and integrate the statements of the controlled subsidiaries that may be located in different countries and sectors. The unconsolidated accounts report information about individual firms, which represent local subsidiaries in the case of multinationals. By using unconsolidated information, we are able to accurately identify the specific sector and country where the production activities take place and, therefore, where the climate- and energy-policy induced carbon costs apply (Gopinath et al., 2017). Hence, we ensure identification of treatment by linking country-specific, sectoral carbon cost data to microdata on individual firms.[11] As such, our sample includes information on the large majority of firm-level accounts from both domestic and multinational firms, i.e the used unconsolidated accounts represent 98.8% of all data entries (see Table A.2).

The final sample containing microdata and shadow price data has around 22 million firm-year observations, covering 3.1 million industrial firms from 32 countries and 15 sectors (Table 1). The number of observations in the regression analyses is somewhat lower and depends on the availability of the outcome variable considered. The sample period runs from 2000–2014 for the baseline analysis, which looks into contemporaneous effects, and 2000–2019 for the analyses on longer-run effects of carbon costs. Features of the dataset are described in more detail in Online Appendix A, where Section A.1 describes the dataset, Table A.1 lists the definitions and sources of the variables, Table A.2 describes the data cleaning, and Table A.3 shows the data coverage.

2.3 Shadow prices as an integrated measure of carbon costs

2.3.1 Background

This paper's aim is to evaluate how industrial firm performance is affected by the total policy-induced carbon costs. To allow for an integrated measure of carbon costs, we employ the shadow price approach, which has become a valued approach to measuring the stringency of pricing and non-pricing policies in a consistent manner (Hille and Shahbaz, 2019; Parry, 2020; Van Soest et al., 2006). Especially in a competitiveness context, shadow prices have been seen as the preferred measure of the relative stringency of environmental and climate policies (Jaffe et al., 2002; Kneller and Manderson, 2012).

Shadow prices are attractive for several reasons. First, they explicitly address the multidimensionality of climate policy by capturing all policies that either explicitly or implicitly impose costs on the use of carbon-intensive energy inputs (Althammer and Hille, 2016; Brunel and Levinson, 2016). This includes direct carbon taxes as well as other policies, such as fuel excise taxes, emission standards, and technology restrictions. Shadow prices are high where such policies exist and are stringent, and they are low where there

[11] Ideally, one would like to identify stringency at the firm level. This would be possible, for instance, by using firm-level energy cost data, which are sometimes available through national surveys (Marin and Vona, 2021). Unfortunately, firms around the world are generally not obliged to report energy consumption and expenditures data nor is the reporting internationally standardized and sufficiently granular. This limits the applicability of such a stringency measure in comprehensive cross-country samples like ours.

are policies that stimulate fossil energy use, such as subsidies or tax exemptions, in which case profit-maximizing firms have incentives to strongly rely on carbon-intensive energy inputs. Second, by capturing general equilibrium effects and controlling for industrial composition, shadow prices measure the regulatory stringency that firms actually faced. Third, shadow prices have a strong basis in microeconomic theory, modeling policy effects on prices using cost or profit functions. This consistent economic approach to measuring carbon costs facilitates international comparisons between countries, sectors, and over time (Van Soest et al., 2006). Fourth, shadow prices can be derived with the help of production data, which are widely available at a granular level. This contributes to a high external validity of the analysis.

Limitations of the shadow price approach are the reliance on the simplifying assumptions of uniformly applicable cost functions (Brunel and Levinson, 2016) and constant production technology (Van Soest et al., 2006). The encompassing nature of shadow prices of fossil energy implies that shadow prices cannot be interpreted as costs solely relating to climate policies, as they can be influenced by policies in other domains if these affect fossil input decisions (Brunel and Levinson, 2016). Moreover, while shadow prices are common to economists, interpreting them may not be straightforward for policymakers. To facilitate interpretation, we provide an order-of-magnitude indication in USD/tCO_2e terms.

2.3.2 Methodology

To derive shadow prices of carbon-related energy use, we build on the methodologies advanced by the environmental economics literature (Althammer and Hille, 2016; Van Soest et al., 2006). Specifically, we estimate a system of equations based on a cost function approach, and then use the determined coefficients for quantifying the sector-specific shadow prices.[12] Data on quantities and prices of production inputs and outputs per sector, country, and year are sourced from the World Input-Output Database (WIOD), energy price data are from Sato et al. (2019) and the IEA, and additional deflators are from the Penn World Table. We distinguish 15 sector groups based on the ISIC Rev. 4 classification system.

In detail, following the spirit of previous structural modeling, we estimate a Generalized Leontief variable cost function (Althammer and Hille, 2016; Morrison and Schwartz, 1996), assume long-run constant returns to scale (Morrison, 1988), and specify time trends for the shadow prices only (Van Soest et al., 2006).[13] We consider three factors of production, i.e. the quasi-fixed capital input K, the variable input labor L, and the polluting input carbon-related energy E. The resulting variable cost function C reads as follows:

$$\begin{aligned} C_{sct} = y_{sct} &\left[\alpha_{LL} p_{L,sct} + \alpha_{LE} p_{L,sct}^{0.5} Z_{E,sct}^{0.5} + \alpha_{EE} Z_{E,sct}\right] \\ &+ p_{L,sct}\left[\alpha_{KK} x_{K,sct} + \alpha_{LK} x_{K,sct}^{0.5} y_{sct}^{0.5}\right] + Z_{E,sct}\left[\alpha_{KK} x_{K,sct} + \alpha_{EK} x_{K,sct}^{0.5} y_{sct}^{0.5}\right] \end{aligned} \quad (2)$$

[12] An alternative approach to estimating shadow prices is using output distance functions and duality theory (Färe et al., 1993; Zhou et al., 2014). However, we follow common practice and use a parametric model that allows for a convenient derivation of shadow prices. While this modeling requires (weak) structural assumptions, it is immune to some of the problems faced by non-parametric alternatives, such as inconsistencies across different choices of the direction vector and extreme shadow price values.

[13] Although the assumptions follow the state-of-the-art literature, carbon costs may be underestimated in case of strongly positive technology shocks (Van Soest et al., 2006).

where y is output in sector s, country c, and time (year) t. x_K is the capital stock, and p_L as well as Z_E are the price of labor and shadow price of carbon-related energy, respectively.

In this approach, the polluting input is treated as a variable input and, hence, its shadow price can be nested directly in the cost function (Morrison-Paul and MacDonald, 2003; Van Soest et al., 2006). This allows the shadow price Z_E of an additional unit of carbon-related energy to deviate from the undistorted market price p_E due to direct and indirect climate regulations that affect the emission-relevant energy costs (Althammer and Hille, 2016):

$$Z_{E,sct} = \alpha_E p_{E,st} + \lambda_{E,sct} D_{sct} \tag{3}$$

In this so-called shadow price equation, λ_E is the wedge, i.e. the difference between the market price and the shadow price, that varies by sector, country, and time. D consequently is a sector-, country-, and time-specific binary variable.[14]

In order to facilitate identification of the various coefficients, we construct factor demand functions of the two variable inputs using Shephard's lemma and normalize the input demand by dividing the functions by the output:

$$\frac{x_{L,sct}}{y_{sct}} = \frac{1}{y_{sct}} \frac{\partial C_{sct}(p_{L,sct}, Z_{E,sct}, x_{K,sct}, y_{sct})}{\partial p_{L,sct}} \tag{4}$$

and

$$\frac{x_{E,sct}}{y_{sct}} = \frac{1}{y_{sct}} \frac{\partial C_{sct}(p_{L,sct}, Z_{E,sct}, x_{K,sct}, y_{sct})}{\partial Z_{E,sct}} \tag{5}$$

As there are common coefficients across the system of equations (3) to (5), it is estimated simultaneously using seemingly unrelated regressions (SUR). As a last step, we use the estimated coefficients to quantify the shadow prices as the measure of carbon costs.

2.3.3 Endogeneity and instrumental variable approach

When testing the effect of carbon costs on firm performance, simultaneity poses a potential concern: shadow prices of fossil energy might affect firms' fuel choices, which in turn changes the energy mix and thus influence the shadow prices. The recent energy price literature has increasingly controlled for such endogeneity sources using shift-share instruments (Linn, 2008; Marin and Vona, 2019, 2021). The idea is that these instruments address the

[14] In equations (2) and (3), the αs and λ_E are the respective regression coefficients. To preserve degrees of freedom, the interaction effect coefficients (α_{EK}, α_{LE}, α_{LK}) and the fixed variable's coefficient (α_{KK}) of each sector are set common across countries, whereas the corresponding variable inputs' direct coefficients (α_{EE}, α_{LL}) are allowed to differ across countries (Morrison, 1988). The wedge λ_E and D are structured in 5 equivalent 3-year periods, in order to account for the limited number of degrees of freedom (Hille, 2018). We find similar results when recalculating shadow prices with price wedges structured in 2- instead of 3-year periods (Table SM.3). Moreover, following prior research, p_E is proxied by the sector- and time-specific sample average (Althammer and Hille, 2016; Van Soest et al., 2006). To reflect rigidities in international energy markets, the corresponding coefficient α_E is not set equal to 1 but determined by the model, so that the estimated undistorted market price can differ from the observed average market price.

issue of fuel choice endogeneity by using fixed-weight energy prices. For identification, the instruments require exogenous shares and exogenous shocks. To address the former, the share of the different energy sources in the entity's fuel mix is fixed over time to the share of a pre-sample base year. For the latter, the fixed fuel shares are interacted with fuel prices that are at a more aggregated level, such as the national level, because aggregated energy prices tend to be less correlated with omitted firm- or plant-level variables.

In the spirit of this literature, we estimate an instrumental variable for the shadow prices. That is, we use the fixed-weight energy price index from Sato et al. (2019) as an input in the shadow price estimation instead of variable-weight energy prices, and modify the system of equations (3) to (5) accordingly. In other words, we replace the potentially endogenous energy input variable by its shift-share instrument. Then, we estimate the system again and construct the instrument for the shadow prices from the resulting coefficient estimates. As can be seen in the results (Table 2), the instrument performs well in terms of the usual specification tests. In Table SM.1 we consider using a simple OLS strategy. The difference with the baseline FE-IV estimates, mainly for employment, underscores the relevance of controlling for simultaneity.

2.4 Descriptive statistics and illustration of the shadow prices

Table 1 provides summary statistics for the considered merged sample of microdata and shadow price data. The shadow price measure shows a substantial cross-sectional variation, which implies that carbon costs differ considerably across countries and sectors. There is also a sizeable temporal variation in the shadow prices, i.e. on average they rose by 102.1% over the sample period. This variation, which is exploited when estimating the baseline model (Eq. (1)), represents about half of the variation observed between countries and sectors. The large temporal variation is consistent with prior analyses on carbon costs (Althammer and Hille, 2016; Carhart et al., 2022) or energy prices (Marin and Vona, 2021).

Note that the shadow prices of fossil energy sources are expressed in terms of energy content (ton of oil equivalent (toe)). To facilitate interpretation and get an order-of-magnitude indication, the USD/toe amounts can be divided by a sample average emission factor of 2.89 tCO_2e/toe. In our sample, the average shadow price of fossil fuels net of their market price corresponds to USD 6.71/tCO_2e when weighted by emissions and USD 49.06/tCO_2e when weighted by value added. The difference reveals that carbon costs tend to be low in the (few) sectors and countries with high carbon intensities but relatively low economic stakes; in contrast, relatively high carbon costs are observed in the remaining cleaner sectors and countries.

Our carbon cost estimates are in line with country-level estimates of 'comprehensive carbon prices' by Carhart et al. (2022) (emission-weighted mean of around USD_{2010} 7/tCO_2e in the period 2008–2014) and considerably higher than explicit carbon prices from Dolphin et al. (2020) (emission-weighted mean of around USD_{2010} 0.40/tCO_2e in 2000–2014). Differences likely stem from the encompassing nature of shadow prices and the broader sample coverage. The finding that carbon costs exceed explicit carbon prices by more than a factor of 15 underlines the importance of our integral measure of carbon costs; apparently, policies other than explicit carbon pricing have been predominant in determining carbon cost signals. The focus of Carhart et al. (2022) on high-emitting countries creates a selection bias to the extent that high emission levels are driven by low carbon costs. Moreover, country-average carbon prices may exceed those for industrial sectors due to the typically lower policy stringency in the latter.

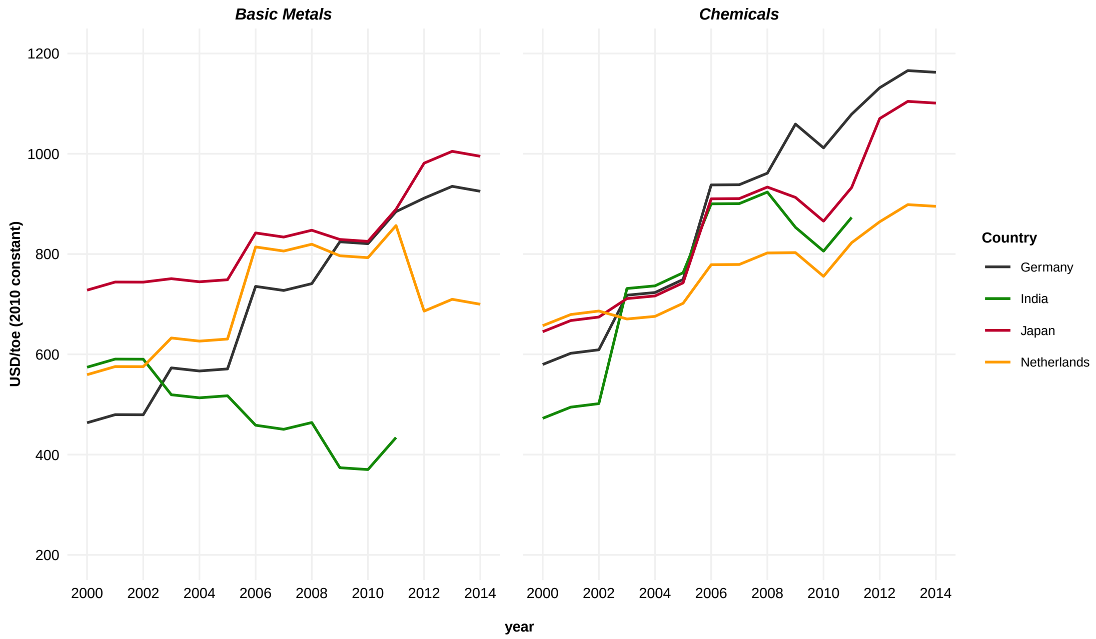
Table 1: Summary statistics

Variable	unit	mean	sd	p25	p50	p75	N
Dependent variables							
Sales	log USD_{2010}	13.57	2.33	12.25	13.65	15.04	15,214,030
Investment	log USD_{2010}	0.00	0.68	-0.27	-0.07	0.14	14,540,461
Employment	log(count)	2.35	1.68	1.10	2.20	3.47	10,990,520
Productivity	unit	3.14	0.95	2.56	3.12	3.66	4,917,344
Profitability	unit (*100%)	1.79	29.55	-0.95	1.97	8.95	16,035,946
Exit	binary (*100%)	0.50	7.04	0.00	0.00	0.00	21,925,981
Control variables							
Size	log USD_{2010}	12.81	2.71	11.50	13.04	14.47	21,593,015
Leverage	unit (*100%)	84.38	117.88	42.76	71.14	92.70	21,217,662
GDP per capita	log USD_{2010}	10.05	0.88	9.64	10.47	10.65	21,979,754
Carbon costs							
Shadow price	USD_{2010}/toe	782.10	350.22	552.67	713.68	918.09	19,617,792
Instrument for shadow price	USD_{2010}/toe	664.67	264.39	490.87	640.35	794.68	19,658,308
Variation in shadow price							
Total variation	USD_{2010}/toe	782.10	350.22				N = 19,617,792
Cross-sectional	USD_{2010}/toe		329.35				n = 3,075,012
Time	USD_{2010}/toe		143.52				T-bar = 6.38
Interpretation of net carbon costs in USD_{2010}/CO_2e							
Mean emission-weighted		6.71					
Mean value added-weighted		49.06					

Presented are the summary statistics for the merged sample of microdata and shadow price data. To ensure consistency with the regression analyses, this only includes countries and sectors with non-missing shadow price data. The microdata cover the period 2000–2019 and the shadow price data 2000–2014. Shown in columns are the mean, standard deviation (sd), 25^{th} percentile (p25), median (p50), 75^{th} percentile (p75) and number of observations (N), respectively. N divided by the number of firms, n, yields the average number of observations per firm, T-bar. Net carbon costs are defined as the shadow price of fossil fuels net of the market price. The translation into USD per CO_2e terms is done by multiplying the sector-, country-, and year-specific fuel mix from WIOD by the global average conversion factor of the main fossil energy sources (coal, oil, and gas) from EIA (link). Mean net carbon costs are calculated by weighting the carbon costs of observations by their share in the sample's total carbon emissions or value added.

Shadow prices are visualized in Figure 1 for two major industrial sectors and a selection of countries. Shadow prices tend to be relatively high in European countries and low in developing countries like India but also in the US, Canada, and Russia. This is intuitive since in the latter countries, energy taxes are close to zero for industrial users and there is no explicit country-wide carbon price. Nevertheless, in most countries, shadow prices have risen over time. This is in line with the 'greening' of the fiscal system in many countries (OECD, 2015). It also follows the introduction of the EU ETS in 2005, which put an explicit price on carbon for many industrial firms. Another observation is that sectoral differences in shadow prices vary across countries. For instance, shadow prices differ more widely across countries in the basic metals sector than in the chemicals sector. In some cases, such as the basic metals sector in India, shadow prices are close to or even below the fossil input's market price. This is an indication that, in such cases, fossil energy inputs face little to no energy taxes in combination with exemptions and subsidies.

Figure 1: Shadow prices for a selection of sectors and countries



Source: Authors' calculations, following Althammer and Hille (2016) and using energy price data from Sato et al. (2019).

3 Results and discussion

3.1 Baseline results

The baseline estimates in Table 2 show little evidence of adverse performance effects of carbon costs for industrial firms overall.[15] Only employment is significantly negatively affected by carbon costs. This effect differs from the EU ETS literature but is consistent with estimates from several energy prices studies, for carbon- and trade-intensive industries, and for lower-income countries (Hille and Möbius, 2019; Marin and Vona, 2021; Yamazaki,

[15] Note that the coefficient estimates for carbon costs (β) represent elasticities in models (1)–(3) and semi-elasticities in models (4)–(6). That is, the estimated effects of a 1% increase in carbon costs are a β% change in sales, investment, or employment and a $\beta/100$ unit change in productivity, profitability, or exit. Signs of control variables are as expected: most outcomes relate positively to size; profitability relates negatively to leverage; productivity tends to rise when countries become richer.

2017).[16] For other performance outcomes, the effects are statistically indistinguishable from zero. Nevertheless, the effect signs are as expected. Carbon cost increases are associated with lower sales revenues. Sales reductions would point to reduced production activity or imperfect carbon cost pass-through. By comparison, the competitiveness literature has sketched a mixed picture on sales: some studies found small negative effects (Aldy and Pizer, 2015), while the EU ETS seemed to have partly increased firms' sales revenues (Dechezleprêtre et al., 2023). We estimate a positive investment effect. This is qualitatively in line with the ETS literature, which points to carbon abatement responses to rising carbon costs (Aus dem Moore et al., 2019; Marin et al., 2018). The positive estimate for productivity corresponds qualitatively to the Porter hypothesis: firms may have responded to carbon cost increases by reducing fossil energy inputs, thereby limiting their performance sensitivity to carbon costs (Albrizio et al., 2017; Trinks et al., 2020). The estimate for profitability is positive but small and insignificant, in line with the ETS literature. The small and insignificant coefficient for firm exit probability is similar to the effects found for the EU ETS (Dechezleprêtre et al., 2023) and the UK's 2001 fossil fuel taxes (Martin et al., 2014a).

Table 2: Carbon costs and industrial firm performance

Model: Outcome: Scale:	(1) Sales *log*	(2) Investment *log*	(3) Employment *log*	(4) Productivity *level*	(5) Profitability *level*	(6) Exit *level*
Carbon costs (*log*)	-0.109	0.123	-0.132**	0.256	0.012	0.012
	(-0.854)	(1.774)	(-4.835)	(1.264)	(0.383)	(1.333)
Size (*log*)	0.881***	0.179***	0.263***	-0.064	0.051***	-0.001
	(17.035)	(13.558)	(7.576)	(-1.284)	(8.155)	(-1.462)
Leverage (*level*)	0.064	-0.005	0.035	-0.075***	-0.113***	0.000
	(2.731)	(-0.968)	(2.840)	(-4.488)	(-20.326)	(1.698)
GDP per capita	-0.111	0.071	0.148	0.133*	-0.025	0.000
(*log*)	(-0.814)	(0.713)	(0.944)	(5.603)	(-1.421)	(1.757)
N (observations)	12,391,027	13,234,333	8,969,509	4,741,493	13,998,001	19,561,350
n (firms)	1,877,230	1,899,328	1,512,646	771,327	2,023,667	3,069,896
G (clusters G_c, G_s)	32, 15	31, 15	32, 15	24, 15	32, 15	32, 15
F statistic excluded IV	149.249	309.291	106.740	43.736	224.821	16.661

FE-IV estimates per Eq. (1) (2000–2014). Firm- and year-fixed effects included in Models (1)-(5). Model (6) includes sector-, country-, and year-fixed effects and the controls are specified as the pre-exit firm average. In parentheses are Wild bootstrapped t-values robust to clustering at both the sector and country level (see Section 2.1). F statistic of excluded IV shows the Kleibergen-Paap Wald F statistic for assessing weak identification. The IV is the shift-share instrument for the shadow price, estimated using fixed-weight energy prices (see Section 2.3.3). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

[16] Worth noting is that we find indications of insignificant labor market effects in the robustness tests, for instance when average wages are considered as an alternative proxy (Table SM.3). This particular difference between the employment and wage effect is analogous to Marin and Vona (2021). The insignificant wage effect may be explained by potentially opposing effects of carbon costs, i.e. transitional costs for workers (Walker, 2013) and skill-biased changes in labor demand (Marin and Vona, 2019).

Economically, carbon cost effects seem modest. The employment elasticity of -0.132 is comparable to that for energy prices in France of -0.081 (Marin and Vona, 2021). To illustrate effect sizes, we consider a hypothetical increase in carbon costs by USD 50/tCO_2e. An order-of-magnitude indication for the employment effect is -2.4% and for the insignificant performance indicators: sales revenue -2.0%, investment +2.3%, productivity +0.04 (+0.9% for the average firm), profitability +0.2% points, and exit probability +0.2%.[17] The performance effects can be compared with the environmental benefits expected from increased carbon costs. Recent projections for national carbon taxes of USD 50/tCO_2e see national carbon emission reductions in the order of 10% to 20% relative to a current policy baseline in 2030 (Bollen et al., 2020; Marin and Vona, 2021). This highlights the trade-off between burdens for firms in certain sectors and environmental benefits. It should be noted that the economic effects can be highly local, given that industrial production is typically concentrated geographically and in a relatively small number of firms, whereas environmental benefits may be more global and therefore less tangible for policymakers.

3.2 Longer-term effects

The baseline model examines immediate effects of carbon costs on firm performance. But effects on some performance outcomes might be non-persistent or delayed. For instance, investment in emission abatement might happen unevenly and, once realized, could undo negative performance effects in future time periods. We therefore examine longer-term effects of carbon costs. We include the 1- up to 5-year lagged carbon costs in Eq. (1) one by one and take the respective lag of the instrumental variable.[18] The ability to study longer-term effects in an international setting is a unique advantage of our dataset, due to its comprehensive coverage in both the cross sectional dimension (up to 3.1 million firms) and the time dimension (2000–2019). Results are visualized in Figure 2, while full results are reported in Table SM.5. Shown are the effect estimates and 95% Wild cluster bootstrapped confidence intervals, which, as mentioned before, need not be symmetrically distributed around the coefficient estimate.

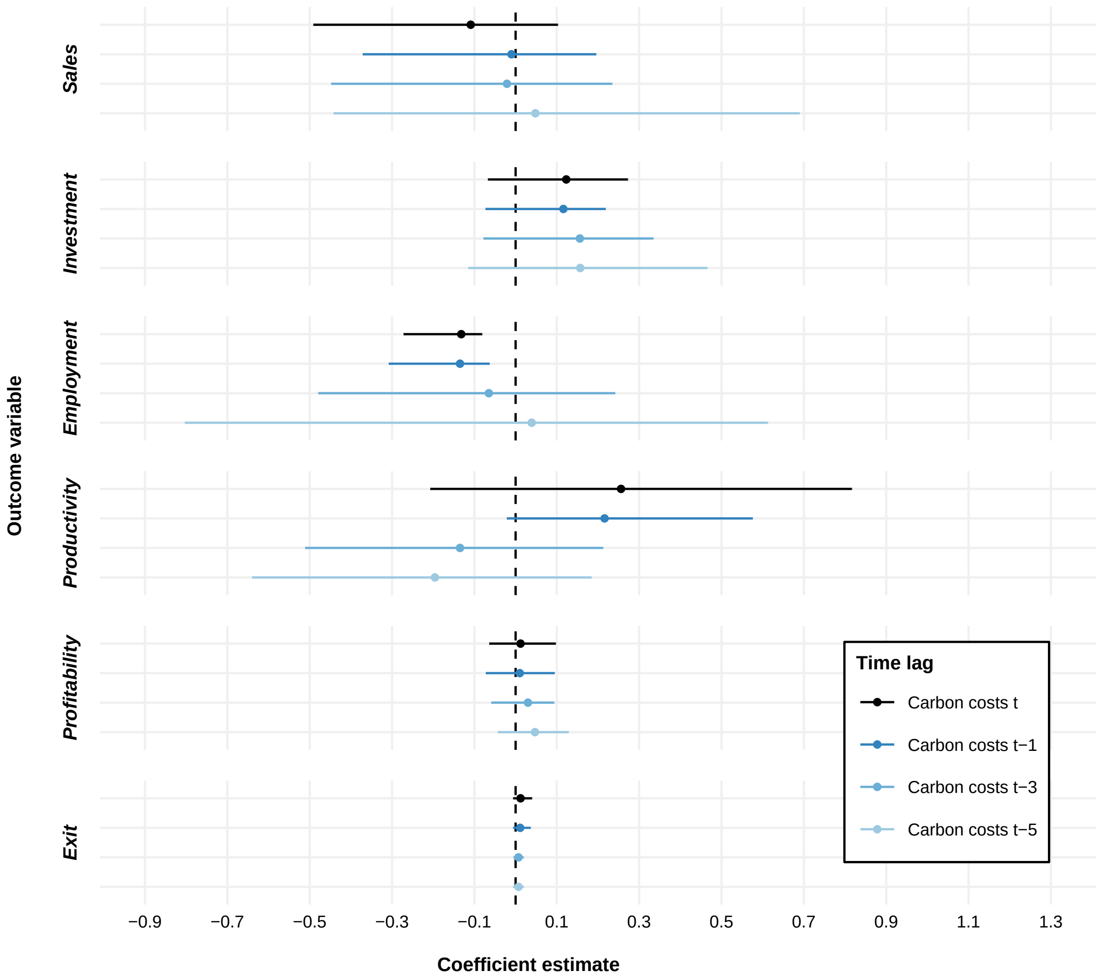
The results suggest that carbon costs affected firm performance both contemporaneously and in the longer-run. Despite considerable heterogeneity in firm responses, a visual inspection indicates that the contemporaneous influences tend to persist over a longer time period, particularly for investments and employment, and to a lesser extent for productivity. In further analysis we find that longer-term *cumulative* effects on employment are significant up to 4 years into the future and that the effect size exceeds the contemporaneous effect by a factor of 2.4 (Table SM.6). Similar evidence has been found for energy prices in France (Marin and Vona, 2021). Although mostly insignificant for an average firm, we estimate consistently positive effects for investment over a longer time period. This may indicate that not only adjustments in investment behavior happen relatively quickly, but

[17] Assuming a multiple of 2.89 to translate shadow prices into USD/tCO_2e terms, a 1% increase in the shadow price corresponds to USD 2.71/tCO_2e ((1% * USD 782.20/toe) / 2.89). Hence, coefficients are multiplied by 18.48 (USD 50 / 2.71) to simulate effects of a USD 50/tCO_2e increase in carbon costs.

[18] Evaluating effects of individual lags is preferred over evaluating all lags simultaneously, to avoid a substantial and potentially severe survivorship bias and to make maximum use of the extensive time coverage of the microdata (2000–2019). We find similar results when testing for long-term cumulative effects (Table SM.6), which are based on up to twice as few observations and long-surviving firms only.

also firms see the need to keep the investment level high for a longer period to cope with rising carbon costs. Combined with significant positive long-term investment effects of the EU ETS (Aus dem Moore et al., 2019; Dechezleprêtre et al., 2023), this suggests that firms reacted to rising carbon costs largely by investing in carbon emission abatement. For productivity, we find positive coefficients for few separate lags of carbon costs, but growing cumulative long-run effects. This difference may be explained by the focus in the latter analysis on longer-surviving firms, which may be more productive, yet acknowledging that the effects are significant at the 10% level only.

Figure 2: Baseline effects and lagged effects of carbon costs on performance outcomes



Dot indicates coefficient of carbon costs from baseline model (Eq. (1)), which can be interpreted as an elasticity for Sales, Investment, and Employment and semi-elasticity for Productivity, Profitability, and Exit; Line indicates 95% confidence interval based on Wild bootstrapped errors robust to clustering at the sector and country level. Note that estimates and confidence intervals for Exit are small but insignificant.

3.3 Heterogeneity analyses

The large dispersion in our baseline estimates may stem from effect heterogeneity across groups in our sample. We therefore conduct a range of subgroup analyses.

Adverse effects of carbon costs may be particularly pronounced in carbon leakage sectors (Marin and Vona, 2021; Yamazaki, 2017). To investigate this, we estimate Eq. (1) separately for firms belonging to NACE 4-digit sectors deemed at risk of carbon leakage by the European Commission (EC) (link) and those that do not. We take the EC list of leakage sectors because it provides a refined and specific indicator of leakage risk, which is based on certain thresholds for carbon or trade intensity, and to follow a related literature (Dechezleprêtre et al., 2022; Koch and Basse Mama, 2019). Similarly, we also examine effects for individual industrial subsectors.

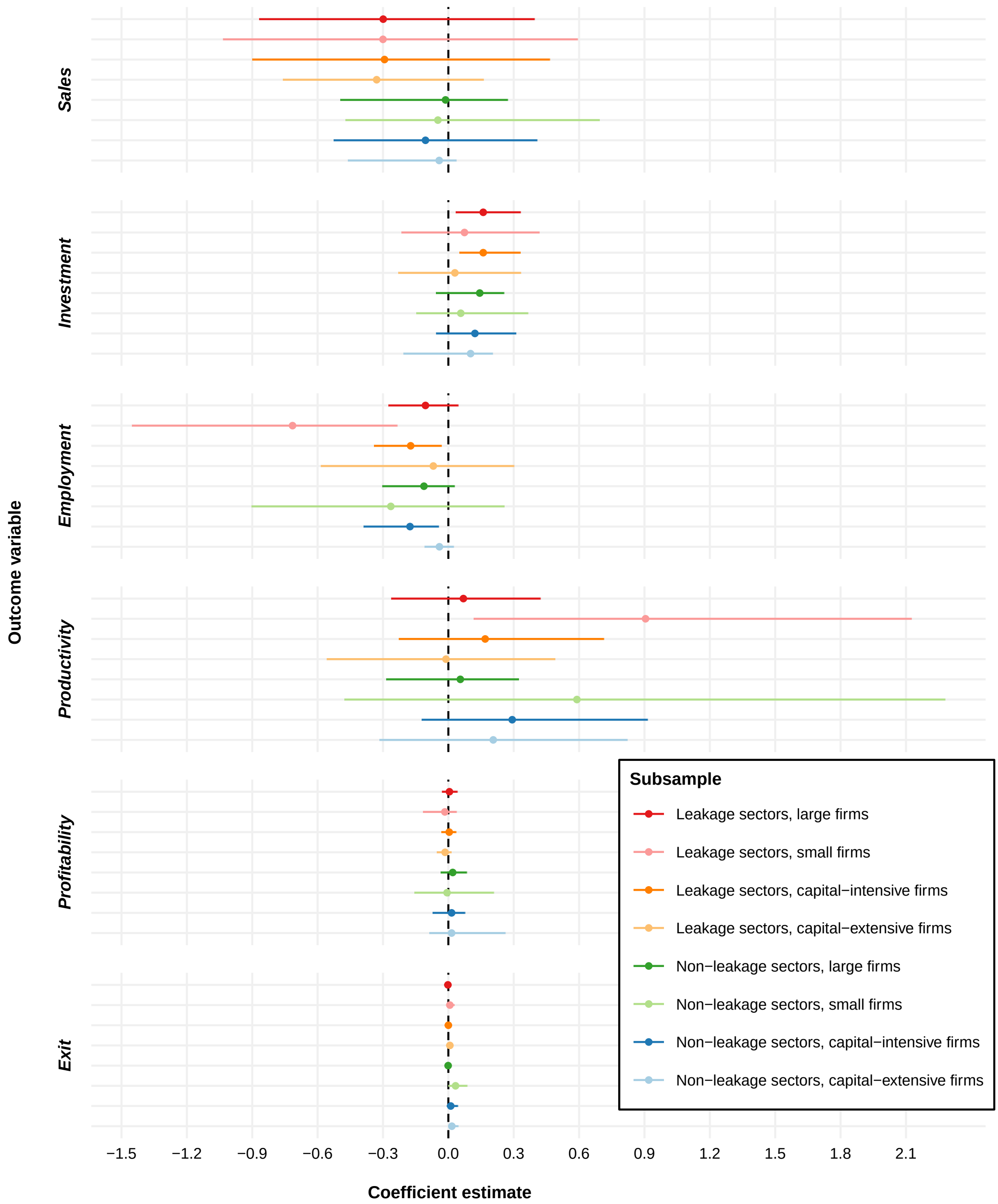
We further define subgroups of firm size and capital intensity. Large firms might be better able to mitigate adverse performance effects than small firms because of larger financial buffers, economies of scale, and their mobility and geographic diversification (Aus dem Moore et al., 2019; Dechezleprêtre et al., 2023). In addition, large firms are more likely to incorporate expected future increases in carbon costs in business operations (Trinks et al., 2022b). Capital intensity provides an indicator of the salience of carbon costs for firms, as it tends to be closely correlated with carbon intensity, which we do not observe at the firm level. Firms with low capital intensity and high trade intensity might be more geographically mobile or 'footloose' (Aus dem Moore et al., 2019) and could therefore be more likely to relocate their activities in response to rising carbon costs (Borghesi et al., 2020; Koch and Basse Mama, 2019). To account for this, we not only consider the subgroups individually but also examine the intersection of leakage sectors with firm size and capital intensity.

Finally, we investigate regional differences. We contrast EU and non-EU countries. Given the generally higher stringency of climate policies in the EU, carbon costs may have been more salient for EU firms' performance. On the other hand, given the EU's comparatively high carbon costs, EU firms might also have adapted production processes to become less sensitive to (moderate) rises in carbon costs, in which case we would observe less detrimental performance effects in EU firms.

The results are summarized in Figures 3, 4, and 5. Full results are available in Tables SM.7, SM.8, and SM.9. Overall, our estimates suggest that carbon costs tend to have more pronounced effects in firms in carbon leakage sectors and EU countries. Especially for these firms, we partly observe carbon cost-induced changes in other performance indicators, such as investment and productivity increases, in addition to significant employment reductions.[19]

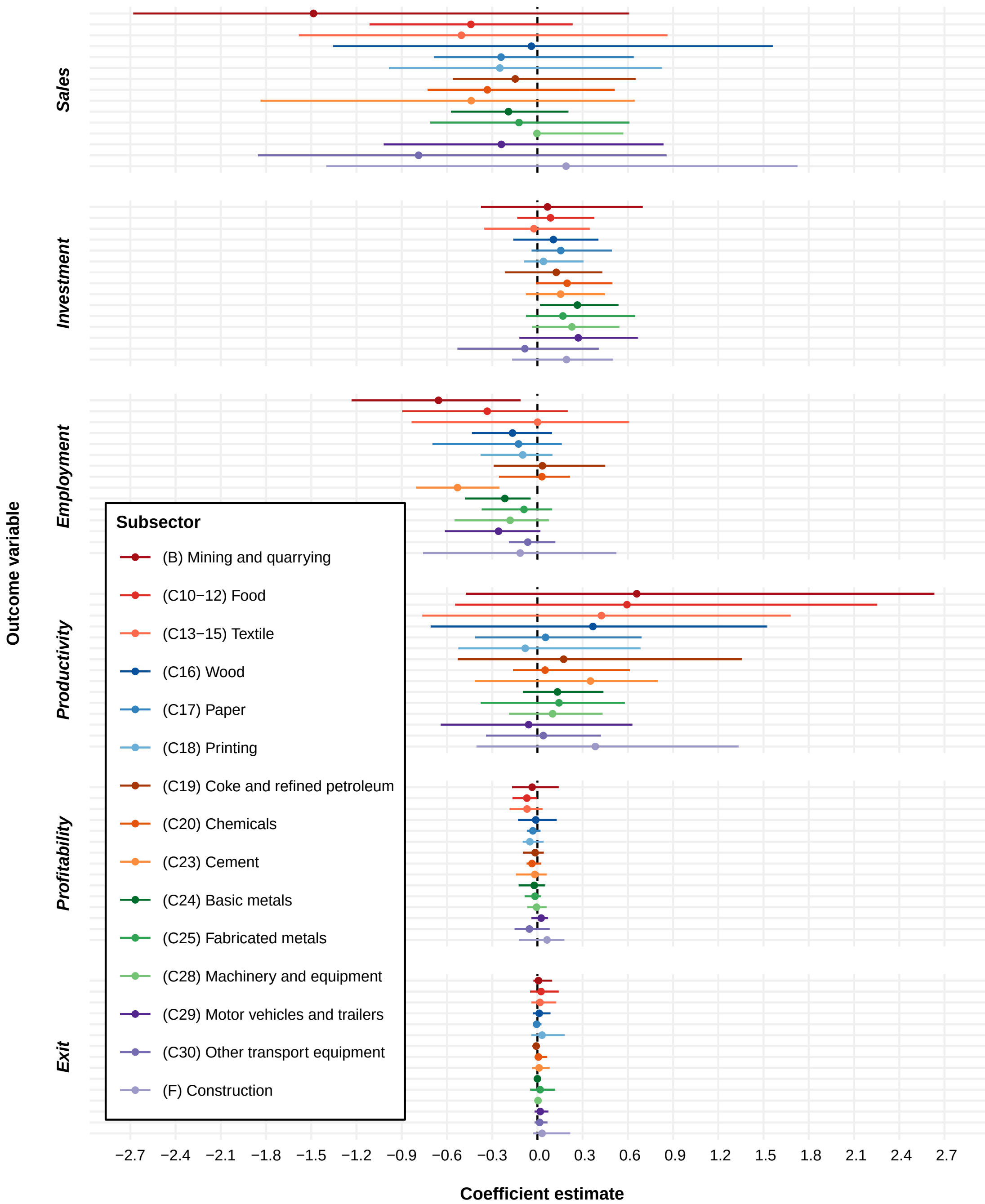
[19] Given the large number of subsamples considered, some caution is advised with regard to statistical significance. Nonetheless, we do not apply Bonferroni-type adjustments to p-values and confidence intervals for several substantive reasons: (1) the universal null hypothesis is not of interest in our analyses, (2) our comparisons directly follow from *a priori* hypotheses that the effects should differ between the considered subgroups, and (3) the adjustments would inflate type-II errors to a large extent.

Figure 3: Effect of carbon costs on performance outcomes in subsamples (leakage vs. non-leakage sectors and firm characteristics)



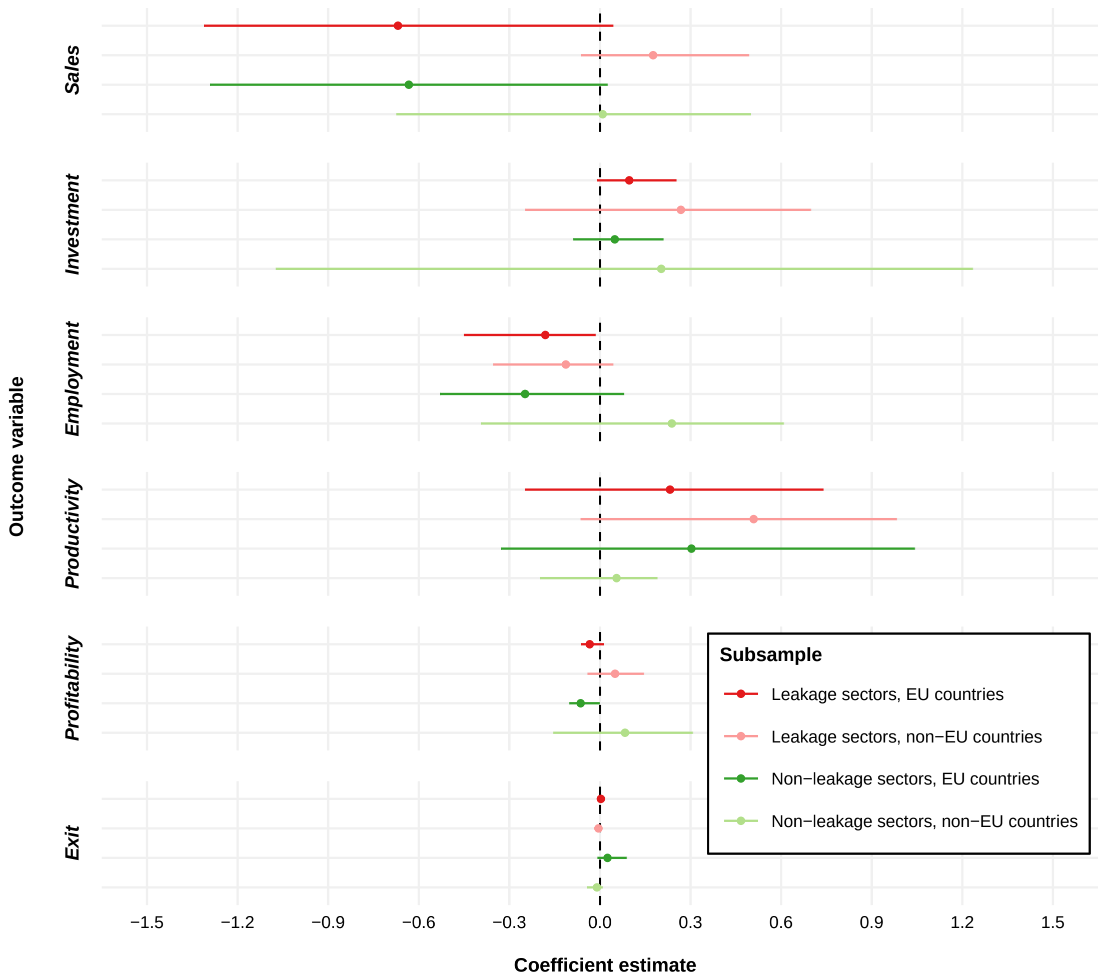
Dot indicates coefficient of carbon costs from baseline model (Eq. (1)), which can be interpreted as an elasticity for Sales, Investment, and Employment and semi-elasticity for Productivity, Profitability, and Exit; Line indicates 95% confidence interval based on Wild bootstrapped errors robust to clustering at the sector and country level; Subsample grouping variables are defined in Table A.1, where splits based on firm characteristics are defined by median firm-average values. Note that estimates and confidence intervals for Exit are small and insignificant for all estimations.

Figure 4: Effect of carbon costs on performance outcomes in industrial subsectors



Dot indicates coefficient of carbon costs from baseline model (Eq. (1)), which can be interpreted as an elasticity for Sales, Investment, and Employment and semi-elasticity for Productivity, Profitability, and Exit; Line indicates 95% confidence interval based on Wild bootstrapped errors robust to clustering at the sector and country level. Note that estimates and confidence intervals for Exit are small but insignificant.

Figure 5: Effect of carbon costs on performance outcomes in subsamples (leakage vs. non-leakage sectors and EU membership)



Dot indicates coefficient of carbon costs from baseline model (Eq. (1)), which can be interpreted as an elasticity for Sales, Investment, and Employment and semi-elasticity for Productivity, Profitability, and Exit; Line indicates 95% confidence interval based on Wild bootstrapped errors robust to clustering at the sector and country level. Subsample grouping variables are defined in Table A.1. Note that estimates and confidence intervals for Exit are small but insignificant.

Visually, sales revenue reductions seem to be associated more strongly with carbon costs in leakage sectors, but the effects are far from significant in all of the industrial subsectors. Hence, we find no evidence to suggest that carbon costs are significantly more relevant in the carbon- and trade-intensive sectors. The relatively strong negative estimates for industrial firms in EU countries, although insignificant, might indicate that these firms faced imperfect carbon cost pass-through and/or reduced production levels in response to rising carbon costs. The latter explanation, however, would not be in accord with the relatively consistent positive investment effects and the positive productivity coefficients. A mechanical explanation could play a role: marginal effects of carbon cost increases may be more pronounced in EU countries because of the higher average level of carbon costs, which implies that a percentage increase in carbon costs represents a stronger levels increase in EU countries than in non-EU countries. Moreover, the comparatively high share of energy- and trade-intensive sectors and small firms in the EU subsample may contribute to the relatively strong negative sales effects.

With a relatively high degree of certainty, we observe that carbon costs induced a ramping up of investment in large and capital-intensive firms, particularly in leakage sectors. The effects tend to be most prevalent in EU countries. Zooming in on subsectors, significant positive investment effects are observed especially in the basic metals sector. For each 1% rise in carbon costs, investments in large and capital-intensive firms in leakage sectors increased by 0.16% on average; or for a USD 50/tCO_2e rise the approximate effect would be a 3.0% increase in investments, ranging up to +4.9% in the basic metals sector.

The positive investment effects are in line with prior evidence on the EU ETS. Aus dem Moore et al. (2019) found that ETS-participating firms increased their tangible fixed assets by on average 12.1% more than non-ETS peers. Dechezleprêtre et al. (2023) reported an 8% increase in their sample. Prior evidence further shows that the ETS induced substantial emission reductions of 8–12% points more in participating firms (Colmer et al., 2022; Jaraite-Kažukauske and Di Maria, 2016; Klemetsen et al., 2020) and increased green technology patenting by 30% (Calel and Dechezleprêtre, 2016). In combination with this evidence, a plausible explanation for our findings would be anticipation and adjustment: firms responded to rising carbon costs to a large extent by initiating investments in carbon abatement technologies. Consistent with this explanation, Marin et al. (2018) find that ETS firms have passed through carbon costs and improved labor productivity, possibly through expanding their asset base. Our relatively small and insignificant effects for capital-extensive firms are consistent with Koch and Basse Mama (2019) and Borghesi et al. (2020). They find indications that, while the lion's share of firms does not behave in ways suggestive of relocation, a subgroup of 'footloose' firms with low capital intensity and high trade intensities behaves differently and might relocate to a certain degree in response to increased carbon costs. This subgroup is small in terms of both the number of firms and total carbon emissions.

Employment reductions seem to be concentrated in capital-intensive firms and small firms in leakage sectors, mainly in mining, cement, and basic metals. The effects are most clearly observed in EU countries. The effect size for a USD 50/tCO_2e cost increase would be in the order of -3.2% for capital-intensive firms and -13.2% for small firms in leakage sectors. The effects for subsectors would be: mining -12.1%, cement -9.8%, and basic metals -4.0%. Marin and Vona (2021) similarly found employment effects of energy price increases in France to be concentrated in leakage sectors, but they observed the effects mainly in large instead of small firms. In further robustness analyses, we find that employment effects decrease in size and significance when shifting the focus to larger firms (Table SM.4). These findings point at the importance of small firm characteristics, such as typically lower financial buffers, lower economies of scale, and possibly less forward-looking processes. A mechanical explanation could also play a role: a comparatively high percentage reduction in firms with only few employees still implies a modest employment effect in absolute terms, whereas for large firms it would correspond to laying off a substantial number of employees.

Productivity improvements are estimated in nearly all subgroups, but are only significant for small firms in leakage sectors. The approximate effect size for a USD 50/tCO_2e cost increase would be in the order of +1.8% for an average firm. The latter may be related to the respective carbon cost-induced employment reductions, entailing marginal productivity increases.

In all subsamples, profitability and exit probabilities are hardly affected by carbon costs. We estimate modest but significant negative profitability effects in the food sector, which may be explained by its high energy intensity, among other things. The weak profitability impact in leakage sectors might be accounted for by policy measures taken to mitigate adverse effects of carbon cost increases on operating performance. Indeed, for industrial firms in the EU ETS, Martin et al. (2014a,b) find indications of overcompensation. In non-leakage sectors in the EU, we document significant but economically modest negative profitability effects, corresponding to $< 0.1\%$ points ROA reduction per 1% increase in carbon costs. This effect may be driven by the reduction in sales, which is tempered in leakage sectors by countervailing policies. If anything, the levels of the remaining subsample estimates provide weak indications that small firms may have higher propensities for exit and are more strongly affected in their profitability. This aligns with the literature's findings that larger and capital-extensive firms are best able to cope with carbon cost increases, possibly because of their larger financial buffers, economies of scale, geographical diversification (Aus dem Moore et al., 2019), and internal systems to manage longer-term risks such as rising carbon costs (Trinks et al., 2022b).

4 Conclusion

This paper uses rich international microdata to evaluate how carbon costs affect firm performance and competitiveness. Shadow prices of fossil energy sources are employed as an encompassing and internationally comparable measure of carbon costs.

Our robust fixed effects instrumental variable estimations show little evidence of adverse performance effects of rising carbon costs for an average firm in industrial sectors, although these generally are expected to be most prone to competitiveness concerns. An exception are significant but economically modest employment reductions. However, we find highly dispersed effect estimates, suggesting considerable heterogeneity in firm responses and sensitivity to more stringent carbon regulations. Performance effects appear to be most pronounced in firms in carbon leakage sectors and EU countries. That is, especially for these firms, we partly observe carbon cost-induced changes in other performance indicators, such as investment and productivity increases, in addition to the significant employment reductions.

Policymakers may use our findings to help assess the risk of adverse effects of climate policies and to efficiently design these policies. Our findings show that adverse effects are limited and concentrated in small subgroups of firms. This is consistent with prior evidence on explicit carbon pricing. The findings may be explained by a pass-through of carbon costs and adaptation of production processes to rising carbon costs, most notably through investments into carbon abatement technologies. Another reason may be that the carbon cost increases studied still predominantly represent a small fraction of firms' total costs, even though the examined variation in carbon costs is comparatively large.

The positive investment effect in leakage-prone sectors, especially in large and capital-intensive firms, together with no considerable evidence for increased losses and exit probabilities, argues against substantial relocation and leakage effects. Indeed, the evidence points to an adaptation rather than relocation of production processes. Note, however, that our analysis does not explicitly consider locational and investment dynamics, such as multinational firms' decisions for up- or downscaling investment in one region in favor of another. The importance of these dynamics is contested (Bose et al., 2021; Dechezleprêtre et al., 2022; Koch and Basse Mama, 2019). Relevant for policymakers is that leakage risk is influenced by a large number of factors other than carbon costs, including agglomeration benefits, the tax system, institutional factors, and policy design. Still, the effects of carbon cost increases on performance and locational choices may be(come) nonlinear: leakage effects may turn considerable if ambitious carbon prices were introduced in one country but not in others and consumption patterns remained comparable.

The large dispersion in our estimates points to substantial effect heterogeneity and calls for further research. Firm performance and behavior may, for instance, relate to ownership structure (Akey and Appel, 2021; Aus dem Moore et al., 2019). In case of improved data availability, it may also be fruitful to build measures of climate policy stringency at higher levels of granularity, as stringency can be firm- or even plant-specific.

An explanation for our modest and dispersed estimates, which may be tested in future research, is that (part of the) firms seem to have anticipated future tightening of climate policies and gradually increased resilience to rising carbon costs. This explanation is supported by prior evidence on the EU ETS, showing that firms reduced regulated emissions (Colmer et al., 2022) and invested in green innovation (Calel and Dechezleprêtre, 2016). Hence, *expectations* over future carbon costs are likely to be an important driver of firm behavior and outcomes, such as abatement technology investment and locational decisions. However, this investor perspective seems to be underrecognized in the policy making, despite its relevance for policy design. When policy instruments are aligned with targets and predictable, it helps avoiding surprises and minimizing adjustment costs in the energy transition. The present study underscores that (if crafted well) climate policies can be strengthened with limited economic damage.

Author contributions

Both authors contributed to the study design and writing of the manuscript. A. constructed the merged dataset and performed the data analysis. E. estimated the shadow prices.

Competing interests

The authors declare no competing interests.

Additional materials

Additional materials associated with this article are included in Online Appendix A (Dataset description) and Online supplementary materials B (Results tables).

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A Online Appendix

Contents

A.1 Data description

International microdata are sourced from BvD Orbis Historical. This is the largest international firm-level dataset that combines information on financial statements and real activity (sales, investment, employment, etc.) of both listed and non-listed firms (Gopinath et al., 2017; Kalemli-Özcan et al., 2022). As such, it covers more variables than most census data sources and is much more encompassing than financial accounts of listed firms only. Orbis Historical further has several advantages in testing the effects of carbon costs compared to Orbis Online, which is commonly employed in related studies (Dechezleprêtre et al., 2022; Verde, 2020; Marin et al., 2018). Most importantly, Orbis Historical covers a long time period (1980–onwards, with the best coverage from the 2000s onwards), which allows us to conduct robust panel analyses for the period in which shadow prices are available (2000–2014). By comparison, Orbis Online would yield a maximum sample period of 4 years due to time restrictions. Another advantage mentioned by Kalemli-Özcan et al. (2022) is being able to circumvent problems and data losses related to data vintages and currency issues (e.g., see Marin et al. (2018)).

Orbis Historical covers a relatively large set of variables from public and private firms' balance sheets and income statements that are sourced from a large number of information providers including business registers. The coverage of firms and the reported information varies from country to country due to differences in legal and administrative filing requirements (Kalemli-Özcan et al., 2022). European countries therefore tend to be overrepresented and the US, for instance, is heavily underrepresented. However, most countries oblige limited liability firms to register upon formation and thresholds for filing are relatively low in the countries in our analysis, as supported by the extensive country coverage (Table A.3). Gopinath et al. (2017) show that the Orbis data are broadly representative on economic indicators such as manufacturing employment and gross output.

We retrieved the full dataset in July 2021. To handle this large amount of data, we follow the procedure suggested by Kalemli-Özcan et al. (2022) (link), which involves splitting up the individual large files by country, selecting the relevant sample and variables, and then appending these files to create one dataset.

Financial data on national firms require checks on reliability and consistency, especially across firms from different countries. We follow common data cleaning procedures by Kalemli-Özcan et al. (2022)[pp. 20–22]. To ensure that results are not driven by firms in financial distress (e.g., Carletti et al. (2020)), we exclude firms that reported negative total equity persistently, i.e. more than half of the time. The baseline analysis does not exclude firms which incidentally reported negative equity, because such cases are highly prevalent and often not indicative of financial distress (Jan and Ou, 2012; Luo et al., 2021). Table A.2 provides an overview of the data cleaning steps and the impact on the sample. We extensively assessed the robustness of the baseline results to additional or alternative data cleaning filters in Table SM.4. We find that our baseline results are not meaningfully altered when excluding (i) the construction sector, which represents a large share of the sample, (ii) firms that exhibit large year-on-year changes in the outcome variable, or (iii) all negative equity reporting firms. Further, notwithstanding the careful data cleaning procedures taken, a potential concern of microdata on unconsolidated reports is that the very small firms included might be more likely to be engaged in non-productive and/or holding activities; in addition, some countries or sectors with low reporting thresholds might be overrepresented. We therefore also consider a range of firm size filters in the robustness checks.[20] To avoid biases due to inflation, all monetary values in our study are in 2010 constant USD prices, consistent with the carbon cost measure. Conversion is based on country-level GDP deflators, in line with, e.g., Marin and Vona (2021). Following the literature, the financial variables are winsorized at the top and bottom 1%.

We further address potential inconsistencies in the financial outcome variables, which might arise due to diverging accounting practices. The measure of profitability faces potential issues, because accounting practices across countries particularly differ regarding the cost items in the profit and loss statement. We therefore tested that our results are similar for alternative profitability indicators based on earnings before interest and taxes (EBIT). Having a 'higher' position on the profit and loss statement, this measure avoids some potential inconsistencies in accounting practices, such as the shifting of interest payments within national firms from the same multinational. Note, however, that differences in accounting practices, which cannot be fully addressed, may still remain. These include the treatment of royalties or transfer pricing that would be relevant particularly for our estimation of profitability and sales effects. Yet, these reporting differences are expected to have limited bearing on our baseline results, given that we considered a wide range of outcome variables, alternative variable definitions, and firm size filters, and our sample does not focus on the typical tax haven countries.

We merge the microdata from Orbis Historical with the shadow price data based on the ISIC Rev. 4 sector classification, country, and year. Specifically, Orbis data from 1999–2019 are matched with the shadow price data from 2000–2014. Note that microdata are used from 1999 onwards because the dataset includes variables which require one-year lagged values, such as investment. The countries covered by the shadow price data represent 75.6% of the full Orbis Historical dataset. Industrial sectors make up 19.1% of all entries. The dataset period 1999–2019 covers 93.7% of the observations in the full Orbis Historical dataset, which can be explained by the sharp increase in the dataset's coverage from around 2000 onwards (Gopinath et al., 2017).

[20] We do not apply a firm size filter to the baseline model for three substantive reasons: First, size filters do not meaningfully alter the estimates (see Table SM.4), while they do eat up statistical power. Only the very demanding size filters render the employment effects insignificant, which is in line with our heterogeneity analyses that find insignificant employment effects in the subgroup of larger firms. Second, our specific interest lies in the heterogeneity of the effects of carbon costs across firm size groups. Last, there is no consensus in the literature about size filters. Various top publications do not apply any size filter (e.g., Gopinath et al. (2017)), whereas some related studies apply their own specific and *ad hoc*-determined filters (e.g., Aus dem Moore et al. (2019)).

Table A.1: Variable definitions and data sources

Variable	Definition	Unit	Source
Outcome variables			
Sales	Ln(Operating revenue)	Firm-year	BvD Orbis
Investment	Net investment = Ln(Tangible fixed assets in year t / Tangible fixed assets in year $t-1$)	Firm-year	BvD Orbis
Employment	Ln(Number of employees)	Firm-year	BvD Orbis
Productivity	Total factor productivity based on Ackerberg et al. (2015), with the following input-output specification: Output = ln(Operating revenue), Capital = ln(Tangible fixed assets), Labor = ln(Number of employees), Intermediate inputs = ln(Material costs)	Firm-year	BvD Orbis
Profitability	Return on Assets (ROA) = (Net income / Total assets) * 100%	Firm-year	BvD Orbis
Exit	Binary variable equaling 1 if year $t >$ firm i's last reporting year t^*, and 0 otherwise	Firm-year	BvD Orbis
Independent variable of interest			
Carbon costs	Shadow price of fossil fuels based on Althammer and Hille (2016)	Sector-country-year	WIOD, Penn World Tables, Sato et al. (2019), IEA, authors' calculations
Control variables			
Size	Ln(Total assets)	Firm-year	BvD Orbis
Leverage	(Total debt / Total assets) * 100%	Firm-year	BvD Orbis
GDP per capita	Ln(GDP per capita)	Country-year	World Bank
Capital intensity	(Tangible fixed assets / Total assets) * 100%	Firm-year	BvD Orbis
Grouping variables			
Leakage sector	Binary variable equalling 1 for firms in NACE 4-digit sectors deemed at risk of carbon leakage by the EC (link) and 0 otherwise	Firm	BvD Orbis, EC
Size	Binary variable equalling 1 for firms which have a mean Size above the sample median and 0 otherwise	Firm	BvD Orbis
Capital intensity	Binary variable equalling 1 for firms which have a mean Capital intensity above the sample median and 0 otherwise	Firm	BvD Orbis
EU country	Binary variable equalling 1 for firms located in an EU member country and 0 otherwise	Firm	BvD Orbis

Table A.2: Data cleaning

#	Filter	**N*** lost	% lost	**N** remaining
	Consolidation level**			
1	Drop consolidation codes “LF”	38	0.00%	37,460,827
2	Drop consolidation codes “C1”	245,091	0.56%	37,215,736
3	Drop consolidation codes “C2”	299,373	0.68%	36,916,363
	Financial and non-limited firms and missing information			
4	Drop non-corporate entities (mainly financial firms)	235,327	0.55%	36,676,552
5	Drop non-limited firms	2,034,809	4.65%	34,641,743
	Kalemli-Özcan et al. (2022) procedure***			
6	Drop if total assets, operating revenue, and employment are all jointly missing	240,543	0.55%	34,401,200
7	Drop firms with negative total assets	27,969	0.06%	34,373,231
8	Drop firms with negative sales	81,103	0.19%	34,292,128
9	Drop firms with negative tangible fixed assets	82,342	0.19%	34,209,786
	Duplicate entries			
10	Drop oldest reporting date for firms with multiple reporting dates	217,146	0.50%	33,992,640
11	Drop entry with the least information	30,101	0.07%	33,962,539
12	Drop entry with the most zeros	754	0.00%	33,961,785
13	Drop consolidation level U2 for firms reporting on U2 and U1 consolidation levels	3,406	0.01%	33,958,379
14	Drop remaining duplicates	5,463	0.01%	33,952,916
	Unreliable information			
15	Drop firms for which total equity is negative for more than half of the time	334,250	0.76%	33,618,666
Total		**3,842,199**	**10.26%**	**33,618,666**

* The original dataset is the BvD Orbis Historical Academic dataset merged with the shadow price data; it has **N** = 37,460,865 entries. To ensure consistency with the regression analyses, this only includes the countries and sectors for which shadow price data are available and the microdata from the period 1999–2019.

** LF stands for limited financials: the information available for such firms is often limited to sales and employment and based on rounded or estimated figures. C1 and C2 are consolidated accounts, which means that the reported information refers to a parent firm integrating the statements of its controlled subsidiaries. C1 refers to reports by parent firms of which the information on its subsidiaries are not reported or included in the dataset, and C2 indicates cases where both parent firms and subsidiaries are included in the dataset. The codes considered in our analysis are U1 and U2, where U1 indicates that the unconsolidated information is the only reported information available for the firm and U2 indicates that the unconsolidated information is from a firm that also reported consolidated accounts.

*** We follow the steps described in Kalemli-Özcan et al. (2022) [pp. 20–22] which are applicable and relevant to our analysis. We do not follow the outlier-correction steps of Kalemli-Özcan et al. (2022) [steps 7–9 on pp. 20–22] in the main analysis, as these corrections are *ad hoc* and would imply a rigorous removal of ‘outliers’ which in our analyse would largely represent firms with missing employment data (but results uphold when applying these steps). Instead, we follow the common practice in the literature and address potential outliers by winsorizing the financial variables at the top and bottom 1%.

Table A.3: Sample coverage

Panel A: Countries			
ISO	Name	**N***	%
AT	Austria	331,283	0.99
AU	Australia	28,574	0.08
BE	Belgium	1,343,658	4.00
BR	Brazil	24,756	0.07
CA	Canada	15	0.00
CH	Switzerland	187	0.00
CN	China	2,822,893	8.40
CZ	Czech Republic	450,447	1.34
DE	Germany	1,717,348	5.11
DK	Denmark	371,074	1.10
FI	Finland	511,383	1.52
FR	France	3,711,846	11.04
GB	United Kingdom	5,554,204	16.52
GR	Greece	96,585	0.29
HR	Croatia	349,369	1.04
HU	Hungary	686,328	2.04
ID	Indonesia	624	0.00
IN	India	261,188	0.78
IT	Italy	3,812,623	11.34
JP	Japan	1,735,908	5.16
KR	Republic of Korea	1,249,859	3.72
MX	Mexico	16,791	0.05
NL	Netherlands	774,379	2.30
PL	Poland	410,199	1.22
PT	Portugal	1,120,986	3.33
RO	Romania	1,408,922	4.19
RU	Russian Federation	3,502,956	10.42
SE	Sweden	918,797	2.73
SK	Slovakia	296,538	0.88
TR	Turkey	96,373	0.29
TW	Taiwan	12,338	0.04
US	United States	235	0.00

Panel B: Sectors			
ISIC	Name	**N**	%
B	Mining and quarrying	505,367	1.50
C10-12	Manufacture of food products, beverages and tobacco products	2,180,576	6.49
C13-15	Manufacture of textiles, wearing apparel and leather products	1,902,355	5.66
C16	Manufacture of wood and of products of wood and cork, except furniture; manufacture of articles of straw and plaiting materials	988,425	2.94
C17	Manufacture of paper and paper products	362,450	1.08
C18	Printing and reproduction of recorded media	904,526	2.69
C19	Manufacture of coke and refined petroleum products	59,843	0.18
C20	Manufacture of chemicals and chemical products	758,926	2.26
C23	Manufacture of other non-metallic mineral products	962,121	2.86
C24	Manufacture of basic metals	428,726	1.28
C25	Manufacture of fabricated metal products, except machinery and equipment	2,906,568	8.65
C28	Manufacture of machinery and equipment n.e.c.	1,737,700	5.17
C29	Manufacture of motor vehicles, trailers and semi-trailers	434,936	1.29
C30	Manufacture of other transport equipment	246,749	0.73
F	Construction	19,239,398	57.23

Continuation of Table A.3

Panel C: Time		
Year	**N**	%
1999	540,986	1.61
2000	609,697	1.81
2001	688,304	2.05
2002	823,000	2.45
2003	923,609	2.75
2004	1,121,279	3.34
2005	1,302,582	3.87
2006	1,450,015	4.31
2007	1,624,044	4.83
2008	1,701,838	5.06
2009	1,735,871	5.16
2010	1,737,920	5.17
2011	1,812,892	5.39
2012	1,929,991	5.74
2013	2,159,396	6.42
2014	2,359,316	7.02
2015	2,128,062	6.33
2016	2,219,677	6.60
2017	2,311,949	6.88
2018	2,332,305	6.94
2019	2,105,933	6.26

* Shown are the number of entries (**N**) of the full cleaned BvD Orbis Historical Academic dataset merged with the shadow price data as per Table A.2; the total number of entries is 33,618,666.

B Online supplementary materials

Contents

Table SM.1: Alternative model specifications

Model:	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Sales	Investment	Employment	Productivity	Profitability	Exit
Scale:	*log*	*log*	*log*	*level*	*level*	*level*
Estimator: FE-IV	-0.536	0.023	-0.335*	0.149	0.010	0.012
Fixed effects: i, t	(-1.697)	(0.425)	(-2.824)	(0.944)	(0.249)	(1.333)
Controls: no	12,753,972	13,301,054	9,180,078	4,741,583	14,128,374	19,561,350
Estimator: FE-IV	-0.060	0.123	-0.128**	0.266	0.019	0.012
Fixed effects: i, t	(-0.402)	(1.772)	(-4.231)	(1.360)	(0.591)	(1.324)
Controls: yes, more	12,089,417	13,234,333	8,766,987	4,741,493	13,588,203	18,886,450
Estimator: FE-IV	-0.003	0.165	-0.155	0.270	0.020	0.011
Fixed effects: i, t, st	(-0.010)	(2.665)	(-3.374)	(1.230)	(0.636)	(1.097)
Controls: yes	12,391,027	13,234,333	8,969,509	4,741,493	13,998,001	19,561,350
Estimator: IV	0.565	0.038	-0.002	-0.130	-0.007	0.012
Fixed effects: s, c, t	(1.664)	(0.934)	(-0.013)	(-1.366)	(-0.487)	(1.333)
Controls: yes	12,786,395	13,556,011	9,363,694	4,915,632	14,349,291	19,561,350
Estimator: FE-OLS	0.388	0.061	-0.100*	0.112	-0.010	0.016
Fixed effects: i, t	(1.728)	(1.877)	(-1.861)	(1.850)	(-0.342)	(1.906)
Controls: yes	12,786,395	13,556,011	9,363,694	4,915,633	14,349,291	19,561,350

FE-IV estimates for variations on Eq. (1). Presented are, respectively, the coefficient estimates, Wild bootstrapped t-values robust to clustering at both the sector and country level (see Section 2.1) in parentheses, and the number of observations (N). The models are altered in a stepwise manner. First, we exclude firm-level control variables to test for bad control issues. We then inspect potential remaining confounding by expanding the set of control variables with *capital intensity* (ratio of tangible fixed assets to total assets). Further, we saturated the model by adding sector-time fixed effects. Next, we remove firm fixed effects to check for the cross-sectional effects. Lastly, OLS estimation is considered instead of IV to assess the relevance of the IV estimator in correcting for simultaneity.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table SM.2: Alternative error clustering regimes

Model:	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Sales	Investment	Employment	Productivity	Profitability	Exit
Scale:	*log*	*log*	*log*	*level*	*level*	*level*
Coefficient	-0.109	0.123	-0.132	0.256	0.012	0.012
Errors: WRE clustering by country and sector						
95% CI	[-0.491, 0.102]	[-0.067, 0.272]	[-0.272, -0.082]	[-0.207, 0.816]	[-0.064, 0.097]	[-0.006, 0.039]
t-statistic	-0.854	1.774	-4.835	1.264	0.383	1.333
p-value	0.345	0.349	0.034	0.513	0.999	0.268
G (G_c, G_s)	32, 15	31, 15	32, 15	24, 15	32, 15	32, 15
Errors: WRE clustering by country						
95% CI	[-0.929, 0.425]	[-0.044, 0.291]	[-0.408, 0.047]	[-0.218, 0.953]	[-0.091, 0.102]	[-0.011, 0.045]
t-statistic	-0.375	1.536	-1.472	1.111	0.266	0.859
p-value	0.634	0.156	0.122	0.339	0.970	0.481
G	32	31	32	24	32	32
Errors: WRE clustering by sector						
95% CI	[-0.502, 0.138]	[0.024, 0.179]	[-0.250, -0.093]	[0.118, 0.343]	[-0.045, 0.053]	[0.004, 0.021]
t-statistic	-0.778	3.866	-5.525	5.485	0.509	3.096
p-value	0.447	0.003	0.014	0.001	0.978	0.005
G	15	15	15	15	15	15
Errors: CRVE clustering by country						
95% CI	[-0.699, 0.482]	[-0.041, 0.286]	[-0.315, 0.051]	[-0.221, 0.734]	[-0.078, 0.101]	[-0.016, 0.040]
t-statistic	-0.375	1.536	-1.472	1.111	0.266	0.859
p-value	0.710	0.135	0.151	0.278	0.792	0.397
G	32	31	32	24	32	32

FE-IV estimates per Eq. (1). Firm- and year-fixed effects included in Models (1)-(5). Model (6) includes sector-, country-, and year-fixed effects and the controls are specified as the pre-exit firm average. The first row shows the coefficient estimate on carbon costs. Presented in the remaining rows are, respectively, the 95% confidence intervals, t-statistics, p-values, and number of clusters ($G \in [G_c, G_s]$) in alternative standard error clustering regimes. First shown is the baseline specification (Table 2), which applies the WRE bootstrap (Davidson and MacKinnon, 2010; Roodman et al., 2019) with 999 replications and two-way clustering errors at the country- and sector level. Further shown is clustering at the country level, sector level, and ordinary clustering as per the cluster-robust variance estimator (CRVE) at the country level. Note that multi-level clustering based on the CRVE is not considered. Although this has become a common sight in the literature, its asymptotic assumptions are generally not met and the theory in this area is still under active development (MacKinnon et al., 2023; Petersen, 2009). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table SM.3: Alternative variable specifications

Model:	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Sales	Investment	Employment	Productivity	Profitability	Exit
Scale:	*log*	*log*	*log*	*level*	*level*	*level*
Shadow price:	-0.117	0.050	-0.168	0.230	-0.001	0.011
2-year wedges	(-0.764)	(0.689)	(-2.413)	(1.369)	(-0.021)	(1.523)
	12,312,680	13,194,010	8,881,332	4,741,344	13,902,220	19,453,341
Investment:		0.117				
fixed assets		(1.401)				
(*log*)		13,899,469				
Employment:			-0.047			
wages (*log*)			(-1.072)			
			10,279,292			
Productivity:				0.269		
by sector-				(1.120)		
country (*level*)				4,741,566		
Productivity:				0.230		
K = fixed assets				(1.179)		
(*level*)				4,818,059		
Productivity:				-0.048		
L = wages				(-0.408)		
(*level*)				7,163,013		
Productivity:				0.030		
I = cost of goods				(2.231)		
sold (*level*)				2,625,520		
Profitability:					-18.381	
ROA operating					(0.930)	
profit (*level*)					14,185,440	
Profitability:					-15.532*	
Operating profit					(2.194)	
margin (*level*)					12,307,207	

FE-IV estimates per Eq. (1), in which we alter the specification of variables. Firm- and year-fixed effects included in Models (1)-(5). Model (6) includes sector-, country-, and year-fixed effects and the controls are specified as the pre-exit firm average. Presented are, respectively, the coefficient estimates, Wild bootstrapped t-values robust to clustering at both the sector and country level (see Section 2.1) in parentheses, and the number of observations (N). First, we employ an alternative carbon cost measure in which we re-estimate the shadow prices using 2- instead of 3-year average price wedges. Next, we alternatively define investment as the growth in fixed assets, which considers tangible as well as intangible fixed assets acquired by the firm (see, e.g., Gopinath et al. (2017)). Next, we re-estimated productivity by sector-country group and by replacing the capital (K), labor (L), and intermediate inputs (I) components one-by-one with fixed assets, wages, and cost of goods sold, respectively. Lastly, we alternatively define profitability as ROA based on operating profit (EBIT / total assets) or operating profit margin (EBIT / sales). Finally, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table SM.4: Alternative data specifications

Model:	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Sales	Investment	Employment	Productivity	Profitability	Exit
Scale:	*log*	*log*	*log*	*level*	*level*	*level*
Exclude	-0.317	0.118	-0.190**	0.217	-0.024	0.004
construction	(-1.298)	(1.460)	(-2.210)	(0.993)	(-1.431)	(0.627)
sector	5,575,703	5,738,986	4,362,094	2,435,466	6,088,321	7,991,106
Exclude firms	-0.097	0.122	-0.140**	0.267	0.013	0.012
with large	(-0.752)	(1.778)	(-5.259)	(1.288)	(0.421)	(1.324)
value jumps	12,026,782	12,933,868	8,756,549	4,615,902	13,598,415	19,071,685
Exclude all	-0.078	0.133	-0.117**	0.211	-0.007	0.014
firms reporting	(-1.019)	(1.971)	(-4.908)	(1.243)	(-0.412)	(1.378)
negative equity	10,876,860	11,564,183	7,813,740	4,272,954	12,164,890	16,832,542
Rigorous data	-0.034	0.252	-0.061	0.228	0.069	
cleaning	(-0.346)	(1.550)	(-1.405)	(1.435)	(2.207)	
KO2022	1,832,308	1,411,005	1,842,225	1,200,920	1,834,027	
Size threshold:	-0.136	0.133	-0.117**	0.188	0.015	-0.000
Duval2020	(-1.279)	(1.856)	(-4.545)	(1.088)	(0.602)	(-0.049)
	8,271,807	8,613,036	7,015,262	3,713,918	8,820,095	11,195,889
Size threshold:	-0.067	0.156	-0.091**	0.165	0.001	0.004
Nikolov2021	(-0.463)	(2.238)	(-4.865)	(1.377)	(0.090)	(0.713)
	5,432,582	5,952,486	4,049,986	1,975,889	5,956,584	8,259,684
Size threshold:	-0.015	0.114	-0.072*	0.064	0.013	0.010
AdM2019	(-0.098)	(1.787)	(-1.768)	(0.609)	(0.644)	(1.253)
	7,276,783	8,975,526	5,461,828	2,505,736	8,355,862	13,219,580
Size threshold:	-0.125	0.141	-0.071*	0.122	0.002	-0.003
EU Micro firm	(-1.254)	(2.064)	(-1.696)	(1.047)	(0.153)	(-0.972)
definition (link)	5,181,609	5,416,183	4,485,708	2,188,992	5,459,771	6,971,279

FE-IV estimates per Eq. (1) for alternative subsets of the data. Firm- and year-fixed effects included in Models (1)-(5). Model (6) includes sector-, country-, and year-fixed effects and the controls are specified as the pre-exit firm average. Presented are, respectively, the coefficient estimates, Wild bootstrapped t-values robust to clustering at both the sector and country level (see Section 2.1) in parentheses, and the number of observations (N). We first test whether our baseline results are driven by the construction sector, which represents a large share of the sample. We then check whether our results are affected by large jumps that sometimes occur in the outcome variables; we follow Aus dem Moore et al. (2019) and exclude firms that report changes in the outcome variables larger or equal to the 99.9^{th} percentile. We further examine if firms that reported negative total equity at some point in time drive our baseline results, as some sources presume that negative equity might signal financial distress and thus reflect abnormal business situations. Next, we test whether results are consistent with our baseline estimates when applying additional 'outlier' removal steps suggested by Kalemli-Özcan et al. (2022). In our case, this approach would be rather rigorous, as requiring all variables to be available implies that many firms with incomplete data, most notably on employee count, would be dropped, even though the data cleaning variables would be relevant in only one of the six models we estimate. Lastly, we exclude very small firms from the sample, using various definitions from the literature, which are sorted from smaller to larger firm size thresholds: Duval et al. (2020) exclude firms with employees < 3; Nikolov et al. (2021) filter out firms with employees < 10 *or* total assets $<$ USD 100,000; Aus dem Moore et al. (2019) exclude firms with employees ≤ 15 *and* sales $\leq$ USD 1 million *and* total assets $\leq$ USD 2 million; and the EU definition of a 'micro firm' would exclude firms with employees < 10 *and* (sales $<$ USD 2 million *or* total assets $\leq$ USD 2 million) (link). For all size threshold specifications, we use firm-average values and require availability of the relevant variables. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table SM.5: Longer-term effects: Past carbon costs and industrial firm performance

Model:	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Sales	Investment	Employment	Productivity	Profitability	Exit
Scale:	*log*	*log*	*log*	*level*	*level*	*level*
t-1	-0.010	0.116	-0.135**	0.216	0.010	0.011
	(-0.081)	(1.909)	(-3.862)	(2.209)	(0.407)	(1.317)
	11,197,560	14,065,226	8,500,813	4,560,822	12,599,892	17,734,945
t-3	-0.021	0.159	-0.065	-0.135	0.030	0.007
	(-0.131)	(1.532)	(-0.890)	(-1.394)	(1.031)	(1.640)
	9,743,706	12,079,280	7,987,821	4,087,465	10,862,075	15,508,779
t-5	0.048	0.157	0.039	-0.196	0.047	0.007
	(0.260)	(1.589)	(0.240)	(-1.486)	(1.541)	(1.383)
	8,610,821	10,815,417	7,548,941	3,635,584	9,602,382	13,842,913

FE-IV estimates per Eq. (1) (2000–2019), where the variable of interest is specified as $Carbon\ costs_{it-k}$ with $k \in [1, 3, 5]$. Firm- and year-fixed effects included in Models (1)-(5). Model (6) includes sector-, country-, and year-fixed effects and the controls are specified as the pre-exit firm average. Presented are, respectively, the coefficient estimates, Wild bootstrapped t-values robust to clustering at both the sector and country level (see Section 2.1) in parentheses, and the number of observations (N). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table SM.6: Longer-term effects: Simultaneous lags of carbon costs and testing for cumulative effects

Model:	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Sales	Investment	Employment	Productivity	Profitability	Exit
Scale:	*log*	*log*	*log*	*level*	*level*	*level*
t until t-1	-0.022	0.119	-0.207**	0.375	0.003	0.015
	(-0.132)	(1.759)	(-4.222)	(1.755)	(0.082)	(1.429)
	10,119,935	12,795,654	7,596,242	4,188,405	11,414,587	16,038,248
t until t-2	0.044	0.176	-0.290***	0.388	-0.008	0.016
	(0.215)	(2.151)	(-4.323)	(2.057)	(-0.243)	(1.372)
	8,247,732	10,721,508	6,349,634	3,583,707	9,337,562	13,150,029
t until t-3	0.096	0.162*	-0.341***	0.628*	-0.005	0.014
	(0.384)	(1.960)	(-3.565)	(2.221)	(-0.131)	(1.378)
	6,692,317	8,865,871	5,244,144	3,024,545	7,609,102	10,767,767
t until t-4	0.156	0.125	-0.317**	0.665**	0.000	0.015
	(0.548)	(1.684)	(-3.121)	(2.114)	(-0.012)	(1.276)
	5,369,078	7,232,866	4,254,403	2,528,156	6,143,723	8,764,711
t until t-5	0.164	0.024	-0.285*	0.765**	0.0024	0.013
	(0.510)	(0.358)	(-2.956)	(2.114)	(0.080)	(1.026)
	4,269,469	5,836,474	3,416,297	2,079,179	4,917,688	7,065,997

FE-IV estimates per Eq. (1), including simultaneously 1- up to 5 lags of the variable of interest, $Carbon\ costs_{it-k}$, with $k \in [1, ..., 5]$. Firm- and year-fixed effects included in Models (1)-(5). Model (6) includes sector-, country-, and year-fixed effects and the controls are specified as the pre-exit firm average. Presented are, respectively, the coefficient estimate of the cumulative effect, Wild bootstrapped t-values robust to clustering at both the sector and country level (see Section 2.1) in parentheses, and the number of observations (N). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table SM.7: Effect of carbon costs in subsamples (leakage vs. non-leakage sectors and firm characteristics)

Model: Outcome: Scale:	(1) Sales *log*	(2) Investment *log*	(3) Employment *log*	(4) Productivity *level*	(5) Profitability *level*	(6) Exit *level*
Leakage sectors, large firms	-0.299 (-1.374) 499,985	0.160*** (2.281) 444,298	-0.105 (-1.724) 414,138	0.069 (0.688) 157,381	0.005 (0.354) 552,882	-0.002 (-0.521) 649,384
Leakage sectors, small firms	-0.300 (-1.009) 79,945	0.074 (0.597) 85,369	-0.715*** (-3.010) 55,715	0.905*** (2.192) 25,350	-0.016 (-0.650) 102,582	0.007 (0.850) 168,873
Leakage sectors, capital-intensive firms	-0.293 (-1.244) 467,768	0.160*** (2.355) 430,388	-0.173** (-2.340) 388,735	0.169 (1.193) 147,542	0.004 (0.287) 491,186	-0.000 (-0.063) 614,545
Leakage sectors, capital-extensive firms	-0.329 (-2.219) 107,882	0.030 (0.250) 99,279	-0.069 (-0.392) 79,726	-0.011 (-0.062) 35,189	-0.015 (-1.098) 128,774	0.007 (1.084) 182,479
Non-leakage sectors, large firms	-0.013 (-0.079) 6,769,629	0.144 (1.917) 7,336,934	-0.112 (-2.601) 5,177,434	0.055 (0.463) 2,717,284	0.020 (0.862) 7,370,287	-0.001 (-0.360) 9,512,246
Non-leakage sectors, small firms	-0.048 (-0.514) 5,041,468	0.057 (0.768) 5,367,732	-0.264 (-2.631) 3,322,222	0.590 (1.689) 1,841,478	-0.006 (-0.117) 6,002,250	0.033* (1.573) 9,230,847
Non-leakage sectors, capital-intensive firms	-0.105 (-0.775) 6,618,512	0.122 (1.844) 7,934,267	-0.176** (-3.651) 5,135,743	0.293 (1.305) 2,705,114	0.015 (0.489) 7,496,553	0.011 (1.277) 10,283,495
Non-leakage sectors, capital-extensive firms	-0.042 (-0.855) 5,118,584	0.102 (1.241) 4,770,399	-0.041* (-2.072) 3,319,123	0.206 (1.186) 1,853,648	0.015 (0.428) 5,784,143	0.016 (1.413) 7,805,931

FE-IV estimates per Eq. (1) by subsample. Subsamples are defined by median firm-average values of the grouping variable (definitions are in Table A.1). Firm- and year-fixed effects included in Models (1)-(5). Model (6) includes sector-, country-, and year-fixed effects and the controls are specified as the pre-exit firm average. Presented are, respectively, the coefficient estimates, Wild bootstrapped t-values robust to clustering at both the sector and country level (see Section 2.1) in parentheses, and the number of observations (N). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table SM.8: Effect of carbon costs in industrial subsectors

Model:	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Sales	Investment	Employment	Productivity	Profitability	Exit
Scale:	*log*	*log*	*log*	*level*	*level*	*level*
(B) Mining and quarrying	-1.485	0.067	-0.656**	0.659	-0.035	0.007
	(-2.522)	(0.402)	(-3.022)	(1.135)	(-0592)	(0.365)
	152,627	159,657	119,007	47,621	175,611	262,602
(C10-12) Food	-0.441	0.087	-0.333	0.594	-0.070**	0.024
	(-1.494)	(0.726)	(-1.588)	(0.922)	(-2.238)	(0.627)
	898,590	877,801	685,218	403,269	998,103	1,223,492
(C13-15) Textile	-0.504	-0.023	0.001	0.425	-0.069	0.017
	(-1.215)	(-0.155)	(0.005)	(0.789)	(-1.530)	(0.537)
	773,574	687,675	577,878	324,017	829,959	1,024,649
(C16) Wood	-0.040	0.106	-0.165	0.368	-0.011	0.012
	(-0.081)	(0.804)	(-1.422)	(0.821)	(-0.228)	(0.562)
	431,637	423,861	356,831	166,047	473,469	577,495
(C17) Paper	-0.241	0.155	-0.125	0.054	-0.030	-0.005
	(-1.145)	(1.557)	(-0.836)	(0.368)	(1.734)	(-0.441)
	139,103	138,937	111,505	55,634	148,123	192,752
(C18) Printing	-0.249	0.040	-0.097	-0.081	-0.050	0.031
	(-0.737)	(0.653)	(-1.102)	(-0.395)	(-2.576)	(0.835)
	323,365	407,919	233,570	136,408	362,272	553,464
(C19) Coke and refined petroleum	-0.147	0.125	0.033	0.174	-0.015	-0.008
	(-1.017)	(1.076)	(0.354)	(1.276)	(-0.807)	(-1.066)
	22,910	19,545	18,684	8,112	24,709	29,995
(C20) Chemicals	-0.332	0.197*	0.030	0.051	-0.036	0.007
	(-1.813)	(2.028)	(0.388)	(0.446)	(-2.498)	(0.424)
	274,678	256,800	220,071	97,638	293,220	379,142
(C23) Cement	-0.439	0.154	-0.530***	0.352	-0.017	0.011
	(-1.003)	(1.295)	(-4.769)	(1.257)	(-0.437)	(0.454)
	378,307	372,234	304,685	161,627	415,486	517,267
(C24) Basic metals	-0.192	0.265**	-0.216***	0.133	-0.021	0.000
	(-1.309)	(2.276)	(-2.349)	(1.538)	(-0.637)	(0.008)
	161,452	156,084	131,938	60,387	172,206	227,029
(C25) Fabricated metals	-0.122	0.169	-0.089	0.143	-0.016	0.018
	(-0.437)	(1.248)	(-0.933)	(0.718)	(-1.004)	(0.670)
	1,124,143	1,290,473	877,891	576,568	1,233,359	1,689,929
(C28) Machinery and equipment	-0.003	0.229	-0.181	0.101	-0.006	0.004
	(-0.014)	(1.632)	(-1.385)	(1.019)	(-0.246)	(0.303)
	651,237	701,504	533,666	292,043	703,453	957,034
(C29) Motor vehicles and trailers	-0.239	0.271	-0.258*	-0.058	0.025	0.019
	(-0.626)	(1.557)	(-1.772)	(-0.288)	(1.133)	(0.934)
	166,618	160,490	130,669	74,624	174,319	222,884
(C30) Other transport equipment	-0.788	-0.083	-0.064	0.039	-0.053	0.015
	(-1.618)	(-0.446)	(-0.830)	(0.233)	(-1.311)	(0.815)
	77,462	86,006	60,481	31,471	84,032	133,372
(F) Construction	0.190	0.193	-0.114	0.384	0.064	0.031
	(0.358)	(1.501)	(-0.659)	(1.278)	(1.016)	(0.749)
	6,815,324	7,495,347	4,607,415	2,306,027	7,909,680	11,570,244

FE-IV estimates per Eq. (1) by ISIC subsector. Full subsector names are included in Table A.3. Firm- and year-fixed effects included in Models (1)-(5). Model (6) includes sector-, country-, and year-fixed effects and the controls are specified as the pre-exit firm average. Presented are, respectively, the coefficient estimates, Wild bootstrapped t-values robust to clustering at both the sector and country level (see Section 2.1) in parentheses, and the number of observations (N). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table SM.9: Effect of carbon costs in subsamples (leakage vs. non-leakage sectors and EU membership)

Model:	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Sales	Investment	Employment	Productivity	Profitability	Exit
Scale:	*log*	*log*	*log*	*level*	*level*	*level*
Leakage sectors,	-0.669*	0.097*	-0.181**	0.232	-0.034	0.003
EU countries	(-3.355)	(1.822)	(-2.468)	(1.115)	(-2.402)	(0.623)
	268,045	312,895	212,062	146,779	306,662	426,719
Leakage sectors,	0.176	0.268	-0.113	0.509	0.050	-0.005
non-EU countries	(1.953)	(1.494)	(-1.792)	(9.478)	(1.408)	(-0.983)
	311,885	216,772	257,791	35,952	318,802	391,538
Non-leakage sectors,	-0.633*	0.049	-0.248	0.303	-0.064***	0.025
EU countries	(-2.203)	(0.728)	(-1.817)	(1.147)	(-4.938)	(1.241)
	9,106,126	10,711,154	6,171,924	4,176,738	10,578,640	15,675,761
Non-leakage sectors,	0.009	0.203	0.238	0.055	0.083	-0.010
non-EU countries	(0.119)	(1.417)	(1.406)	(1.463)	(2.837)	(-1.232)
	2,704,971	1,993,512	2,327,732	382,024	2,793,897	3,067,332

FE-IV estimates per Eq. (1) by subsample. Subsample grouping variables are defined in Table A.1. Firm- and year-fixed effects included in Models (1)-(5). Model (6) includes sector-, country-, and year-fixed effects and the controls are specified as the pre-exit firm average. Presented are, respectively, the coefficient estimates, Wild bootstrapped t-values robust to clustering at both the sector and country level (see Section 2.1) in parentheses, and the number of observations (N). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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