Does the EU Emissions Trading System induce investment leakage? Evidence from German multinational firms

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ABSTRACT

This study exploits the incomplete participation requirements of the European Union Emissions Trading System (EU ETS) to investigate the policy's causal effect on outward foreign direct investment (FDI) decisions of German multinational firms. Using a combination of difference-in-differences with bias-corrected matching, our baseline specification indicates that the sample average treatment effect is very small and levels out at −0.2%, but its standard error is large (0.16). Looking at a sub-sample of firms which can be considered as geographically more mobile because they are supposedly less capital-intensive, we find that a small number of EU ETS regulated firms have increased their FDI outside the EU by 52% ± 47% compared to a counterfactual scenario. Paradoxically, relocating firms neither operate in the targeted energy-intensive sectors, nor are they emission-intensive. The small emissions share of these footloose firms indeed indicates a limited potential for policy-induced leakage of emissions. On the extensive margin, we find that all EU ETS firms on average have increased the number of their affiliates outside the EU by 28% ± 24% relative to control firms. This causal change in network structures of multinational firms outside the EU is suggestive of endeavors undertaken by regulated firms to facilitate relocations in the future.

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1. Introduction

Environmental policies limiting emissions of greenhouse gas (GHG) are designed to regulate emissions-related negative externalities (Goulder, 2013; Schmalensee and Stavins, 2013). However, such policies do not come without a cost. It is commonly believed that environmental regulations "impose significant costs, slow productivity growth and thereby hinder the ability of [...] firms to compete in international markets" (Jaffe et al., 1995: 133). This debate has received renewed attention with the launch of an unprecedented carbon pricing experiment in Europe. In 2005, the European union (EU) unilaterally launched the first and largest mandatory carbon trading scheme of its kind — the EU Emissions Trading System (EU ETS). By putting a price on carbon emissions of firms in the energy

and manufacturing industry, the EU ETS has tightened environmental regulation in the EU. Unsurprisingly, there have been concerns that the unilateral policy initiative will cause multinational companies to shift the location of production to regions with weaker policies, thereby creating "carbon leakage" (e.g. Martin et al., 2014a), i.e. the fact that emissions are relocating with production (e.g. Babiker, 2005).

Relocation may happen via two main channels: in the short-run through changes in trade flows (Copeland and Taylor, 2004), and in the medium to long-run through foreign direct investment (FDI), i.e. production relocation (Keller and Levinson, 2002; Hanna, 2010). The latter channel is of particular importance as it leads to major discontinuous changes with a high degree of irreversibility in both emissions and production. In the presence of global externalities such as carbon dioxide (CO_2) emissions, policy-induced industrial relocation indeed not only bears negative economic implications (e.g. job losses, decreased income taxes), but it also weakens the environmental effectiveness of the policy. The device commonly used by policymakers to mitigate carbon leakage has been to compensate

firms that are deemed to be most adversely affected by the policy (Martin et al., 2014b). Leakage risks seem indeed a powerful argument to extract concessions from policymakers who are concerned that policy-induced economic impediments may cloud their re-election prospects (Martin et al., 2014a). However, evidence on whether the threat of relocation and leakage is in fact credible is remarkably weak (Branger and Quirion, 2014).

This study uses administrative firm-level data on outward FDI of German multinational firms to estimate the incidence of the EU ETS on investment leakage. Exploiting the incomplete participation requirements of the policy program, we provide a first causal analysis of the conventional prediction that the higher costs imposed on industries regulated under the EU ETS lead (Germany-based) firms to relocate via FDI. The investigation of this grand experiment in market-based climate policy (Goulder, 2013), contributes to better understanding the causal effects of environmental policy on industrial relocation and carbon leakage, which are of central importance in an international climate regime that is shifting towards a bottom-up architecture with asymmetric unilateral or regional policies (Nordhaus, 2015). Our study further enlightens the controversially debated question relative to the effectiveness of the current compensation mechanism to mitigate the relocation problem through free emissions permit allocation (Martin et al., 2014a, 2014b; Schmidt and Heitzig, 2014; Branger et al., 2015). As the European compensation scheme serves as a prototype for the design of emerging emissions trading schemes worldwide, providing empirical content to the political and theoretical discussions seems particularly valuable to policymakers, environmentalists and industrial representatives.

Rigorous ex-post evaluations of the EU ETS are emerging only gradually; this paucity is due partly to the lack of comprehensive microdata conducive to such evaluations (Martin et al., 2016). We use Germany as testbed and bring together two administrative micro-level data sources: (i) the firm-level panel data on direct investment of German firms provided by Deutsche Bundesbank, and (ii) the plant-level data from the European Union Transaction Log (EUTL), the official trading registry of the EU ETS. To our knowledge, this is the first time these two high-quality data sources have been combined to study relocation risks of the European climate policy.

There are at least two substantive reasons that motivate the choice in this study of Germany as testbed for evaluating whether ETS firms increased their outward FDI compared to non-ETS firms in the post-2005 period. First, Germany is Europe's largest economy[1] and concomitantly the largest emitter among ETS countries[2]. Second, the strong orientation of German firms towards export markets and their considerable FDI activity worldwide (UNCTAD data for the year 2014 indicate a share of 6.4% in the world outward FDI stock held by German firms) make Germany a particularly interesting case for testing the validity of widespread concerns that unilaterally regulating European firms would lead to the relocation of production outside the EU.

To identify the causal effect of the EU ETS on industrial relocation by German firms through FDI, we rely on design features unique to the program. Indeed, to achieve cost effectiveness, the EU has designed the program to include only industry-specific large installations. Firms operating smaller installations are not regulated. For instance, installations in the steel industry are included only if their production capacity exceeds 2.5 tons per hour. By contrast, the threshold for cement installations is set to 500 metric tons of production per day. This threshold-like inclusion mechanism makes

of the EU ETS a regulatory program with incomplete participation requirements. Such a setting allows applying quasi-experimental techniques that are well suited to assessing causal impacts of policy (Imbens and Wooldridge, 2009; Fowlie et al., 2012). The inclusion criteria imply that EU ETS and non-EU ETS firms can in principle be comparable with respect to their FDI behavior, but differ merely in the size of installations they operate (which determines firms' regulatory status). Thus, for a suitably constructed set of firms, the plant-level thresholds may produce a treatment assignment mechanism that mimics a randomized experiment at the firm-level (Calel and Dechezlepretre, 2016).

On this reasoning, our empirical strategy builds on a two-stage strategy for the estimation of treatment effects. The intuition is as follows: We first use a data-driven algorithm proposed by Crump et al. (2009) to restrict our analyses to a subset of firms that are similar on pre-2005 characteristics (e.g. firm size, operating performance, and degree of internationalization). For this restricted sample of firms (of the two regulatory regimes) with substantial overlap in observable pre-2005 attributes, it is less likely that some unobservable post-2005 shock (other than the EU ETS) could have had systematically different impacts on the FDI behavior of EU ETS and non-EU ETS firms. Despite visible improvements, results from this first step point to "surviving" traces of imbalances. In a second step, we therefore adopt a difference-in-differences (DID) with bias-corrected matching estimator à la Abadie and Imbens (2006, 2011) to robustly estimate our causal effects of interest. A DID specification allows us to account for any additional time-invariant unobservable firm heterogeneity even after matching.

Based on all EU ETS firms, we find no statistically significant effect of the EU ETS on outward FDI flows to countries not subject to the policy. More specifically, our baseline specification indicates that the sample average treatment effect is very small and levels out at −0.2 %, but its standard error is large (0.16). However, further analyses point to the existence of within-treatment variations in the relocation choice among regulated multinational firms. Looking at a subsample of regulated firms that operate in one of the more footloose[3] and less capital-intensive sectors, we find that these firms have increased their FDI outside the EU by 52%±47% compared to the counterfactual. The impact is even more pronounced for the subset of firms that are footloose and with a permit shortage. On the extensive margin, we find that all EU ETS firms on average have increased the number of their affiliates outside the EU by 28% ± 24% relative to control firms. This expansion of the subsidiary network outside the EU is suggestive of endeavors undertaken by regulated multinational firms to facilitate relocations in the future.

1.1. Related work

This study relates to two different strands of research. On the one hand, we broadly contribute to the "pollution haven" literature on the nexus of environmental policy and relocation decisions. Kellenberg (2009: 242) summarizes this body of literature as follows: "the empirical validity of pollution haven effects continues to be one of the most contentious issues in the debate regarding international trade, foreign investment, and the environment". One explanation for the mixed results relates to important econometric and data issues (Levinson and Taylor, 2008; Millimet and Roy, 2016). First, most previous studies fail to adequately control for heterogeneous firm behavior as well as unobserved industrial trends and country-level heterogeneity. Second, prior studies use broad measures of environmental stringency (e.g. Wagner and Timmins, 2009;

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[1] Eurostat; URL: http://ec.europa.eu/eurostat/statistics-explained/index.php/File:GDP_at_current_market_prices,__2003\T1\textbackslash%E2\T1\textbackslash%80\T1\textbackslash%9304_and_2012\T1\textbackslash%E2\T1\textbackslash%80\T1\textbackslash%9314_YB15.png (Accessed 26 January 2016).

[2] Eurostat; URL: http://ec.europa.eu/eurostat/statistics-explained/index.php/Greenhouse_gas_emission_statistics (Accessed 26 January 2016).

[3] Ederington et al. (2005: 92) argue and provide evidence that in the presence of important "transportation costs, plant fixed costs, or agglomeration economies", firms would tend to be less geographically mobile, or " footloose".

Bialek and Weichenrieder, 2015) or abatement costs (e.g. Keller and Levinson, 2002; Eskeland and Harrison, 2003) that are potentially correlated with other relevant FDI determinants. We take a step forward in fixing both issues. Our identification strategy (i) controls for firm, sector and country heterogeneity and (ii) exploits the variation in firm-level regulation created by the EU ETS in order to identify its causal effects on industrial relocation (see Greenstone, 2002; List et al., 2003a, 2013b; Hanna, 2010 for related approaches in a US context).

On the other hand, this study relates to the burgeoning policy evaluation literature on the impact of the EU ETS on firm competitiveness and carbon leakage, a special case of the pollution haven effect. The evidence from the existing literature has focused mainly on intensive-margin adjustments to production or employment (see Martin et al., 2016, for an overview); it suggests an absence of strong detrimental effects of the policy program (Chan et al., 2013; Petrick and Wagner, 2014 ; Wagner et al., 2014). Research establishing ex-post impact estimates for leakage effects caused by the EU ETS is by contrast just emerging.[4] The few existing empirical studies either focus on sectoral trade flows, i.e. operational leakage (Naegele and Zaklan, 2019) or intra-firm shifts of production, i.e. leakage within firms (Dechezleprêtre et al., 2015; aus dem Moore et al., 2019). These studies collectively report the absence of leakage effects. In a study more germane to ours, Borghesi et al. (2018) examine changes in the internationalization of regulated Italian firms induced by the policy. They find a positive but small effect of the EU ETS on the number and sales of non-EU subsidiaries of Italian multinationals. Similarly, it is often argued (particularly by industry associations) that there have been considerable investments leaking outside the EU (Alliance of Energy-Intensive Industries, 2015). We contribute to that debate by providing a first investigation into investment leakage as a channel for carbon leakage.

2. Emissions trading and firm relocation

2.1. The EU ETS

The EU ETS is the central pillar of European climate policy. The mandatory emissions trading program imposes a cap on the total volume of GHG emissions from approximately 11,000 heavy energy-using installations in 31 countries, accounting together for about 50% of GHG emissions in the EU (EC, 2015). It operates in the 28 EU member states and in the three European Economic Area-European Free Trade Association (EEA-EFTA) states, namely Iceland, Liechtenstein and Norway.[5] The program has been launched in 2005 with multiple commitment periods and successively more stringent emissions caps. (See Ellerman et al., 2016, for a comprehensive review of the EU ETS.)

The EU ETS covers mainly CO_2 emissions from (i) power stations and other combustion plants as well as (ii) a wide range of energy-intensive industrial activities.[6] To limit administrative costs (and its adverse effects on cost effectiveness; see, e.g. Stavins, 1995), the EU ETS is designed to include only large energy-intensive installations, as defined by activity-specific size criteria in the Emissions

Trading Directive.[7] First, specific process-related capacity thresholds have been defined for nine industrial activities — the production of iron and steel, cement, glass, lime, ceramics, mineral wool, pulp and paper, bulk organic chemicals, hydrogen and synthetic gas. For example, participation of steel plants is limited to installations with a production capacity exceeding 2.5 tons per hour. For cement installations, the relevant inclusion threshold is instead 500 metric tons of production per day. In addition, all combustion plants (mainly related to heat, electrical or mechanical energy production) with an annual thermal input exceeding 20 MW are covered by the EU ETS, regardless of the industry.

Given that the EU has been the first to put a price on carbon,[8] the European Commission (EC) acknowledges in the Emissions Trading Directive that "this could lead to an increase in greenhouse gas emissions in third countries where industry would not be subject to comparable carbon constraints (carbon leakage), and at the same time could put certain energy-intensive sectors and subsectors in the Community which are subject to international competition at an economic disadvantage". Such concerns have indeed translated into a central design element of the policy: the free allocation of permits (Böhringer and Lange, 2005). At least 95% and 90% of permits were required to be allocated freely in Phase I (2005–2007) and Phase II (2008–2012), respectively. Coupled with the fact that allocation rules were highly decentralized and based on historical emissions (grandfathering), industrial emitters received fairly generous allocations of free permits (Ellerman et al., 2016). In Phase III (2013–2020), the permit allocation has been centralized but, contrary to initial plans of transitioning towards full auctioning, free permits are still the dominant principle of allocation. A gradual phase out of free allocation from 80% in 2013 to 30% in 2020 has been agreed upon for the regulated industrial sectors. In lieu of historical emissions, the EU ETS now uses product benchmarks for the free allocation; these benchmarks are defined as the average GHG emissions of the 10% best performing installations in the EU producing a given product. However, 100% of benchmark allocations are granted for free to installations in sectors that are considered to be at risk of carbon leakage. Examples of sectors deemed to be at risk of carbon leakage are aluminum production (2742), manufacture of paper and paperboard (2112), and manufacture of flat glass (2611).

The Carbon Leakage Decision[9] defines leakage risk of a sector based on two quantitative criteria: carbon intensity (CI) and trade intensity (TI), both measured at the level of the 4-digit industry code. While the first criterion is used to gauge the cost burden of a sector imposed by full auctioning, the second is used to assess whether these costs can be passed through to product prices. A sector is considered at risk of leakage (and put on the official carbon leakage list) if (i) its CI is greater than 5% and its TI is greater than 10%, or (ii) either CI or TI is greater than 30%. Martin et al. (2014a, 2014b) show that most sectors are placed on the list on the basis of the TI criterion alone. Martin et al. (2014b) show that this is the main reason why

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[4] There is, however, a notable literature using computable general equilibrium (Böhringer et al., 2012) or partial equilibrium analysis (Demailly and Quirion, 2008) to assess, ex ante, the prevalence of carbon leakage. The results of these simulations differ significantly depending on parameterization and modeling assumptions, highlighting the need for empirical analysis.

[5] Romania and Bulgaria joined the EU ETS in 2007; Norway, Iceland and Liechtenstein (i.e. the three EEA-EFTA states) were included in 2008; Croatia joined in January 2013. We, therefore, label these states late joiners.

[6] Emissions from commercial aviation have also been included since 2012. The sector is, however, excluded in this study due to its special regulatory features.

[7] Directive 2009/29/EC of the European Parliament and of the Council amending Directive 2003/87/EC so as to improve and extend the greenhouse gas emission allowance trading scheme of the Community (2009) OJ L 140 (Emissions Trading Directive).

[8] Only New Zealand (2008) and Switzerland (2008), and more recently Kazakhstan (2013) and South Korea (2015) have enforced comparable mandatory pricing schemes. In addition, there are a number of subnational trading schemes. The Regional Greenhouse Gas Initiative (RGGI) was launched by states and provinces in the Northeastern US in 2009, followed by programs in California and Quebec in 2012. Since 2013/2014, there have been seven pilot ETS in provinces and municipalities of China. See Kossoy et al. (2015), for a comprehensive overview of existing and emerging carbon pricing schemes.

[9] Commission Decision 2010/2/EU determining, pursuant to Directive 2003/87/EC of the European Parliament and of the Council, a list of sectors and subsectors which are deemed to be exposed to a significant risk of carbon leakage (2010) OJ L 1/10 (Carbon Leakage Decision).

85% of GHG emissions from industry are exempt from the transition to auctioning.

2.2. Conceptual framework

In this section, we combine insights from the theoretical literature on cap-and-trade and on the relationship between environmental policy and plant location to uncover the conditions under which the enforcement of the EU ETS could lead to relocation choices of firms regulated by the policy.

To fix ideas, consider a two-country setting à la Motta and Thisse (1994) with country d being an EU ETS member state, while country f is not. Let firm i earn a profit π_{id} from the operation of plants in its home country d. If the firm relocates production to the foreign country f, it would earn a (gross) profit π_{if} and incur relocation costs κ. In this setting (Martin et al., 2014a), the decision for firm i to relocate or not production to country f hinges, c.p., critically on the difference between π_{id} and the resultant "net" profit from relocation $\eta_i = \pi_{if} - \kappa$. A profit-maximizing firm will relocate only if the difference $\pi_{id} - \eta_i$ is significantly negative; if true, we should observe a positive and significant increase in firm i's FDI activities in country f following on the climate policy shock. However, the firm will find it more judicious to continue its production activities only in country d if the differential profit proves either trivial or even significantly positive. It appears that controlling for market size and the pre-policy production costs, firm's relocation decision ultimately depends on two factors: (i) the incurred relocation costs κ; and (ii) the regulation effect on π_{id}.[10]

One might think of κ as capturing the cost of registering the production facility in country f and the cost of building the plant itself (Elliott and Zhou, 2013). Such sunk fixed relocation costs may render some firms or industries less "footloose", i.e. less geographically mobile. In particular, Ederington et al. (2005) demonstrate that firms in capital-intensive industries with large fixed costs are less likely to move production. This is because in such industries the costs of relocation – specifically, large sunk investments into a new plant – may outweigh the gains from relocating to a less stringent jurisdiction. Thus, firms in less capital-intensive industries that are often also less emission-intensive, such as machinery, should be more likely to be influenced by changes in environmental regulation than more traditionally emission- and capital-intensive industries such as cement (see also Kellenberg, 2009).

In turn, the profit impact of the cap-and-trade system in country d is ambiguous because the policy jointly affects costs and revenues through pass-through into product prices (Bovenberg et al., 2005; Goulder et al., 2010 ; Bushnell et al., 2013). This is illustrated in a simple but effective demand and supply setting for a competitive industry[11] in Fig. 1. To begin with, the EU ETS raises the cost of production because firms are now required to surrender a permit per unit of emissions. The higher marginal cost arising from (a) abatement opportunities such as switching to less carbon-intensive fuels (c_{switch}) and/or (b) the actual or opportunity cost of permits (c_{permit}) shift the supply curve upward. This causes firms to restrict the level of production and thereby to increase the equilibrium output price, which can generate rents to firms, in much the same way
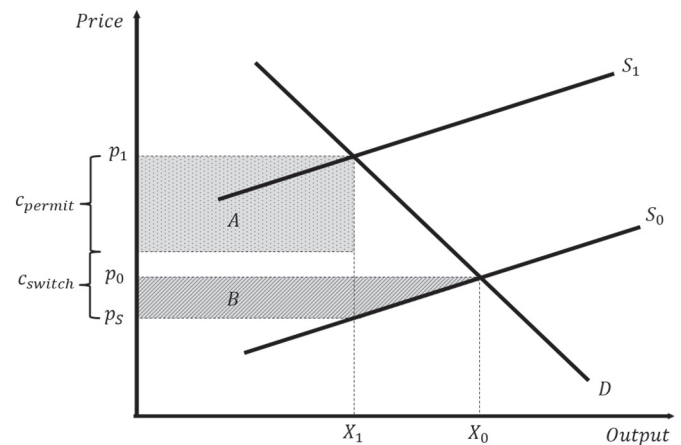

Fig. 1. The effect of cap-and-trade regulation on profits.
Source: Note: Adapted from Goulder et al. (2010).

an oligopoly generates rents by reducing output. Under free permit allocation, firms garner these rents (equivalent to the shaded rectangular area A) as "windfall profits" (Hepburn et al., 2013) that are likely to offset the gross loss of producer surplus (represented by area B). If all permits were auctioned instead, the rents would be bid away and the firm would experience losses of profits. However, as long as some permits are allocated for free, as is the case in the EU ETS, the policy might not only preserve firm's profits but it could also enable the firm to earn a higher profit than in the absence of the policy. The net profit effect ultimately depends on the elasticity of demand relative to the elasticity of supply, which determines the extent to which costs can be passed on to consumers (Goulder et al., 2010).

To summarize, the "relocation" case obtains from an EU ETS-induced net profit loss in d that exceeds in absolute terms the cost of relocating to f. Many EU ETS firms presumably face relatively high relocation costs because they operate in capital-intensive industries with high fixed plant costs (e.g. iron and steel, industrial chemicals, pulp and paper, cement). Because permits, in the EU ETS, are mainly allocated for free the policy can, in addition, generate rents that are very large in relation to compliance costs. As long as supply (demand) is sufficiently (in)elastic, profits in the regulated market will improve, which would induce the "no relocation" case. In the extreme, one may think of firms even reducing their outward FDI in non-EU ETS countries following the policy enforcement.

However, one might expect heterogeneous profit effects across firms due to cross-sectional variations in profit-preserving allocation. When a firm is a large emitter and faces a permit shortage (i.e. emissions exceed the number of freely allocated permits), it will have higher abatement needs and direct compliance costs; when the firm also sells in a market with relatively elastic demand, it is more likely that the firm's allocation is not profit-preserving. If such a firm, furthermore, operates in a more footloose industry with low fixed costs, it may be more easily influenced at the margin by profit changes in the regulated domestic market, precisely because its costs of relocation are relatively smaller. As a result, we expect relocation to be more likely for the subset of firms with deficit of allowances, relatively elastic demand and low capital intensity.

3. Data and research design

3.1. Data overview and description

To understand the relocation effects of the EU ETS, we bring together two administrative micro-level data sources, heretofore not

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[10] Note that total relocation is unlikely to be the optimal outcome to the extent that "firms are linked by historical, cultural and economic reasons to their mother country" (Motta and Thisse, 1994: 565). Firms may rather decide to partially relocate (i.e. open a new plant in f and still operate in d). The relocation decision will then depend on the difference between the profit of operating exclusively in d and the total profit of producing in d and f.

[11] Goulder et al. (2010) present an intertemporal equilibrium model that delivers similar results as the static graphical illustration. Bushnell et al. (2013) show that the main intuition remains the same for perfect and imperfect competition. Therefore, we focus on the case of perfect competition.

used to study the relocation risks of European climate policy. Relocation decisions take place at the level of the firm, while EU ETS regulations apply at the level of the installation. Consequently, we combine a panel (sample period: 1999–2013) of firm-level data on outward FDI stocks with a plant-level database detailing which firms operate any installations subject to the EU ETS.

3.1.1. Foreign direct investment data

The primary data source used in this study is the confidential, anonymized Microdatabase Direct Investment (MiDi) gathered by the Deutsche Bundesbank in accordance with the provisions of the Foreign Trade and Payments Regulation ("Aussenwirtschaftsverordnung"). [12] The MiDi is based on a legally mandated annual survey on firm-to-firm FDI stock relations that covers the universe of German firms with directly and indirectly owned foreign corporate holdings above certain reporting thresholds (minimum ownership share and balance sheet total[13]). We obtain (from the MiDi database) information on the countries in which multinational firms are active, the volume of their outward FDI activity in Euro, as well as firm and industry characteristics of both the parent and affiliate company, including three-digit NACE 2 sector classifications. Since the FDI records are subject to comprehensive quality checks and data editing under the supervision of Deutsche Bundesbank (Schild and Walter, 2015), they are presumably free of measurement error compared to existing commercial databases.

The entire MiDi database consists of 367,032 annual parent-affiliate-location records for outward FDI in the sample period 1999 to 2013. To reduce dimensionality, we aggregate the foreign affiliate records of a German investor in a country per year, i.e. we transform the data to 277,305 annual parent-location observations.[14] We have three main sample restrictions. First, as we are interested in the relocation effect of the EU ETS, we exclude any FDI activity within countries of the European Economic Area, where the EU ETS is in place. Second, our sample includes only firms that operate in those sectors, in which at least one company is regulated by the EU ETS. Third, we limit the sample to firms with at least one annual FDI record both in (i) the period prior to the introduction of the EU ETS (1999–2004) and (ii) the period after the policy program has been started (2005–2013). The pre- and post-treatment records may relate to an investment or divestment flow and to different affiliates/investee countries. The existence of at least one pre-policy record is a necessary condition for matching on pre-2005 characteristics to the extent that the MiDi provides no firm characteristics for firms that have never invested abroad. We accordingly impose the existence of a post-policy record.

Applying these sample restrictions results in 27,248 annual parent-location observations. As we are interested in comparing FDI outcomes before and after the enforcement of the regulation (i.e. the identifying variation occurs in 2005), we then collapse the annual data, which includes zeros, investment and divestment flows, into a pre-treatment (1999–2004) and post-treatment (2005–2013) period by taking the average of the imbalanced annual parent-location FDI records over the two respective periods. This leaves us with 2633 parent-location FDI observations (relating to 1033 German multinational firms) in both periods across 20 sectors and 58 investee
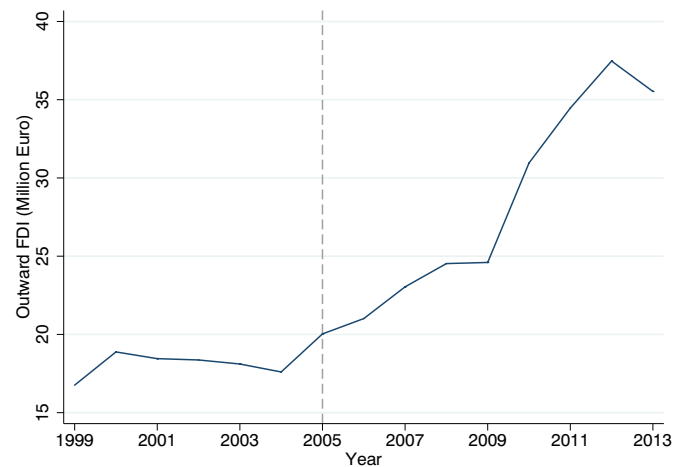

Fig. 2. FDI over time.
Source: Source: Research Data and Service Centre of Deutsche Bundesbank, Microdatabase Direct investment (MiDi) 1999–2013, own calculations.

countries. Thus, our unit of observation is an outward FDI flow from a German investing firm to a non-EU ETS country before and after the enforcement of the EU ETS. In a slight abuse of language, we hereafter refer to our firm-country observations as "firm observations".

Given the data at hand, our empirical analysis captures both channels through which the EU ETS may impact foreign investment choices. First, it captures whether a firm that already produces in a non-EU ETS country pre-2005 will increase its foreign investment activities in this country in response to regulation (intensive margin). Second, it captures whether a firm will start investing more in non-EU ETS countries once it becomes regulated (extensive margin). However, it is important to note that the MiDi does not include firms that never invested abroad.[15] Thus, the estimated regulation effect derived in this paper provides evidence for the response of multinational firms to the EU ETS. This estimate is most likely an overstatement for the entire universe of German firms. While this is a limitation of the study, from a policy standpoint, it is precisely the multinational response to regulation we are concerned about. What is more, it seems fair to argue that other (purely domestic) firms tend to have relatively high barriers to foreign production and that the regulatory burden is unlikely to cause them to move abroad.

Fig. 2 shows the average outward FDI over time. While FDI activity was relatively constant in the early 2000s, investments in non-EU ETS countries has increased rapidly after 2005 (from about 20 million to 35 million Euro). The upsurge in investment activity, therefore, appears to coincide with the launch of the EU ETS.

The observed patterns of investment decisions could, however, be a common phenomenon among Germany-based energy and manufacturing multinationals caused by other factors than the EU ETS, such as energy prices and macroeconomic conditions. Therefore, we subsequently compare the FDI outcome of firms regulated under the EU ETS with those of firms that are not covered by the regulation. However, such a comparison requires additional data relative to the regulatory status of the sample firms. These data are discussed below.

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[13] Since 2007: all firms or individuals who own directly (indirectly), at least 10% (50%) of the shares/voting rights in a foreign company that has a balance sheet total of more than 3 million Euros.

[14] Investigating relocation patterns within the network structure of foreign affiliates related to multinational firms goes beyond the scope of this paper. Please, refer to, e.g., Dechezleprêtre et al. (2015) who study the distribution of carbon emissions within multinational firms across countries and over time. See also aus dem Moore et al. (2019).

[15] Similarly, the exclusion of firms (a) that only become non-EU ETS investors post-2005 and (b) that only invest in non-EU ETS countries pre-2005 is an indispensable methodological choice in our DID study design. Reassuringly, the loss of observations is fairly limited. We effectively lose 12 EU ETS firm-country observations due to restriction (a) (there is also no firm information available based on FDI in EU countries) and 141 EU ETS firm-country observations due to restriction (b).

3.1.2. Regulatory data

To determine the regulatory status of firms, we draw on plant-level data from the European Union Transaction Log (EUTL), the official trading registry of the EU ETS.[16] The EUTL database contains information about each individual installation regulated under the EU ETS. Each installation is associated with exactly one operating account holder, but one operating account holder might own several installations. The availability of detailed account holder information (name, address, trade registry number) allows us to identify identical firms in the EUTL and MiDi database. A string matching algorithm based on Schild and Schultz (2016) is used to link EUTL installation operators to the MiDi investing companies; this algorithm is run by the Research Data and Service Centre of Deutsche Bundesbank. From the complete list of 1008 German EU ETS firms (excluding hospitals, universities and other non-corporates), 232 EU ETS firms can be mapped to an investing firm in the MiDi panel. 547 of our 2633 parent-location FDI observations relate to these 232 regulated EU ETS firms. Together they operate 840 installations, accounting for 55% of regulated German emissions (which represents roughly 13% of emissions EU ETS-wide). Note that our sample is strongly dominated by manufacturing firms. Just about 1% of our sample observations relate to "electricity, gas, steam and air conditioning supply". The underrepresentation of the particularly emission-intensive utility sector partly explains the emissions share. Based on a rigorous pre-testing of the matching procedure, we are confident that the rest of account holders most likely could not be matched to any entry in the MiDi because these firms do not invest abroad. Appendix A briefly discusses the Schild and Schultz (2016) matching algorithm and quality checks pertaining to linking the MiDi and the EUTL database.

Fig. 3 shows the average outward FDI trends among the 547 EU ETS and 2086 non-EU ETS participants. Both trajectories of FDI activity are relatively constant in the five years before the enforcement of the policy program. After the introduction of the policy regime in 2005, investments outside the European Economic Area appear to have risen significantly faster among regulated firms than among unregulated firms. However, the divergence seems to have unfolded with a delay in 2006, and, the second, more stringent trading period of the EU ETS may have provided a particular impetus to investments of regulated firms.

The revealed differences in Fig. 3 reflect the causal impact of the EU ETS, if we were to assume that the investment behavior of
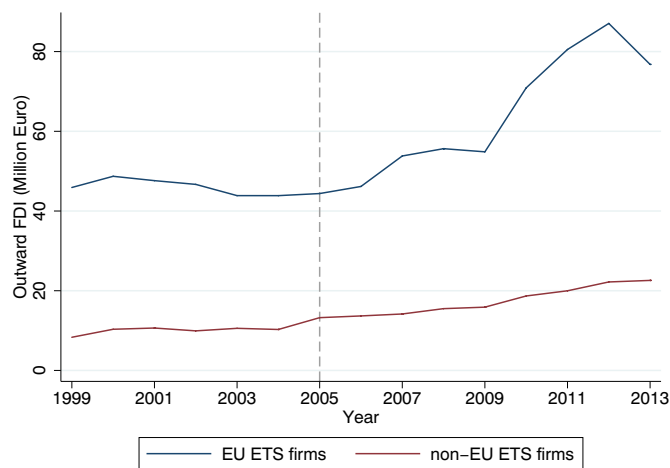
Fig. 3. FDI by EU ETS and non-EU ETS firms.
Source: Source: Research Data and Service Centre of Deutsche Bundesbank, Micro-database Direct investment (MiDi) 1999–2013, own calculations.

Table 1
Summary statistics for pre-treatment variables.
Source: Source: Research Data and Service Centre of Deutsche Bundesbank, Micro-database Direct investment (MiDi) 1999–2013, own calculations.

	Non-EU ETS ($N_c = 2086$)		EU ETS ($N_t = 547$)	
	Mean	S.D.	Mean	S.D.
FDI_{t_0}	8.46	24.33	38.1	143.66
$Total\ assets_{t_0}$	1.98	9.07	10.58	14.41
$Sales_{t_0}$	0.95	4.41	9.22	15.07
# of $countries_{t_0}$	7.63	10.3	21.31	15.18
# of $affiliates_{t_0}$	16.01	42.15	74.79	88.86

Note: Table 1 presents summary statistics for the covariates in the pre-treatment period t_0 (1999–2004). FDI_{t_0} is in million Euro. $Total\ assets_{t_0}$ and $Sales_{t_0}$ are in billion Euro.

non-EU ETS firms provides a valid counterfactual estimate of how EU ETS firms would have invested had they not been regulated. However, this assumption seems highly questionable. A closer inspection of Fig. 3 reveals that the two groups of firms are significantly different even before the introduction of the EU ETS, as suggested by the considerably larger FDI activity of EU ETS participants in the pre-2005 period. Evidently, EU ETS participants are more international and more prone to invest abroad than non-regulated firms. In this light, we cannot preclude the possibility that some unobservable shock (other than the EU ETS) could have driven the different FDI trajectories of EU ETS and non-EU ETS firms.

3.1.3. Descriptive statistics

The MiDi dataset contains further information on German investing firm characteristics that are potentially related to both the outcome of interest (outward FDI) and the likelihood of being regulated under the EU ETS. We have data on (i) the size of firms in terms of total assets, (ii) the operating performance of firms as measured by sales, and (iii) two measures of the degree of internationalization. First, we use the number of non-ETS countries in which the firm has affiliates as one of the internationalization measures to capture the dispersion of a firm's international operations (e.g. Aabo et al., 2015). We call this measure the breadth of internationalization. Second, we use the number of affiliates in non-ETS countries, and denote that measure the depth of a firm's internationalization (e.g. Hutson and Laing, 2014). While the two measures might be correlated, they capture different aspects of a firm's international operations. To see this, imagine that a firm holds 40% of its assets abroad and is concentrated in a single country; another otherwise comparable firm with the same proportion of assets abroad might operate in many countries and regions. As a result, FDI behavior of these two firms might respond differently to shocks.

Table 1 presents summary statistics for the FDI and covariate data in the pre-treatment period t_0 (i.e. prior to the 2005 launch of the EU ETS). For each variable we report the mean and standard deviation by treatment group (i.e. non-EU ETS and EU ETS firms).

Table 1 reveals that there are substantial differences between the distributions of all pre-treatment variables in the two sets of firms. Prior to being regulated under the EU ETS, EU ETS firms tend to be significantly larger, more profitable, more international, and more prone to invest abroad than non-EU ETS firms. This highlights the major role of selection on observable firm characteristics that need to be accounted for in any research design evaluating the differences in foreign investments across EU ETS and non-EU ETS participants. The lack of covariate balance can make inferences about causal effects both imprecise and sensitive to ostensibly minor changes in the methods and specifications used. Therefore, we need to remove visible pre-treatment differences by some kind of adjustment, as we subsequently discuss in our empirical strategy.

3.2. Research design

The aim of this study is to estimate the causal impact of the EU ETS (hereafter the treatment) on outward FDI decisions of Germany-based firms over the period from 2005 to 2013, which is orthogonal to any indirect effects of the program on both participating and non-participating firms. However, as in any observational study and unlike randomized experiments, we lack control of treatment assignments, which might severely restrict our ability to lend comparisons of control and treated firms a causal interpretation (Imbens, 2015). To mitigate this issue, our empirical strategy builds on the two-stage strategy proposed in Imbens and Rubin (2015) for the estimation of robust causal effects of treatments in observational studies. These stages are labeled the design stage and the causal analysis stage, respectively.

In what follows, we turn to the potential outcome framework that is commonly used in policy evaluation[17] and sequentially elaborate on each of the two stages. We subsequently use log transformed data in order to constrain the treatment effect to be proportional to the firm's foreign investments, which ensures that the magnitude of the EU ETS effect is not simply driven by the largest firms.[18]

3.2.1. Setup and notation

The analysis is based on a sample of N firm observations, indexed by $i = 1, \ldots, N$. For each observation there are two potential outcomes, $Y_i(0)$ and $Y_i(1)$, i.e. the outward FDI of firm i in the post-2005 period without and given the EU ETS. Let W_i be the treatment indicator that is 1 if firm i participates in the EU ETS, i.e. is treated, and 0 otherwise. In addition, we have a variety of observable firm characteristics (Table 1) that are collected in the 5-dimensional vector X_i. These covariates are unlikely to be affected by treatment because they are measured prior to the introduction of the EU ETS. Thus, for each unit i in the sample, we have the triple (Y, W, X).

3.2.2. Stage 1: Design

The design stage aims at improving overlap in covariate distributions. This objective is attained by selecting from the original data a subsample that is more suitable for estimating causal effects. This selection serves the purpose of making any subsequent causal analysis, irrespective of the estimation method, more robust, and thus more credible (Imbens and Rubin, 2015). Just as in the design phase of a randomized study, an important feature of the design stage is that it is entirely based on pre-treatment data (X, W) and makes no use of outcome data (i.e. FDI flows post-2005). The result of this first stage is a sample, $(Y_S, W_S, X_S) = f(Y, W, X)$ with the sample size in the selected sample, N_S less than or equal to N.

Crump et al. (2009) suggest a straightforward way of constructing such a trimmed subsample. It is consistent with the common practice of dropping observations with estimated propensity score values close to zero or one. In our case, the propensity score, denoted $\hat{e}(x \mid X, W)$ reflects the firm's probability of participating in the EU ETS given observable pre-treatment characteristics X_i. Crump et al. (2009) suggest discarding all observations with estimated propensity scores $\hat{e}(x \mid X, W)$ outside the interval $[\alpha, 1 - \alpha]$, where the cut-off value α determines whether the propensity score estimate is too close to zero or one. In these cases, it is difficult to obtain precise estimates of the treatment effect. As a result, we put aside firms with such propensity scores. Rather than using an ad hoc cut-off value (e.g. Dehejia and Wahba, 1999, 2002), the threshold is chosen based on some optimality properties that allow for more precise estimation of

Table 2
Sample sizes for trimming based on estimated propensity score ($\hat{\alpha} = 0.0897$).
Source: Source: Research Data and Service Centre of Deutsche Bundesbank, Microdatabase Direct investment (MiDi) 1999–2013, own calculations.

	$\hat{e}(X) < \hat{\alpha}$	$\hat{\alpha} < \hat{e}(X) < 1 - \hat{\alpha}$	$1 - \hat{\alpha} < \hat{e}(X)$	All
Non-EU ETS	1668	418	0	2086
EU ETS	36	313	198	547
All	1704	731	198	2633

the average treatment effect (in terms of minimum asymptotic variance). More specifically, the optimal selection rules depend solely on the marginal distribution of the propensity score.

To implement the approach, we first estimate the propensity score. We use a data-driven algorithm for choosing among the possible linear, quadratic and interaction terms. It involves step-wise logistic regressions where the number (and functions) of the covariates entering into the logistic regression model is chosen sequentially. Details on the specification algorithm and the parameter estimates are provided in Appendix B. Second, given the estimated propensity score, we estimate the threshold value $\hat{\alpha} = 0.0897$. There are 1704 firm observations with estimated propensity scores less than 0.0897 (mainly non-EU ETS firms), and 198 units with estimated propensity scores exceeding 0.9103 (only EU ETS firms), which leaves us with 731 observations in the trimmed sample. Table 2 shows the subsample sizes by regulatory status and propensity score value.

For the trimmed sample, we recalculate the averages by treatment group, including two additional measures of overlap for each variable: (a) the difference in means by treatment group, normalized by the square root of the average of the two within-group variances;[19] (b) the proportion of treated firms with a covariate value outside the 0.025 and 0.975 quantiles of the covariate distribution for control firms.[20] The results in Table 3 show that trimming substantially improves the covariate balance. All normalized differences are significantly smaller in the trimmed sample than in the full sample. For example, the normalized difference for the variable $Total\ assets_{t_0}$ decreased from 1.94 in the full sample to 0.51 in the trimmed sample. This is because the trimming procedure discards units that tend to have relatively extreme values for some of the covariates, such as $Sales_{t_0}$ (see last two columns of Table 3). The trimmed sample is, therefore, more likely to lead to robust estimates. Despite these dramatic improvements in balance, it is noteworthy that three variables still exhibit some degree of imbalance that requires careful control in the subsequent treatment effect estimation. Given the magnitude of imbalances, linear regression methods will not reliably remove the bias in our case.[21] However, the low coverage proportions in the last column of Table 3 suggest that there is substantial overlap in the central ranges of the variable distributions. This implies that adjustments for covariate differences are in principle feasible and that more sophisticated methods may lead to credible results.

An important consequence of trimming the sample is that some external validity is sacrificed by changing the focus to average treatment effects for a subset of the original sample. The advantage,

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[17] See Imbens and Wooldridge (2009) for a survey and a discussion of the history of this framework.

[18] We use the neglog transformation of Whittaker et al. (2005) to handle negative FDI flows.

[19] It is defined as $\Delta_{ct} = \frac{\bar{X}_t - \bar{X}_c}{\sqrt{s_c^2/N_c + s_t^2/N_t}}$, where $\bar{X}_c$ and $\bar{X}_t$ denote the sample averages of covariate values by treatment group, N_c and N_t are the number of control and treated units, and s_c^2 and s_t^2 are the within-group sample variances of the covariates. We focus on the normalized difference, rather than on the t-statistic, because the former provides a scale and sample size free way of assessing overlap.

[20] It is defined as $\pi_t^{0.05} = \left(1 - \left(\hat{F}_t\left(\hat{F}_c^{-1}(0.975)\right)\right) + \hat{F}_t\left(\hat{F}_c^{-1}(0.025)\right)\right)$, where $\hat{F}_c$ and $\hat{F}_t$ are the empirical distribution function of the covariate in the control and treated sample, respectively. While our choice of $\alpha = 0.05$ is for convenience, this choice is common in the related literature (see e.g. Imbens and Rubin, 2015).

[21] Imbens and Rubin (2015) suggest as a rule of thumb that with a normalized difference exceeding one quarter, linear regression methods tend to be sensitive to the specification.

however, is the improvement in internal validity: causal effect estimates in the well-defined, trimmed sample are likely to be more credible and precise than estimates for causal effects in the original, full sample. Reassuringly, the primacy of internal validity over external validity is a general theme in the literature (Imbens and Rubin, 2015).

3.2.3. Stage 2: Causal analysis

With a more appropriate subsample of EU ETS and non-EU ETS in hand, in the second stage the outcome data Y are used and estimates of the average treatment effect of interest are calculated, $\hat{\tau} = \hat{\tau}(Y_S, W_S, X_S)$. We are primarily interested in the average treatment effect of the EU ETS on participating firms, that is, the average treatment effect on the treated (ATT)

$$\tau|_{W=1} = E[Y_i(1) - Y_i(0) \mid W_i = 1] \tag{1}$$

The "fundamental problem of causal inferences" (Holland, 1986) is to construct a precise and tenable estimate of $E[Y_i(0) \mid W_i = 1]$, that is, the FDI behavior of a treated firm in the absence of the EU ETS.

As discussed above, the simplest and most naïve counterfactual is provided by the incomplete program participation requirements of the EU ETS, which provide two natural comparison groups of firms. However, we have shown that regulated and unregulated firms differ in several other characteristics apart from treatment status. The assignment of firms to the EU ETS is clearly not random.

Therefore, we resort to a combination of Difference-In-Differences (DID) with bias-adjusted matching on observed covariates. The important advantage of the DID matching approach is that it uses pre-policy outcomes to efficiently disentangle the effect of the EU ETS from that of confounding factors. Matching captures selection due to observable firm characteristics and/or actions. It helps us to find a control group that is most similar to each treated firm in various pre-policy underlying attributes. A key concern with matching on observables is, however, that we may observe only a subset of conditioning variables. Particularly, due to unobserved firm-specific heterogeneity our data may be contaminated by temporally-persistent differences between the treatment and the control groups. DID, by differencing post-policy outcomes with respect to pre-policy outcomes, gets rid of any additional time-invariant unobservable differences between comparison sets. Thus, the DID extension of matching is particularly effective in reducing bias (Heckman et al., 1998, 1997) ; we subsequently explain its implementation.

Let t' and t denote the time period before and after the program. The conditional DID estimator compares the before-after FDI of EU ETS participants with those of non-participants conditional on a set of observable covariates X (Heckman et al., 1997):

$$\tau_{DID}|_{W=1} = E[Y_{it}(1) - Y_{it'}(0) \mid X_i, W_i = 1] - E[Y_{it}(0) - Y_{it'}(0) \mid X_i, W_i = 0] \tag{2}$$

To estimate this quantity, for each before-after FDI change in the EU ETS sample, a weighted average of before-after changes in the non-EU ETS sample is formed and then the average impact is given by

$$\hat{\tau}_{DID}|_{W=1} = \frac{1}{N_1} \sum_{j \in I_1} \left\{ (Y_{jt}(1) - Y_{jt'}(0)) - \sum_{k \in I_0} w_{jk} (Y_{kt}(0) - Y_{kt'}(0)) \right\} \tag{3}$$

where I_1 denotes the set of EU ETS participants, I_0 denotes the set of non-participants, and N_1 is the number of firm observations in the treatment group. In this specific equation, the EU ETS firms are indexed by j; the non-EU ETS firms are indexed by k. w_{jk} is the weight

attached to control firm k when constructing the counterfactual estimate for the treated firm j. The intuition behind the weighting is that control firms that most closely resemble the treatment firms in terms of the covariate space X are weighted more strongly.

The specific weighting in our analysis is based on a non-parametric nearest neighbor matching estimator. With m selected neighbors for each EU ETS firm, the w_{jk} are set to $1/m$ for the selected neighbors and zero for all other members of the non-EU ETS control group. Following Abadie and Imbens (2006, 2011), we augment the non-parametric matching estimation with a regression-based bias adjustment in order to mitigate any bias introduced by poor matching quality. More specifically, after matching the treated firms with m nearest neighbors, within-pair differences are adjusted using a parametric regression of the control outcome on X (see Abadie and Imbens, 2006, 2011). For simplicity, we refer to this estimator as the Abadie-Imbens (AI) estimator.

The main identifying assumption that lends a causal interpretation to our estimates below is that there exist no unobserved variables that simultaneously influence changes in FDI and the probability of being regulated by the EU ETS. In addition to this unconfoundedness assumption, we must rule out the possibility of spillover effects from regulated to unregulated firms (stable unit treatment value assumption). We discuss and assess the tenability of these identifying assumptions in Appendix D.

The observable covariates that we use to match similar firms include the pre-policy (1999–2004) FDI level, total assets, sales, the number of non-EU ETS foreign affiliates and the number of countries in which the firm operates. These variables are likely important determinants of firm's available resources and capacity to invest abroad in response to the EU ETS. To improve covariate balance, the matches were also penalized for dissimilarities in the square of sales. Matching is performed with replacement to reduce the asymptotic bias that arises if the covariate vector contains more than one element (Dehejia and Wahba, 2002).

An important feature of our matching procedure is that we impose an exact match on the 3-digit sector classification and on the investee country code. We prioritize sector classification because industry attributes are likely correlated with unobserved determinants of firm FDI including factor intensities, intangible assets, or technological characteristics. We also require that treated and control firms invest in the same country to capture traditional explanatory factors related to location characteristics such as GDP/distance, infrastructure/factor endowment, and political environment (Copeland and Taylor, 2004). Thus, if matched firms operate in the same country and economic sector, they are arguably exposed to the same business and regulatory environment as well as country and sector specific shocks and trends.

Finally, the MiDi dataset also allows us to identify majority ownership.[22] Using this information, we exclude non-EU ETS firms with ownership ties to an EU ETS firm. This reduces the chance of matching two potentially dependent observations.

4. Results

In this section, we primarily present our treatment effect estimates obtained from applying the DID with bias-corrected matching estimator à la Abadie and Imbens (2006, 2011) to the trimmed sample as discussed in the design stage. We also discuss results relative to the original, full sample (Section 4.1). To complete the picture, we test for heterogeneity in the treatment effect estimates (Section 4.2).

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[22] We identify ownership ties using two identifiers of Deutsche Bundesbank: Ultimate Beneficial Owner (UBO) which is used until 2010, and Ultimate Controlling Institutional unit of a foreign affiliate (UCI) which is the relevant concept since 2011 (Schild and Walter, 2015).

Table 3
Covariate balance in full and trimmed sample (logs).
Source: Source: Research Data and Service Centre of Deutsche Bundesbank, Microdatabase Direct investment (MiDi) 1999–2013, own calculations.

	Full sample				Trimmed sample				Discarded	
	Mean Non-EU ETS ($N_c = 2086$)	Mean EU ETS ($N_t = 547$)	Normalized difference	Coverage proportion	Mean Non-EU ETS ($N_c = 418$)	Mean EU ETS ($N_t = 313$)	Normalized difference	Coverage proportion	Mean $\hat{e}(X) < \hat{\alpha}$	Mean $1 - \hat{\alpha} < \hat{e}(X)$
FDI_{t_0}	7.90	8.91	0.44	0.10	8.39	8.73	0.14	0.05	7.79	9.30
$Total\ assets_{t_0}$	11.96	15.22	1.94	0.00	14.27	15.06	0.51	0.01	11.41	15.92
$Sales_{t_0}$	11.33	14.82	1.77	0.11	13.82	14.50	0.43	0.03	10.74	15.74
$\#\ of\ countries_{t_0}$	1.72	2.82	1.30	0.00	2.60	2.79	0.22	0.00	1.51	3.03
$\#\ of\ affiliates_{t_0}$	1.96	3.65	1.44	0.06	3.13	3.57	0.36	0.02	1.68	4.04

4.1. Average treatment effects

As a "sniff-test", we start the discussion by plotting, side by side, the average FDI of the matched treated and control firms (in logs) spanning the period before (1999–2004) and after (2005–2013) the enforcement of the EU ETS. Fig. 4 shows that our research design has produced a subset of treated and control firms that exhibit sensibly homogeneous FDI trajectories in the pre-treatment period (at least until year 2003). While the FDI patterns of treated firms and control firms seem to diverge as from 2004 potentially due to anticipation of regulatory changes, Fig. 4 also reveals that the matched treated and control firms do not exhibit different trends prior to 2005. Consequently, the common support assumption is upheld by the data in this study. Notably, the divergence in FDI trajectories of the two treatment arms in the treatment period, especially in the years 2007 and 2013, remains limited.
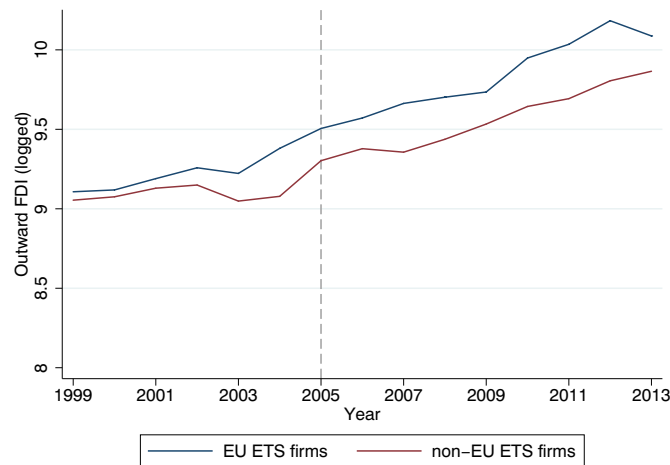
In the ensuing lines, we now more formally examine the intuition gleaned from Fig. 4. Estimation results are shown in Table 4. They are structured around two panels: the first panel pertains to the trimmed sample obtained from the design stage, while the second panel is used for comparison purposes and relates to the full sample (i.e. without applying the design stage). Common to both panels in Table 4 is that each of them contains two estimations: the first is based on a single match ($m = 1$); the second uses three matches ($m = 3$) for each treated firm observation. Using only one control firm observation is more likely to yield an unbiased estimate of the treatment effect, albeit at the cost of sacrificing some precision (Imbens and Wooldridge, 2009: 36). However, the

sampling precision gain from using multiple matches is limited and Imbens and Rubin (2015: 425) warn against going beyond two or three matches. Thus, our baseline estimation pertains to the trimmed sample and uses a single match.

Our baseline specification (first entry in Table 4) shows that the treatment effect amounts to $-0.2\,\%$ and is insignificant. The contrast to the intuition gained from Fig. 4 mainly stems from imperfect matching since the apparent difference in FDI across the two treatment arms disappears after regression adjustment (see Appendix Table C1). When we increase the number of matches to 3 ($m = 3$), the differential treatment estimate between our treated and control firms rises to 0.9% but remains insignificant at conventional levels.[23] Because we report results generated using log transformed data in a DID setting, the sample ATT can be interpreted as the average differential treatment effect in percentage terms. While the range of AI estimates from $-0.2\,\%$ to 0.9% suggests that the differential treatment effect over the 2005–2013 period is substantively unimportant, the corresponding confidence interval of these point estimates is fairly large.

Is this conclusion attained on the basis of the trimmed sample valid for the original, full sample of EU ETS firms, i.e. has our pre-processing method had a significant impact on the estimation of the treatment effect? With $m = 1$, we observe a statistically insignificant treatment effect on the treated of 6.8% in the full sample. In the specification that uses three matches, the treatment effect remains statistically insignificant and decreases to 4.4%. Despite the differences in both the magnitude and sign as compared to the trimmed sample, the estimates from the full sample suggest that concerns about external validity do not impair our earlier conclusion.[24] In Appendix Table C3, we further show that the inferences attained hitherto remain robust when we utilize an alternative estimator based on the estimated linearized propensity score.

Note, however, that the causal effect estimates in the well-defined, trimmed sample are likely to be more credible and precise than estimates for causal effects in the full sample. The difference becomes apparent if we compare the matching estimator performance in terms of balancing the covariates in the trimmed sample versus the full sample (Appendix Table C2). When we combine trimming with matching, all normalized differences are remarkably low. In particular, our baseline specification with a single match successfully eliminates the persistent traces of imbalance that we still observed (for total assets, sales, and number of affiliates) in Table 3 of the design stage. If we instead only match the full sample, all covariates except pre-treatment FDI remain highly unbalanced

Fig. 4. Logged FDI by matched EU ETS and non-EU ETS firms.
Source: Source: Research Data and Service Centre of Deutsche Bundesbank, Microdatabase Direct investment (MiDi) 1999–2013, own calculations.

[23] If we exclude firm observations that relate to "electricity, gas, steam and air conditioning supply" sector, the treatment effect with $m = 1$ levels out at $-3.7\,\%$ ($t = -0.24$) and with $m = 3$ at $-0.1\,\%$ ($t = -0.01$).
[24] Appendix Table C1 also demonstrates that our results are not overly sensitive to the bias adjustment.

Table 4
Main estimates of treatment effect on outward FDI.
Source: Source: Research Data and Service Centre of Deutsche Bundesbank, Micro-database Direct investment (MiDi) 1999–2013, own calculations.

	Trimmed sample		Full sample	
	$N_t = 313$		$N_t = 547$	
	$N_c = 418$		$N_c = 2086$	
	Est.	Std. Err.	Est.	Std. Err.
$m = 1$	−0.0016	(0.152)	0.0684	(0.132)
		[0.160]		[0.159]
$m = 3$	0.0092	(0.132)	0.0444	(0.128)
		[0.156]		[0.171]

Note: Table 4 presents mean differences between the treatment and control groups. m denotes the number of matches used per treatment observation. N_t (N_c) stands for the number of observations pertaining to the treated (control) firms. All the specifications in this table include a linear bias adjustment and use the inverse of the variance-covariance matrix in the vector norm (Mahalanobis). The estimations are collectively performed with exact matching on 3-digit sector dummies and non-ETS country in which the FDI takes place. The vector of covariates in the AI estimations also contains the following pre-treatment variables (averages): FDI, total assets, sales, sales squared, number of non-EU ETS countries in which the regulated firm operates, and number of affiliates in non-EU ETS countries. Standard error estimates reported in brackets are constructed using the variance formula proposed by Abadie and Imbens (2006). Cluster-robust standard errors at the parent firm level as per Hanson and Sunderam (2012) are reported in squared brackets.

Table 5
Estimates of treatment effect on degree of internationalization.
Source: Source: Research Data and Service Centre of Deutsche Bundesbank, Micro-database Direct investment (MiDi) 1999–2013, own calculations.

	Est.	Std. Err.
Panel A: Outcome = # of affiliates		
$m = 1$	0.282**	(0.12)
$m = 3$	0.199*	(0.11)
Panel B: Outcome = # of countries		
$m = 1$	0.193*	(0.10)
$m = 3$	0.129	(0.10)

Note: Table 5 presents mean differences between the treatment and control groups. To obtain these results we aggregate the firm-country observations of the more balanced trimmed sample to firm observation. We then performed AI estimates with a vector of covariates containing the following pre-treatment variables (averages): FDI, total assets, sales, sales squared, number of non-EU ETS countries in which the regulated firm operates, and number of affiliates in non-EU ETS countries. m denotes the number of matches used per treatment observation. All the specifications in this table include a linear bias adjustment and use the inverse of the variance-covariance matrix in the vector norm (Mahalanobis). The specification with a single match successfully reduces all normalized differences to below 0.4 at least (available upon request). Standard error estimates reported in brackets are constructed using the variance formula proposed by Abadie and Imbens (2006).
** $p < 0.05$.
* $p < 0.1$.

across treatment groups. Thus, we cannot preclude that the treatment effect estimates in the full sample are biased. The adopted two-stage research design appears as an essential prerequisite to produce sensibly balanced covariate distributions in the two treatment arms.

So far, our identification strategy has implicitly assumed independence across FDI flows of a given firm in different host countries. However, this assumption is rather strong given that one could argue that the investment of a focal firm in, say, Brazil is not independent of that firm's investment in Mexico. If the independence assumption is not met, our identification will lead to incorrect estimates of the standard errors. To surmount this challenge, we resort to Hanson and Sunderam (2012), who provide a methodology for consistently estimating the variance of the AI estimator in the presence of clustering. Table 4, however, shows that accounting for potential within-firm correlation produces standard error estimates (reported in squared brackets) that are similar to those from our baseline estimations. The causal effect estimate with a single match now is -0.2 ± 0.32. Therefore, clustering is not the driving force behind the results reported hitherto.

In another vein, our outcome variable of interest, the volume of outward FDI flows, embeds information of both the extensive and intensive margin of FDI. Following Mayer and Ottaviano (2008), the FDI volume (*FDI*) can be decomposed into the number of FDI projects ($N_{affiliates}$), the extensive margin, and the average size of FDI projects ($\frac{FDI}{N_{affiliates}}$), the intensive margin (see also Desbordes and Wei, 2017). An alternative dimension of the extensive margin is the number of investee countries outside the EU ($N_{countries}$) that reflects the firms' degree of internationalization. Table 5 uses the two outcome variables pertaining to the extensive margin of FDI. To obtain these outcomes, we aggregate firm-country observations to firm observations (see Table 5 for details of the adapted AI estimation procedure). We find that treated firms on average have increased the number of their affiliates outside the EU by 28.2% compared to a counterfactual scenario. Similarly, treated firms have increased the number of FDI destinations outside the EU by 19.3% more than their control counterparts. Yet, the latter effect is statistically significant only at the 10% level; the effect becomes insignificant at conventional levels when we increase the number of matches to 3. In short, EU ETS-regulated firms have expanded their network structure outside the

EU after 2005 relative to control firms. These policy-induced changes in the degree of internationalization may be suggestive of endeavors undertaken by regulated multinational firms to facilitate relocations in the future. Note that all estimates for the average size of FDI projects ($\frac{FDI}{N_{affiliates}}$) at the firm level turn out to be statistically insignificant and, for the sake of brevity, we do not present them. Similarly, when we aggregate outward FDI flows to the firm level (rather than working with firm-country observations as in our main specifications), treatment effects remain unambiguously insignificant (these two estimations are available upon request).

In Appendix D, we investigate how the data used in the main estimations measure up with respect to the underlying identifying assumptions. These supporting analyses focus on estimating "pseudo"-causal effects with a priori known values, and they indicate the tenability of the unconfoundedness and stable unit treatment value assumption.

4.2. Heterogeneous treatment effects

Treatment effects may vary systematically across (i) the different regulatory trading phases of the EU ETS and (ii) across subsets of regulated firms. In this section, we stratify the treatment group based on potential sources of heterogeneity to explore whether a particular group of regulated firms (in a particular regulatory period) shows significant policy-induced relocation patterns. The ensuing discussion focuses on the results based on the more balanced trimmed sample (see Table 6). Results relative to the full sample are reported for comparisons purposes in Appendix Table C4.

4.2.1. Trading phases [25]

None of the estimated treatment effects across different trading phases is statistically significant. Yet, we observe a positive and economically more meaningful treatment effect in the more stringent Phases II and III than in the early pilot phase. This evidence is consistent with the intuition gained from Fig. 4.

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[25] As our data only covers the first year of Phase III, we do not differentiate Phases II and III. More importantly, both trading periods are connected through permit banking.

Table 6
Heterogeneity of treatment effects.
Source: Source: Research Data and Service Centre of Deutsche Bundesbank, Microdatabase Direct investment (MiDi) 1999–2013, own calculations.

	Est.	Std. Err.	N_t	N_c
Regulatory EU ETS trading phases				
Phase I	−0.004	(0.189)	273	367
Phases II & III	0.056	(0.267)	285	333
Sector-specific characteristics				
Carbon leakage sectors	−0.049	(0.158)	371	302
Process regulated sectors	−0.157	(0.272)	153	163
Non-process regulated sectors	0.519**	(0.236)	160	255
Firm-specific characteristics				
High-emitting firms	−0.208	(0.249)	184	418
Firms with permit shortage	0.439**	(0.211)	70	418
Interactions				
Non-process regulated firms with permit shortage	1.215***	(0.346)	48	255
Non-process regulated firms w/o permit shortage	0.373	(0.275)	112	255

Note: Table 6 summarizes the results relative to tests of heterogeneity of the treatment effects across (i) the different regulatory trading phases of the EU ETS and (ii) across subsets of regulated firms. All the specifications shown match a treatment unit with a single control unit with replacement (i.e. $m = 1$). In all our matching, we require an exact match on sector and country. The vector of covariates in the AI estimations also contains the following pre-treatment variables (averages): FDI, total assets, sales, sales squared, number of non-EU ETS countries in which the regulated firm operates, and number of affiliates in non-EU ETS countries. All the specifications include a linear bias adjustment and use the inverse of the variance-covariance matrix in the vector norm (Mahalanobis). N_t (N_c) stands for the number of observations pertaining to the treated (control) firms. Reported standard errors are based on Abadie and Imbens (2006).
** $p < 0.05$.
*** $p < 0.01$.

4.2.2. Sector-specific characteristics [26]

Next, we look at policy effects for firms that operate in sectors on the carbon leakage list.[27] Although these firms are deemed to be at risk of carbon leakage, they do not exhibit different FDI patterns from other EU ETS regulated firms. The treatment effect for this subset of regulated firms levels out at −4.9 %, but it is statistically insignificant. Put differently, for the German data at hand, we find no empirical evidence that firms operating in sectors that look vulnerable according to EU criteria are indeed those firms that are more likely to relocate. We also estimate the treatment effect for firms that are found in those NACE2 sector classifications that mainly relate to the nine energy-intensive activities regulated through process-specific capacity thresholds — namely paper, non-metallic mineral products (including glass, ceramics, and cement), basic metals and chemicals (including pharmaceuticals).[28] We refer to these sectors as the "process regulated sectors". This estimate is contrasted with the estimated treatment effect for firms in the "non-process regulated sectors" that are regulated most likely due to combustion activities. The non-process regulated subsample is dominated by firms operating in the machinery (37%), electrical equipment (29%) and automotive (16%) sectors that are often also less emission-intensive. The comparison is motivated by the assertion that process regulated sectors tend to be more capital-intensive and presumably less footloose sectors, while the other sectors are often relatively less capital-intensive and, thus, more likely to be footloose. Our data corroborates such differences in the average capital intensity using the log of total assets per full-time employee as (noisy) telltale sign (1.8 for process regulated and 1.62 for non-process regulated sectors).[29] We observe a negative treatment effect of −15.7 % for the subsample

of process-regulated firms that engage in particular energy-intensive activities. In marked contrast, we find that non-process regulated firms have increased their FDI activity outside the EU by 51.9% as compared to FDI reported by a control group of observationally equivalent firms located in the same country and operating in the same sector. The effect is statistically significant at the 5% level ($t = 2.20$) and economically meaningful.[30]

4.2.3. Firm-specific characteristics [31]

To begin with, we investigate whether pollution-intensive firms respond to more stringent environmental regulations by investing more outside the EU than in a counterfactual scenario. The estimated differential treatment effect is negative but statistically insignificant; it is fairly similar to the treatment effect relative to process-regulated firms. Indeed, the majority of high-emitting firms (about 60%) engage in activities regulated through process-specific capacity thresholds. Rather than focusing on the emission-intensity, it might be more informative to look at the permit endowment of firms, i.e. their emissions relative to their holdings of free permits. Firms with permit shortage face higher abatement needs and direct compliance costs, which makes it more likely that their allocation is not profit-preserving. In this respect, we expect the treatment effect to be more pronounced for firms with short allocations. Indeed, the subset of treated firms with allowance shortage ($N_t = 70$) exhibits a positive and economically meaningful differential treatment effect of 43.9% that is statistically significant at the 5% level. A closer inspection of the sectoral composition reveals that the subsample is dominated by firms operating in non-process regulated sectors (69%), suggesting

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[26] We do not present results for each sector due to confidentiality restrictions.
[27] As the carbon list relies on 4-digit sector codes and the data provided by Deutsche Bundesbank is up to 3-digit sector codes, our conclusions should be viewed through the lens of this limitation.
[28] At the 2-digit level, they correspond to the codes 1700, 2300, 2400, 2000, and 2100 respectively.
[29] Based on aggregate sector data of the German Federal Statistical Office (provided in Löbbe, 2009; see Ederington et al., 2005 and Kellenberg, 2009 in an international context), the capital intensity (measured as gross stock of fixed assets per employee in thousand Euros) for machinery and electrical equipment amounts to 87 and 98, respectively. In contrast, in the process-regulated sectors metals, pulp and paper, and chemicals the capital intensity levels at 200, 224, and 271, respectively.

[30] We obtain similar findings when we split our sample into a low and high capital-intensity subsample using the log of total assets per employee as telltale sign.
[31] The subsamples of high emitters is obtained from using the sector-specific median of average verified emissions over the period from 2005 to 2013 as a telltale sign. In turn, to obtain the subsample of firms with allowance shortage, we first compute the net position in emissions permits as the difference between average allocated allowances and verified emissions over the period from 2008 to 2013; we then classify observations as short or long depending on whether the net position is negative or not (Phase I data is excluded, as only few firms have a permit shortage in this period; results including Phase I positions are qualitatively similar and available upon request). Emissions and allowances data come from the EUTL (seeSection3.1). We aggregate the installation-level data up to the firm level.

that the treatment effect may be driven by less capital-intensive and, thus, more footloose firms.

To test this conjecture and to better understand the role of permit shortage, we further investigate the interaction of permit endowment and sector affiliation. We first focus exclusively on the 48 non-process regulated firm observations with a permit shortage; the ensuing result should be interpreted in the light of the small sample size. We find an economically and statistically solid treatment effect (120.5%, $t = 3.51$), indicating that the under-allocation of non-process regulated firms may indeed be the driving force behind the treatment effect of 43.9% for the subsample of firms with short allocations.[32] Interestingly, 32 of the 48 firm observations relate to the machinery sector that is particularly characterized by low capital intensity. We then re-estimate the treatment effect for non-process regulated firms excluding the 48 companies with short allocations. The treatment effect decreases to 37.3% and becomes statistically insignificant. Thus, the substantial differential policy effect of 51.9% reported above for non-process regulated firms seems most likely to be driven by the subset of non-process regulated firms with a permit shortage. Finally, it is also noteworthy that in the (untrimmed) full sample only the treatment effect for non-process regulated firms with permit shortage turns out to be marginally statistically significant (70%, $t = 1.75$, Appendix Table C4).

In unreported analyses, we also seek to explore the consequences of the more stringent allocation rules implemented in 2013. If we focus only on the permit endowment position in 2013, almost 70% of our treated firms have a permit shortage given their actual emissions in 2013. It is notable that now more than 50% of the "short" sample relate to process-regulated firms. The treatment effect for this group of (allegedly more exposed) firms may give a first indication for the effects of a phase out of free allocation. Yet, the analysis certainly remains exploratory and it implicitly builds on the assumption that the short position in 2013 is anticipated by firms. We find a negative and statistically insignificant ($t = -0.55$) policy effect of -11.2 %. This finding is not consistent with the interpretation that the level of free permit allocation is a driving force of relocation.

Taken together, the heterogeneity analyses indicate that a small set of under-allocated firms that operate in non-process regulated sectors have shifted part of their production to non-EU ETS countries. Although the differential treatment effect for these firms is remarkable, we have to be cautious in making claims about aggregate effects. In fact, the firms account only for about 3% of total regulated German emissions, which can be explained by the sectoral composition. In terms of FDI, they are responsible for about 9% of the total sample FDI activity.[33]

4.3. Discussion

While we cannot find any statistically significant effect of the EU ETS on outward FDI flows for the universe of regulated multinational firms in Germany, a number of additional analyses still point to the presence of investment leakage. First, we find that all EU ETS firms on average have increased the number of their affiliates outside the EU by about 28% relative to control firms. This expansion of the subsidiary network outside the EU is a potential sign of leakage. Second, we show that a small but notable group of regulated firms also increased their FDI budgets outside the EU in response to the enforcement of the EU ETS. Paradoxically, relocating firms neither operate in the targeted energy-intensive sectors, nor are they emission-intensive; they are rather firms with a permit shortage in

one of the more footloose and less emission-intensive sectors that participate in the EU ETS just because they own combustion plants. Here, the machinery sector – that ranks 25 out of 28 in terms of capital intensity (Löbbe, 2009) – particularly stands out. This result corroborates the evidence by Ederington et al. (2005) and Kellenberg (2009) that firms in less capital-intensive industries are more easily influenced by changes in environmental policy. Because their fixed costs of relocation are small, these firms might pursue sources of comparative advantage even if the current costs of compliance remain fairly limited given low short positions and permit prices. The observation that footloose firms are not emission-intensive is particularly important from the perspective of environmental effectiveness as it indicates that the potential for policy-induced leakage of emissions may be reasonably limited.

Our findings for the German sample are consistent with evidence in a contemporaneous study by Borghesi et al. (2018), who show that, on average, the EU ETS has had a positive but small effect on the number and sales of non-EU subsidiaries owned by regulated multinational firms in Italy. They also complement evidence from large-scale survey data in Martin et al. (2014a, 2014b). The latter authors find that the average downsizing risk is low, and most firms in their European sample report that the EU ETS has no impact on their location decisions. Likewise, they complement the finding by Dechezleprêtre et al. (2015) that, based on emissions survey data for multinational firms, the EU ETS, has not induced intra-firm carbon leakage. Combining our findings and evidence from adjacent research, it stands reasonable to argue that the detrimental competitiveness effects of the EU ETS mainly manifest through a relocation of new production facilities outside the EU.

Our heterogeneity analyses also point to the potential relevance of relocation costs in explaining why firms may refrain from relocating. In fact, the nine targeted emission-intensive industrial activities of the EU ETS (most notably, the production of steel, cement, glass, pulp and paper) coincide with those sectors that are particularly capital-intensive; the associated large fixed plant costs, i.e. significant sunk investments into new plants, may dampen capital outflows and explain our statistically insignificant policy effect estimates. A second, non-mutually exclusive, explanation discussed in our conceptual framework is a positive net profit impact induced by the EU ETS. Direct evidence for the relevance of the profitability channel is provided in Bushnell et al. (2013); they find that high-emitting firms benefit from the EU ETS in terms of stock market valuations rather than being hurt (see also Oestreich and Tsiakas, 2015). Furthermore, firms may largely benefit from country-specific factors in the regulated home market (Antweiler et al., 2001; Chung, 2014). For instance, the proximity to the domestic market, advanced technology and sizable agglomeration economies in the Rhine-Ruhr area, where much of the industry is traditionally located in Germany, may impede the relocation of steel producers (Wagner and Timmins, 2009). Firms may also face higher entry costs in countries with weaker regulations (Eskeland and Harrison, 2003; Hanna, 2010). In unreported analyses, we find that regulated firms do not disproportionately increase their FDI in non-OECD countries.

From a policy perspective, our findings raise the more profound question about the effectiveness of free allocation as a compensation mechanism to mitigate the relocation problem. Indeed, meaningful treatment effects are found precisely for those firms with incomplete free permit allocation. At face value, the evidence attests to the relevance of free allocation in alleviating adverse policy effects. However, further decomposing the group of "under-allocated" firms (i.e. those with short allocations) provides reasonable indication that we have to be very cautious in attributing the relocation outcome exclusively to the EU ETS compensation scheme. This is because the estimated treatment effect seems to be driven by the subset of under-allocated firms that operate in relatively footloose industries. Thus, low relocation costs incurred by such industries are more likely to be the

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[32] Note that the sample size is too small to present reasonable estimates for the ATT of process regulated firms with short allocations.
[33] For comparison, if we focus on all (process and non-process regulated) firms with permit shortage, the share in total emissions and FDI amounts to 3% and 10%, respectively.

dominant force behind the relocation patterns. In addition, our preliminary analysis on the effects of reduced free allocation starting in 2013 does not indicate that the relocation propensity is necessarily increasing in permit shortage. Indeed, firms with under-allocation in 2013 show no significant policy response. Yet, if the level of compensation was a driving force of relocation, one would a priori expect these firms to be more prone to move production outside the EU. More targeted research is needed to properly disentangle the empirical relevance of the allocation scheme in shaping firms' location choices.

This study has presented estimates of the impact observed of the *de facto* EU ETS on relocation decisions. In this respect, we could not answer important questions concerning the potential policy impacts if the permit price had been higher or if permits had been auctioned. Moreover, we only estimate the direct effect of the EU ETS on firms, over and above any indirect effect induced through higher electricity prices, which is very challenging to separately identify. Finally, we emphasize that our focus here has been on FDI as a channel for carbon leakage. However, policy-induced changes in the trade balance and international fossil fuel prices are other potentially important carbon leakage channels.

5. Conclusion

This paper provides the first causal evidence on the credibility of the threat of relocation and investment leakage in the EU ETS. We exploit the installation-level inclusion criteria of the policy program to investigate its causal effect on outward FDI decisions of German multinational firms.

A matched difference-in-differences design enables us to control for confounding factors that affect both participating and non-participating firms as well as firm-level heterogeneity. We provide evidence that is compatible with leakage effects through FDI. Our baseline specification indicates that the sample average treatment effect levels out at −0.2 % and is statistically insignificant. Nevertheless, our study provides evidence of heterogeneity in the treatment effect by documenting that a comparatively small number of firms have shifted part of their production to non-EU ETS countries. These relocating firms neither operate in the targeted energy-intensive sectors, nor are they emission-intensive. On the contrary, they are firms with a permit shortage, operating combustion plants in sectors (in particular, machinery) that are supposed to be more geographically mobile due to low plant fixed costs. A particularly important finding from the perspective of environmental effectiveness is that these footloose firms only account for a small share of total regulated emissions. This evidence indicates that the policy-induced leakage of emissions may be reasonably limited. More generally, we have to be cautious in making general claims about the credibility of leakage risk in the entire EU ETS. This is because German multinational firms have a strong export orientation and considerable FDI activity worldwide and, as such, one would expect relocation to be particularly pronounced for these firms.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.eneco.2019.04.018.

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