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The impact of ETS on productivity in developing economies: A micro-econometric evaluation with Chinese firm-level data

Rushi Chen [a], Peter Howley [b], Effie Kesidou [c,*]

[a] Postdoctoral Fellow, Economics Department, Leeds University Business School, University of Leeds, Leeds LS2 9JT, United Kingdom
[b] Professor in Economics and Behavioural Science, Economics Department, Leeds University Business School, University of Leeds, Leeds LS2 9JT, United Kingdom
[c] Chair Economics of Innovation and Sustainability, Head of Graduate School, Economics Department, Leeds University Business School, University of Leeds, Leeds LS2 9JT, United Kingdom

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ABSTRACT

An emission trading system (ETS) is a market-based tool for reducing emissions. Pricing carbon dioxide (CO_2) emissions could impose additional costs on regulated firms, thus hindering their competitiveness. Prior literature has focused on ETS's economic impact in developed economies, especially the EU ETS. However, the impact of ETS on firms in developing economies is still unclear. Here, we examine the causal effect of CO_2 ETS on the labour productivity of firms in China. We use, for the first time in this research context, a recently released firm-level panel dataset coupled with a variety of estimation techniques, such as a time-varying difference-in-difference model and a plethora of non-parametric matching approaches. Across all our estimation strategies and using two distinct datasets, we find no evidence in support of the suggestion that ETS will diminish the competitiveness of regulated firms. Our results provide significant evidence in favour of the strong Porter hypothesis, namely that ETS will boost the productivity of participating firms in China. Furthermore, we demonstrate that the beneficial impact of ETS is concentrated on relatively smaller and younger firms. In policy terms, these results illustrate that market-based environmental tools can reduce pollution while simultaneously boosting the competitiveness of small and new firms in a developing economy.

1. Introduction

The emission trading system (ETS) has become increasingly popular as a market-based tool for reducing emissions (Fell, 2016). In general, an ETS seeks to limit the maximum amount of carbon dioxide (CO_2) emissions that participating firms are allowed to emit. The key advantage of an ETS relative to command-and-control approaches is that it provides firms with a certain amount of flexibility to realise emission reduction targets by trading their permits in the carbon markets when it is cost-effective to do so. To date, ETSs have been implemented in 37 developed economies, including Canada, New Zealand, Norway, Iceland, Switzerland, Liechtenstein, the UK, the USA, Korea, and Japan, as well as the 27 EU Member States. In addition, developing economies such as China, Kazakhstan, Montenegro, and Mexico have begun to implement ETS as a means of achieving their international commitments when it comes to the reduction of global greenhouse gases (GHG). The net result is that as of 2022, about 17% of the world's GHG emissions were traded in emission trading markets (ICAP, 2022).

Despite the advantages associated with the ETS, one factor limiting the further development of ETSs, particularly in developing economies, is the fear that such schemes will put regulated firms at a competitive disadvantage. The concern is that pricing carbon dioxide emissions imposes substantial extra costs and financial constraints on regulated firms. This, in turn, hinders their competitiveness vis-à-vis unregulated firms, ultimately adversely impacting their economic development (Dechezleprêtre and Sato, 2017). A countervailing argument has been put forward by Porter (1991) and Porter and Van Der Linde (1995), who argue that well-designed environmental regulations will incentivize firms to promote cost-cutting efficiency improvements, which will in turn reduce or completely offset regulatory costs, potentially enhancing the productivity of regulated firms.

While the debate between the traditional view and the Porter hypothesis has sparked a good deal of research on the impact of ETS, it has principally concentrated on the ETS implemented in developed economies, especially the EU ETS (see Dechezleprêtre and Sato, 2017 for a recent review). The impact of ETS on firms in developing economies is

* Corresponding author.
E-mail address: E.Kesidou@leeds.ac.uk (E. Kesidou).

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still unclear. Strict environmental regulations in developing economies may encourage firms to import green technologies from advanced economies rather than investing in domestic pollution control technologies, thereby increasing costs for local firms (Lanjouw and Mody, 1996). Thus, the findings observed in developed economies relating to the effectiveness of an ETS may not be applicable in a developing economy context.

We contribute to this literature by conducting the first comprehensive micro-econometric evaluation of the impact of CO_2 ETS pilots on firm-level labour productivity in China. China serves as an appropriate setting for this research because it is the world's largest contributor to CO_2 emissions. This trend has steadily risen over the years, reaching 10 billion tons of CO_2 in 2017.[1] Implementing a "least cost" environmental regulation is essential for balancing what at times can be seen as a conflict between economic development and environmental protection in China.

This study makes several contributions to the literature: First, we extend the concept of the strong Porter hypothesis to the context of a developing economy. The extant literature has examined the productivity effects of ETS in developed economies such as the EU and the US (Aus Dem Moore et al., 2019; Löschel et al., 2019; Marin et al., 2018). By contrast, relatively little is known about whether ETS will boost the productivity of firms in developing economies.

Second, we contribute to empirical research in this field by utilizing, for the first time in this context, a representative and precise firm-level dataset from a newly released Chinese Industrial Firm Database. This database covers over 90% of regulated firms, and to the best of our knowledge, it is the first time it has been used to study the implementation of ETS in China. In previous studies, the effects of ETS in China have been examined using aggregated data (e.g., Zhang et al., 2021; Li et al., 2022) or data from listed firms (e.g., Ren et al., 2022; Xiao et al., 2023). However, the use of either type of data has significant limitations: (i) Given that China's CO_2 ETS scheme targets firms, utilizing aggregated data may introduce aggregation bias. This bias arises from the difficulty in distinguishing between unregulated and regulated firms when using aggregated data, making it difficult to isolate the impact of environmental regulation from other confounding factors (Dechezleprêtre and Sato, 2017; Levinson and Taylor, 2008). (ii) Listed firms may not be representative of the wider population of Chinese firms as they are typically older, larger, more innovative, and skill- and capital-intensive. Relying on listed firm data also results in a small treatment group, which precludes accurate heterogeneity analysis.[2]

Third, we contribute to the environmental economics literature by demonstrating that ETS leads to heterogenous responses among firms. Specifically, we argue that not all firms facing similar environmental regulatory pressures will respond in the same way. Prior literature remains inconclusive regarding the heterogeneous impact of environmental regulations according to size, age, or ownership. Particularly, limited attention has been paid to how firms' age is related to environmental regulation, with notable exceptions (Curtis and Lee, 2018; Yamazaki, 2022). Here, we explore how the impact of ETS upon firms' labour productivity varies across different firm characteristics, including firm size, age, and ownership structure.

Fourth, we make a contribution to empirical research in this field by adopting a novel approach to identify unregulated firms with historically large CO_2 emissions (i.e., over 10,000 tons) to serve as our control group. This is crucial because China's CO_2 ETS pilots exclusively regulate firms with historical CO_2 emissions exceeding 10,000 tons. Previous research was unable to do this due to the unavailability of firm-level CO_2 emission data in China.

Lastly, we introduce a new methodological approach in this research by examining the impact of China's CO_2 ETS on labour productivity. We employ a time-varying difference-in-difference (DID) estimator, which accounts for the variation in the timing of ETS introduction across pilot regions. Previous research has commonly used the conventional DID approach, with notable exceptions (Li et al., 2022; Wu and Wang, 2022; and Xiao et al., 2023). We also employ an array of parametric conditioning strategies (e.g., firm and year fixed effects) in our DID model, which helps us isolate the impact of the ETS. Finally, we supplement our DID technique with a propensity score matching (PSM) approach to further minimise any possible bias led by the non-random selection of regulated firms. As a sensitivity check, we also estimate the impact of the ETS on total factor productivity (TFP) using a smaller and more commonly used sample of Chinese firms listed on the stock market.

Our results, derived from analysis employing a variety of estimation strategies and utilizing two district datasets, consistently reveal no evidence supporting the notion that the implementation of ETS will erode the competitiveness of regulated firms. These findings provide significant evidence in favour of the strong Porter hypothesis, namely that ETS will boost the productivity of participating firms in China. Furthermore, we demonstrate that the beneficial impact of ETS is concentrated on relatively smaller and younger firms. In policy terms, these results illustrate that market-based environmental tools can reduce pollution while simultaneously boosting the competitiveness of small and new firms in a developing economy.

In the following section, we review the existing research on the economic consequences of ETS intervention in developed and developing countries. It also summarises the key features of China's seven CO_2 ETS pilots. Section 3 introduces the sampling strategies, econometric models, and data employed by this research. Empirical results, heterogeneous analysis, and several robustness checks are outlined in Sections 4 and 5. Finally, in Section 6, we conclude with a discussion of our main findings and associated policy implications.

2. Background

2.1. The Porter hypothesis and empirical evidence

The introduction of an ETS, similar to other environmental regulations such as command and control approaches, is expected to impose new costs on firms (Albrizio et al., 2017). These costs may include purchasing pollution permits and complying with requirements for monitoring, reporting, and verifying emissions. The conventional economic view is that these additional costs will put regulated firms at a competitive disadvantage compared to unregulated ones. This can result in the relocation of economic activity from regulated regions to low-abatement-cost regions, causing policy-induced pollution leakage (Baksi and Bose, 2016; Eskeland and Harrison, 2003).

An alternative view, proposed by Porter (1991), is that more stringent environmental regulations promote efficiency improvements, which in turn reduce or completely offset the extra regulation costs. Porter and van der Linde (Porter and Van Der Linde, 1995, p. 98) go further by proposing that environmental regulations may actually "trigger innovation that may more than fully offset the costs of complying with them". Under this scenario, environmental regulations will improve the economic performance of the regulated firms relative to what it would otherwise have been.

With the increasing prevalence of ETS, numerous studies have examined the economic outcomes of its implementation in developed countries, such as the EU ETS. Much of the existing research is based on simulation studies, which generally suggest that the EU ETS may weaken economic performance (e.g., Demailly and Quirion, 2006; Ponssard and Walker, 2008). However, more recently, there have been ex-post evaluations of the economic impact of the EU ETS on various measures of firm outputs. The advantage of ex-post evaluations relative to simulation studies is the identification of a control group consisting of similar firms

[1] The source is CAIT data: Climate Watch. 2020. GHG Emissions. Washington, DC: World Resources Institute. Available at: https://www.climatewatchdata.org/ghg-emissions

[2] Only around 10% of the regulated firms in ETS are listed firms.

unaffected by environmental regulation. While there are exceptions (Linn, 2010; Yu, 2013), these studies have generally indicated that the EU ETS does not lead to losses in competitiveness (Anger and Oberndorfer, 2008; Chan et al., 2013). Some studies even support the strong version of the Porter hypothesis, namely that the EU ETS will enhance economic performance in areas such as investment intensity, employment, fixed assets, markup, labour productivity, efficiency, and TFP (Aus Dem Moore et al., 2019; Löschel et al., 2019; Marin et al., 2018).

Evidence of the economic impact of ETS in developing economies like China is still, however, scarce. Relatively early studies in this area have principally relied on computable general equilibrium (CGE) models, and the findings have been mixed. For example, Yang et al. (2016) and Wang et al. (2015) find that ETS can lead to an increase in China's GDP, but Lin and Jia (2018, 2019) and Liu et al. (2017) suggest an insignificant and even negative influence. A potential limitation with the use of ex-ante simulation approaches like CGE models is that the results are sensitive to different modelling assumptions, data selection, and simulation scenarios (Wang et al., 2015), and this could explain the mixed findings.

Ex-post evaluations of the economic impact of China's CO_2 ETS have been conducted, but they have had to rely on aggregated data (at the province or city level) due to the lack of high-quality firm-level data. Using this approach, Zhang and Zhang (2020) and Zhang and Duan (2020) have reported a decrease in GDP, gross industrial output value, and employment resulting from China's CO_2 ETS. Wang et al. (2022) further indicate that the pilot markets of China's CO2 ETS are overall inefficient, which may negatively contribute to economic performance. In contrast, Wang et al. (2019), Yang et al. (2020), Zhou et al. (2020), and Zhang et al. (2021) have examined both the environmental and economic consequences of China's CO_2 ETS based on provincial data and found that it was associated with an increase in provincial carbon productivity and green TFP.

A few recent studies have used firm-level data to assess the impact of China's ETS, but these have focused on listed firms in the stock markets, and the findings are mixed. For example, using a PSM-DID model, Zhang and Liu (2019) and Wen et al. (2020) find that China's CO_2 ETS led to a growth in listed firms' Tobin's Q and stock returns, respectively. Ren et al. (2022) demonstrated a positive impact of China's CO_2 ETS on the TFP of listed firms. In Xiao et al. (2023), China's CO_2 ETS is positively associated with listed firms' labour income share. In contrast, Zhang and Wang (2021) find that the CO_2 ETS led to a reduction in investment expenditures. Dai et al. (2018) did not find any significant impact of the CO_2 ETS in China on the TFP of listed firms.

2.2. Emission trading schemes in China

China has become the largest global energy consumer and CO_2 emitter, resulting in significant international criticism and pressure to reduce emissions. In response, the Chinese government has shown considerable interest in adopting ETS as a means to address the need to decrease both GHG emissions and potential compliance costs.

The Chinese government's earliest official use of ETS began in 1998 with the implementation of a sulfur dioxide (SO_2) ETS, principally at the provincial or municipal levels (Liu et al., 2022; Feng et al., 2020). However, the effectiveness of the SO_2 ETS was limited due to institutional deficiencies (Schröder, 2011; Shin, 2013). Additionally, excessive government intervention in regulating transaction prices and volumes of carbon emissions in the SO_2 emission trading markets resulted in distortions (Lo, 2013).

In 2011, the Chinese government introduced a series of CO_2 emission trading pilot schemes, forming the basis for this study. These pilots in China have introduced substantial improvements in their designs compared to the SO_2 ETS. For example, they employed a variety of well-designed allocation approaches. Most allowances in China were allocated for free by grandfathering, wherein firms simply received emission allowances based on their historical cumulative emissions. Additionally, benchmarking was applied in some sectors, such as electric power, gas, and water. Under this method, allowances were allocated according to historical activity levels and sector-specific benchmarks (Zhang et al., 2015; Chang et al., 2017). To stabilise carbon prices, increase market supply, and meet compliance demand, a proportion of allowances were reserved for auction, new entrants, and sales. Other means, such as project-based carbon offset credits, namely Chinese Certified Emission Reduction (CCERs), with qualitative and quantitative limits, were also allowed to increase the compliance rates (ICAP, 2014). These improvements have made the CO_2 ETS pilots in China more likely to effectively promote the economic performance of firms.

Table 1 summarises key features of the CO_2 ETS pilot from 2013 to 2015, which broadly relate to our treatment period. The selected CO_2 ETS pilots were in Beijing, Shanghai, Guangzhou, Shenzhen, Tianjin, Hubei, and Chongqing.[3] These regions are more developed and urbanized, accounting for approximately 27% of the national GDP and 19% of the population of the Chinese mainland (National Bureau of Statistics of China, 2012).

These ETS pilots primarily targeted firms in carbon-intensive industries, such as mining, manufacturing, and industries producing and supplying electric power, gas, and water (ICAP, 2014). Approximately, 1896 industrial firms were selected to participate in these pilots from 2013 to 2015 based on their historical emission levels.[4] In the Shenzhen pilot, firms with annual CO_2 emissions of >3000 tons were regulated by ETS, while in the other six pilots, firms were regulated if their CO_2 emissions exceeded at least 10,000 tons in any year from 2009 to 2012.

The Hubei and Chongqing pilots began in 2014, while the other five

Table 1
Key features of China's CO_2 ETS pilots in 2013–2015.

Pilots	Periods	Number of covered entities[a]	ETS threshold (tons CO_2 /year)
Beijing	2013 2014 2015	490 543 981	> 10,000 in any year before 2013
Guangdong	2013 2014 2015	202 193 186	> 20,000 in any year before 2013
Tianjin	2013–2014 2015	114 109	> 20,000 in any year of 2009–2012
Shanghai	2013–2015	197	> 20,000 for industry in 2010 or 2011
Shenzhen	2013–2015	635	> 3000 for industry in any year before 2013
Chongqing	2014–2015	242	> 20,000 in any year of 2008–2012
Hubei	2014 2015	138 167	> 120,000 in either 2010 or 2011

Source: Chang et al. (2017), Websites of regional governments and Development and Reform Commissions.

[a] Referring to ICAP (2014), "entities" is used as the collective name here for all firms, public institutions, and governmental departments covered by China's CO_2 ETS pilots.

[3] China's eighth CO_2 ETS pilot, in Fujian Province, was not announced to be implemented along with the other pilots in 2011 but launched at the end of 2016. Because our dataset is limited to 2015, the pilot in Fujian is not included in our analysis.

[4] The total number of treated industrial firms is based on the authors' count. During the treatment period from 2013 to 2014, the Beijing pilot included not only industrial firms but also many non-industrial firms, public institutions, and governmental departments. However, the Beijing pilot did not disclose any industry information to help us distinguish which of them belonged to the industrial sectors. Therefore, for the firms treated in the Beijing pilot, we searched for these firms on https://top.tianyancha.com/ and obtained their industry information.

pilots started in 2013. Some firms in the Beijing and Hubei pilots joined the ETS pilots in 2015. Therefore, a time-varying DID approach (which we will discuss in more detail later) is necessary to account for this variation in treatment timing.

3. Data and methodology

3.1. Data source

The main database we use in this study is the Chinese Industrial Firm Database.[5] This database is maintained by the National Bureau of Statistics of China and collects data from all industrial firms above a designated scale, which pertains to firms with an annual sales turnover of twenty million RMB or above since 2011. This dataset includes over 500,000 firms, with annual data collected from 2011 to 2015, resulting in 1,586,708 observations. This database has been widely used in examinations of productivity growth, international trade, regulation performance, and patent applications (e.g., Brandt et al., 2012; Fang et al., 2020; Kee and Tang, 2016; Fan et al., 2021). To date, it has not (to the best of our knowledge) been used to evaluate the effectiveness of an ETS aimed at reducing CO_2.

Notwithstanding the strengths of this dataset, there have been reported issues regarding missing values and misreported data (Wang et al., 2021). To mitigate against any problems caused by these issues, we dropped observations that violated a set of strict conditions (see Appendix A), following the approach used by Fang et al. (2020) and Wang et al. (2021). After implementing these changes, we have an unbalanced panel dataset from 2011 to 2015, comprising 277,792 firms (1,065,066 observations). We restrict the study period to 2011–2015 because data for our key variables of interest is unavailable in 2010 and post-2015. Following Brandt et al. (2012), we construct a consistent industrial classification code and deflate the original data so that the variables are comparable over time.[6]

3.2. Variables

3.2.1. Outcome variable

Our key outcome variable is labour productivity (*lnLP*), measured as gross output per employee. This variable is arguably the most relevant indicator for assessing the economic impact of environmental regulation. In the short run, firms may absorb the extra compliance costs by diverting production inputs and resources towards pollution control activities, potentially harming labour productivity. In the medium to long run, environmental regulations may stimulate firms to engage in cost-saving, efficiency-improving, or environmentally friendly innovations. This, in turn, can reduce emissions intensity, save abatement costs, and enhance productivity. Thus, in the face of ETS, firms that become environmentally efficient may also improve labour productivity (Dechezleprêtre and Sato, 2017; Mazzanti and Zoboli, 2009; Murty and Kumar, 2003; Rubashkina et al., 2015).

3.2.2. Control variables

We consider a vector of commonly used firm characteristics in the empirical literature, including firm age (*AGE*, defined as the difference between the present year and the firm's birth year), firm size (*SIZE*, measured as the logarithm of total assets), debt ratio (*DEBT*, calculated as the ratio of total liabilities to total assets), and ownership type (*OWN*, equal one for state-owned firms and zero otherwise).

Prior studies have shown that younger firms are more likely to generate radical innovations, potentially leading to faster productivity growth (Coad et al., 2016; Barba Navaretti et al., 2014; Fu, 2012; Pellegrino and Savona, 2017). Therefore, we expect a negative relationship between firm age and labour productivity. We include firm size because larger firms typically benefit from economies of scale (Amin and Islam, 2015; Tybout, 2000), which tend to result in higher labour productivity on average (Zheng et al., 2021; Li and Su, 2022; Guo et al., 2023). The debt ratio may influence a firm's operating behaviour and economic efficiency, perhaps leading to a negative correlation with labour productivity (Sharpe, 1994; Lu and Zhang, 2022; Chen and Guariglia, 2013). Lastly, ownership type could be important because non-stated-owned firms in China often exhibit higher management flexibility and efficiency because of less government intervention in comparison to stated-owned firms (Wang et al., 2017; Chen et al., 2019). We include all these variables as covariates because, in addition to being significantly related to labour productivity, they could also be related to the probability of treatment, i.e., being regulated by the CO_2 ETS.

3.3. The treatment and control groups

One notable feature of the ETS implemented in China is that it primarily regulated industrial firms with historical CO_2 emissions above 10,000 tons in any year between 2009 and 2012. To account for this non-random selection of firms, it is important to identify firms with similar emission levels to serve as our control group. This is because firms with high emissions may also differ in production efficiency and abatement costs, factors correlated with their response to the ETS. We note that existing studies have generally overlooked this problem, likely due to the unavailability of firm-level CO_2 emission data in Chinese datasets.

We adopted a novel empirical design to mitigate this problem. By the end of 2011, industrial firms that consumed a minimum of 10,000 tons of CO_2 in 2010 (approximately 5000 tce of energy) were required to join the national or provincial 'Top 10,000 Firms of Energy Conservation Program'.[7] This program sought to encourage high-emission firms to reduce their energy consumption. Using this publicly available list of firms with high emissions ($>$ 10,000 tons), we could identify firms closely matching our treatment group in terms of prior emissions but not subject to ETS regulation. In Fig. 1, we provide a visual illustration of our sample construction process and our methodological approach, which we describe in more detail below.

Our starting point was the identification of treated industrial firms in our Chinese Industrial Firm Database using firm names and IDs published by the Development and Reform Commission of each pilot region. This process resulted in 1320 matches (5389 observations), i.e., treated firms that were listed in the Chinese Industrial Firm Database. We note here again that all treated firms had historical CO_2 emissions above 10,000 tons in any year between 2009 and 2012. These firms comprised about 70% of all regulated industrial firms during the ETS period of

[5] In some studies, it is referred to as 'the Annual Surveys of Industrial Enterprises in China'.

[6] As the Chinese industrial classification code was changed in 2011, we harmonised firms' industrial classification codes before and after 2011, thus ensuring the industrial classification code of each firm is consistent over time. Also, referring to Brandt et al. (2012), gross outputs, total liabilities, and total assets that were used to calculate our key variables are deflated using the "Ex-factory Price Index" from the China Statistical Yearbooks, and thus our variables are comparable over time.

[7] Our list of the Top 10,000 industrial firms is consistent with two lists of the 'Top 10,000 Firms of Energy Conservation Program'. The first one is a national list covering all industrial firms with CO_2 emissions in excess of 20,000 tons in China. The second is a provincial list covering industrial firms with CO_2 emissions in excess of 10,000 tons in Guangdong province. We are able to reveal counterfactuals with similar emission levels to most of our treatment firms by using these two lists.

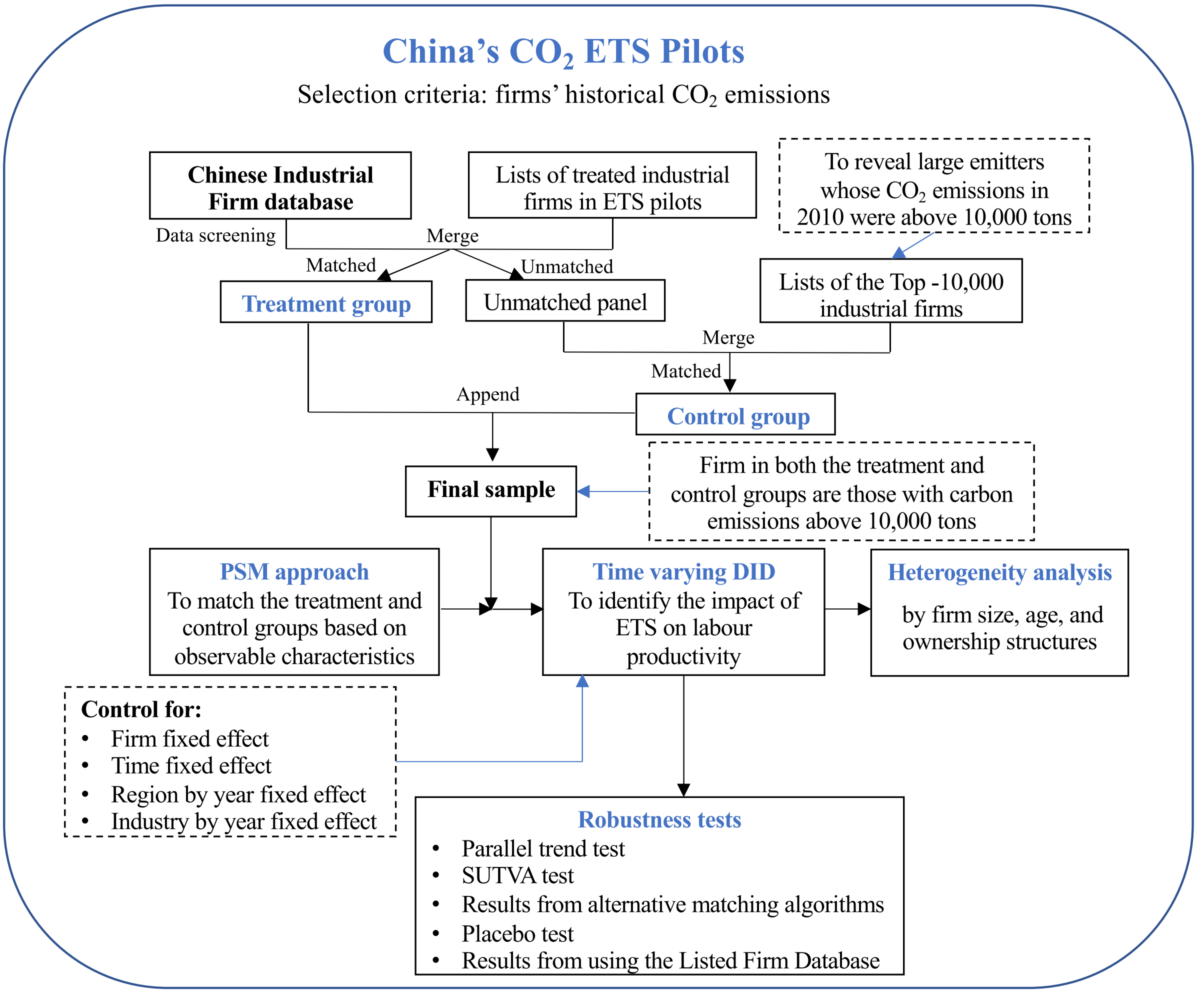

Fig. 1. Research framework.

2013 to 2015, forming our treatment group.[8]

Our next step is to identify firms that closely match our treatment group in terms of prior carbon emissions, utilizing the 'Top 10,000 Firms of Energy Conservation Program' lists. Each firm on these lists had a unique name and ID, facilitating matching with the Chinese Industrial Firm Database. This matching process allowed us to derive a control group of firms similar to our treatment group, not only in terms of firm characteristics such as size and ownership structure but also crucially in terms of prior emissions.

Overall, our final sample (both treatment and control groups) contained 13,098 firms (52,796 observations).[9] The descriptive statistics of the full sample as well as the treatment and control groups separately are reported in Table 2. This table illustrates that both groups are broadly similar in terms of size, debt ratio, labour productivity, and age. There are some differences when it comes to ownership structure, as a higher proportion of firms in the treatment group are state-owned. Later, we will examine the impact of ETS on state-owned firms.

3.4. Empirical model

3.4.1. The time-varying DID approach

We evaluate the productivity impact of the ETS implemented in China using a DID estimation approach. The central idea behind our DID analysis is to disentangle the average effect of China's CO_2 ETS pilots from any confounding by comparing the difference in average labour productivity between regulated firms and our control group before and after the introduction of the ETS. We employ a time-varying DID approach to account for variations in firms' treatment timing. For example, firms in Hubei and Chongqing were regulated in 2014 (or 2015

[8] We do not have the other 30% of the total number of regulated industrial firms in the ETS period of 2013 to 2015 for the following reasons: (1) About 2% of them are not contained in the Chinese Industrial Firm Database. (2) 12% of them were dropped because their observations violated a set of strict conditions listed in Appendix A. Specifically, 11.2% of them were dropped because their observations contain missing values and do not have data for both before and after the implementation of ETS. 0.08% of them are dropped because of the misreported or misclassified issues with their data. (3) 16% of them were deleted from our sample because they are regulated industrial firms in the Shenzhen pilot that emitted CO_2 between 3000 and 10,000 tons. It is impossible to find suitable matches (counterfactuals) in terms of prior emissions for these relatively small emitters when firm-level CO_2 data is unavailable in China.

[9] To prevent any bias caused by extreme outliers, we winsorize and replace observations of all continuous variables at the 0.5th and 99.5th percentiles of their distributions, as suggested by Wang et al. (2018). However, we end up with a very similar estimated treatment effect without winsorizing the data (see Table C.1 in Appendix C).

Table 2
Descriptive statistics for samples.

	Full sample (N_F = 13,098)					
	#Obs.	Mean	Min	Max	S. D.	
lnLP	52,796	6.80	3.87	11.63	1.31	
SIZE	52,796	13.20	9.29	17.99	1.68	
AGE	52,796	11.98	2	39	6.14	
DEBT	52,796	0.61	0.02	1.79	0.30	
OWN	52,796	0.23	0	1	0.42	
	Treatment group (N_T = 1320)			Control group (N_C = 11,778)		
	#Obs.	Mean	S. D.	#Obs.	Mean	S. D.
lnLP	5389	6.84	1.37	47,407	6.80	1.31
SIZE	5389	13.50	1.61	47,407	13.16	1.68
AGE	5389	13.41	6.81	47,407	11.82	6.04
DEBT	5389	0.57	0.28	47,407	0.61	0.30
OWN	5389	0.34	0.47	47,407	0.22	0.42

Notes: N_F, N_T, and N_C indicate the number of firms in the full sample, treatment group, and control group, respectively.

for some firms), whereas regulation started in 2013 in the other pilots. Since firms were not all exposed to the regulations at the same time, the standard DID model is not suitable in this context (Callaway and Sant'anna, 2021; Wang et al., 2021). Formally, our time-varying DID specification takes the following form:

$$y_{i,t} = a + \beta_1 ETS_{i,t} + \beta_2 Control_{i,t} + F_i + T_t + R_{r,t} + I_{s,t} + \varepsilon_{i,t} \tag{1}$$

Where $y_{i,t}$ is the labour productivity (*lnLP*) of firm *i* in year *t*, defined as the logarithm of gross output per employees. $ETS_{i,t}$ is an interaction term between dummy variables capturing firms' treatment status and time periods. Specifically, $ETS_{i,t}$ equals one in year *t* if the ETS pilots treat a firm *i* from year *t* (i.e., post-treatment periods equal one) and equals zero before year *t* (i.e., pre-treatment periods equal zero). For firms that have never been treated, $ETS_{i,t}$ remains zero. Unlike the standard DID model, wherein the values of $ETS_{i,t}$ in each year are the same for all treated firms, in a time-varying DID model, the values of $ETS_{i,t}$ can vary across treated firms in the years before and after year *t*, indicating the different years in which they were 'treated'. β_1 is the coefficient of interest, indicating the economic consequences of ETS implementation.

$Control_{i,t}$ refers to our firm-level control variables, including firm age (*AGE*), size (*SIZE*), debt ratio (*DEBT*), and ownership (*OWN*), as defined in Section 3.2.2. F_i, denotes firm fixed effects, controlling for any firm-level time-invariant heterogeneity. T_t represents year fixed effects, controlling for any period-specific shocks. $R_{r,t}$ refers to the region by year fixed impacts, controlling for any time-varying regional shocks.[10] $I_{s,t}$ indicates the industry by year fixed impacts, helping to eliminate time-varying industry-level shocks (Fernandes and Paunov, 2012).[11]

3.4.2. PSM-DID method

To strengthen the robustness of our findings, we supplement the DID approach described above with a propensity score matching approach. This helps to remove potential bias resulting from the non-randomized assignment of treatment groups (Heckman et al., 1998). The propensity score is the probability of being regulated by ETS conditional on observed baseline characteristics (Rosenbaum and Rubin, 1983). We estimate a propensity score for each firm using a logit model, where a dummy variable indicates whether firms were regulated by the ETS.

The intuition behind this approach is to ensure that, once matched by their propensity score, treatment and control group firms should have similar pre-treatment characteristics and, thus, be impacted by observable confounding factors in a similar way (Imbruno and Ketterer, 2018). This involves reweighting or discarding firms with poor matches so that both treatment and control groups have similar distributions of characteristics, much like what would be observed under random assignment (Hainmueller, 2012). The matching variables we selected in deriving our propensity score include age, size, debt ratio, and ownership structure in the pre-treatment period (i.e., 2011–2012), since these pre-treatment characteristics are likely to either determine the treatment assignment or the reaction of firms to the ETS.

There are a plethora of techniques available when it comes to matching sets of treated and untreated firms. Here, we adopt a one-to-three nearest neighbour (NN) matching approach with a calliper of 0.01. This approach pairs each treated firm with three untreated firms that have the closest similarities in propensity score and lie within the specified calliper (i.e., the maximum tolerable difference in propensity score). Selecting more than one NN from the control group increases precision in our estimates (i.e., less variance), as greater information is used. We only use NNs within the calliper to prevent significant differences in propensity scores between matched firms, decreasing the risk of bias from bad matches (Aus Dem Moore et al., 2019; Caliendo and Kopeinig, 2008).

We conducted various sensitivity checks, including tests proposed by Rosenbaum and Rubin (1985) and Caliendo and Kopeinig (2008), to assess the matching quality (Table B.1 of Appendix B). These include t-statistics, the standardised bias, and the percentage bias reduction before and after matching.[12] We also tested the sensitivity of our results to alternative matching techniques, namely the radius matching method and the kernel matching method.

4. Estimation results and analysis

4.1. The overall impact of ETS implementation on labour productivity

Our results relating to the estimated impact of the ETS on regulated firms in China are reported in Table 3. In Columns 1 and 2, we report results from a parsimonious model, initially including only firm and year fixed effects (Column 1) and then adding industry by year and region by year fixed effects (Column 2). In Columns 3–6, we add controls for firm size, age, debt ratio, and ownership structure. Across all specifications, our ETS variable, reflecting the DID estimate of the ETS's impact on the labour productivity of regulated firms, exhibits a positive and statistically significant coefficient. This provides initial evidence in favour of the strong version of the Porter hypothesis.

In addition to being statistically significant, there is a notable degree of consistency across all specifications in terms of effect size. In Column 1 in Table 3, the ETS coefficient is 0.0493, suggesting that the ETS enhanced labour productivity by an estimated 4.9%. This figure does not materially change across all the remaining specifications.

In Columns (7), we report the regression results from our propensity score matching approach, which we label as PSM-DID. The coefficient estimate (0.0483) in this approach is similar to those in our baseline DID model. All in all, across a variety of specifications and using both DID and PSM-DID, we find no evidence to suggest that the ETS harms the labour productivity of regulated firms. Indeed, our analysis would suggest that the ETS is associated with a modest improvement in the labour productivity of regulated firms. The results relating to our control variables are all along expected lines, as discussed in Section 3.2.2, but we leave them unreported for parsimony.

[10] The regional dummy variable equals one if a firm comes from the eastern economic regions (i.e., developed regions in China) and zero otherwise.

[11] The industrial category variable equals one if a firm belongs to the mining industry, equals two if a firm belongs to the manufacturing industry, and equals three if a firm belongs to the industries producing and supplying electric power, gas, and water.

[12] The standardised bias (%) in relevant covariate means between the treatment group and control groups equals the mean difference between the two groups divided by the standard deviation, which is measured by: $Bias_{after}(X) \equiv 100(\overline{X}_{tM} - \overline{X}_{cM})/\sqrt{(V_1(X) + V_0(X))/2}$ (Rosenbaum and Rubin, 1985).

Table 3
The DID regression results.

	DID						PSM-DID
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$ETS_{i,t}$	0.0493***	0.0548***	0.0470***	0.0481***	0.0478***	0.0480***	0.0483***
	(0.0169)	(0.0171)	(0.0164)	(0.0163)	(0.0163)	(0.0163)	(0.0163)
SIZE			0.3801***	0.3802***	0.3784***	0.3784***	0.3790***
			(0.0183)	(0.0183)	(0.0183)	(0.0183)	(0.0184)
AGE				−0.0679***	−0.0699***	−0.0697***	−0.0619**
				(0.0237)	(0.0238)	(0.0238)	(0.0254)
DEBT					−0.1221***	−0.1218***	−0.1223***
					(0.0301)	(0.0301)	(0.0301)
OWN						−0.0485	−0.0486
						(0.0451)	(0.0451)
Constant	6.8686***	6.5651***	1.7725***	2.4764***	2.5883***	2.5990***	2.5110***
	(0.0037)	(0.1481)	(0.2660)	(0.3668)	(0.3712)	(0.3712)	(0.3834)
No. of Obs.	52,796	52,796	52,796	52,796	52,796	52,796	52,754
Firm FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Industry by year FE	NO	YES	YES	YES	YES	YES	YES
Region by year FE	NO	YES	YES	YES	YES	YES	YES

Note: Standard errors clustered at the firm level are presented in parentheses. *** stands for the 1% significance level. FE indicates the fixed effects.

4.2. Heterogeneity analysis

Our estimates in Table 3 suggest that the implementation of CO_2 ETS pilots led to a modest positive impact on labour productivity for regulated firms. Next, we explore the possible mechanisms by conducting heterogeneity analysis based on different sub-groups related to firm size, age, and ownership structure.[13]

4.2.1. Heterogeneity analysis by firm size

To investigate the heterogeneous effect of ETS on labour productivity across firm size, we divided our sample (both our treatment and control groups) into two sub-samples, "Large" and "Small" firms, using the median value of the log of total assets (Cerqua and Pellegrini, 2014; Barrows and Ollivier, 2021).[14] We hypothesise that firms may be impacted differently by ETS according to their size due to differences in production technologies, resource endowment, compliance costs, and management efficiency (Wang et al., 2018). The regression results of the PSM-DID models are reported in Columns (1) and (2) of Table 4. They suggest that ETS has a stronger impact on smaller firms. Smaller firms experienced an estimated treatment effect of 0.0707 (7.07%), while larger firms had an effect of 0.0252 (2.52%). In addition to being substantial, we also note that this difference is statistically significant (Z = 1.30, *p-value* = 0.097).[15]

Overall, our results indicate that the beneficial effects of China's ETS on productivity are more pronounced among smaller firms, contrary to the expectation that larger firms would benefit more due to economies of scale in regulatory compliance costs. In line with Becker et al. (2013), one possible explanation is that diseconomies of scale in compliance costs could also arise. This could be due to certain advantages of smaller firms over larger ones, such as greater flexibility and simpler reaction processes, enabling smaller firms to respond more quickly to regulatory pushes (Aragón-Correa et al., 2008; Chen and Hambrick, 1995; Fiegenbaum and Karnani, 1991). Another plausible explanation for these findings is that smaller firms are, on average, especially in the context of a developing economy, further away from the technology frontier. Thus, they have more room to catch up. Prior research on distance-to-frontier has shown that the larger the technology gap, the more a firm can benefit from the technology produced by the frontier, thereby resulting in a higher rate of productivity growth (Madsen et al., 2010; Blalock and Gertler, 2009; Keller and Yeaple, 2009).

4.2.2. Heterogeneity analysis by firm age

Next, we investigate how the impact of ETS on labour productivity changes with firm age. We divide our sample into two sub-samples, "Old" and "Young" firms, using the median value of firm age (11 years) as the threshold. We expect that the impact of ETS may vary across firms' ages, as previous literature indicates that firm age plays a significant role in investments in R&D and adoption of innovations, which, in turn, affect productivity (Coad et al., 2016).

Estimation results relating to our comparison of firms above and below the median firm age are reported in Panel B of Table 4. In column (3), firms above the median age exhibit an estimated treatment effect of 0.0238 (2.38%). In contrast, firms below the median age show a more substantial impact, with an estimated 0.1102 (11.02%) increase in productivity, as seen in column (4). This difference according to firm age, apart from being substantive, is also statistically significant (Z = 2.43, *p-value* = 0.008).

In sum, our results provide evidence pointing out that the impact of ETS on labour productivity is stronger for younger firms compared to older ones. A plausible explanation is that younger firms are more prone to innovate and to engage in radical innovations, which allows them to enhance their technological competitiveness and, in turn, speed up productivity growth (Coad et al., 2016; Alon et al., 2018; Fu, 2012; Dunne et al., 1989). On the contrary, older firms suffer from certain drawbacks, such as organisational inertia, that hinder their ability to translate R&D investment into higher growth rates (Coad et al., 2016; Yamakawa et al., 2011).

4.2.3. Heterogeneity analysis by ownership structure

Finally, we examine differences in ownership structure by comparing state-owned firms (SOFs) to non-state-owned firms (NSOFs). State-owned firms are those where the state holds >50% of the shares, or where the state is the ultimate controller of firms even if the state holds

[13] We also test if there is any regional heterogeneity in the impact of ETS by dividing our sample into developed and less developed regions based on the Chinese National Bureau of Statistics' classification. We find, however, no evidence for any significant regional differences in ETS impacts.

[14] To some extent, all firms in our sample are large, as they all have emissions over 10,000 tons. As such, we are looking for relative differences according to size as opposed to comparing large and small firms. We keep the labels "large" and "small" for ease of description.

[15] We use the number of employees as an alternative measurement of firm size to examine the heterogeneous effects of ETS. The large and small firms are divided based on the median value of their employees. The estimated treatment impact on small firms is 5.87%, compared to 1.69% for large firms. We end up with a consistent conclusion, although this difference is not statistically significant (Z = 1.17, *p-value* = 0.121).

Table 4
Heterogeneity analysis results.

	Panel A: Size		Panel B: Age		Panel C: Ownership	
	Large	Small	Old	Young	SOFs	NSOFs
	(1)	(2)	(3)	(4)	(5)	(6)
$ETS_{i,t}$	0.0252	0.0707**	0.0238	0.1102***	0.0846***	0.0419**
	(0.0197)	(0.0289)	(0.0206)	(0.0291)	(0.0301)	(0.0197)
Constant	2.3718***	3.3540***	1.5084**	4.1522***	2.6262***	2.9195***
	(0.5767)	(0.6415)	(0.6725)	(0.6761)	(0.7325)	(0.4981)
No. of Obs.	26,360	26,394	26,997	25,757	12,291	40,463
Control variables	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Industry by year FE	YES	YES	YES	YES	YES	YES
Region by year FE	YES	YES	YES	YES	YES	YES

Note: Standard errors clustered at the firm level are presented in parentheses. ***, **, and * stand for 1%, 5%, and 10% significance levels, respectively. FE indicates the fixed effects.

no >50% of the shares. It is reasonable to expect that the ETS might affect state-owned firms differently due to their close ties with the government, potentially providing them with greater access to resources (such as market opportunities, external funding, and low-interest-rate loans). These resources could enable them to react more efficiently to the ETS (Hering and Poncet, 2014; Li et al., 2008; Wu, 2018). Contrastingly, NSOFs may face lower government intervention and higher management flexibility (Wang et al., 2017). Panel C of Table 4 reports the estimation results related to our comparison of SOFs and NSOFs. The estimated impact of the ETS is positive and statistically significant for both groups. In terms of size, we find an estimated treatment effect of 0.0846 for SOFs and 0.0419 for NSOFs. We note here that while this difference is substantial, it is not a statistically significant difference (Z = 1.19, *p-value* = 0.117).

5. Robustness analyses

5.1. Parallel trend test

A fundamental assumption underlying the valid estimation of the DID model is the parallel trend assumption, which requires the average labour productivity of the treatment and control groups to follow a parallel trend in the period before ETS implementation (Bai and Clark, 2018; Lechner, 2011). If this assumption is invalid, our estimated impacts of ETS might be attributable to differences (e.g., pre-ETS efforts) in the pre-treatment trends of labour productivity between groups. We test this assumption by generating a set of relative-time indicators as illustrated in Eq. (2) below (Jung and Polasky, 2018; Li et al., 2022).

$$y_{i,t} = \Omega_0 + \Omega_1 Before_{i,t}^{-2} + \Omega_2 Before_{i,t}^{-1} + \Omega_3 After_{i,t}^{0} + \Omega_4 After_{i,t}^{1} + \Omega_5 After_{i,t}^{2} + \Omega_6 Control_{i,t} + F_i + T_t + R_{r,t} + I_{s,t} + \varepsilon_{i,t} \tag{2}$$

where $Before_{i,t}^{-k}$ (k = 1, 2) in Eq. (2) is equal to one in the kth year prior to the year a firm *i* was treated (regulated) and zero otherwise. $After_{i,t}^{k}$ (k = 0, 1, 2) is equal to one in the kth year since a firm *i* was treated and zero otherwise. All other terms are the same as those we generated in Eq. (1). The indicator, $Before_{i,t}^{-1}$, for the year prior to ETS operation is excluded to avoid multicollinearity and used as the comparison period (i.e., reference period or base year) following the standard practice in the literature to date (Hu et al., 2023; Cao et al., 2022). As we have only two relative-time indicators prior to ETS operation and $Before_{i,t}^{-1}$ is excluded, the parameter Ω_1 captures the difference between the difference in labour productivity between treated and control groups in $Before_{i,t}^{-2}$ relative to that in$Before_{i,t}^{-1}$ and thus tests the parallel trend assumption. A statistically insignificant term $Before_{i,t}^{-2}$ would provide some initial evidence in favour of the parallel trend assumption. Parameters Ω_3 to Ω_5 capture the post-treatment effect dynamics relative to $Before_{i,t}^{-1}$. The estimated results of Eq. (2) are given in Table 5.

Columns (1) and (2) in Table 5 report the regression results of the DID and PSM-DID models, respectively. The coefficients on $Before_{i,t}^{-2}$ in both columns are negative and insignificant, suggesting no significant difference in labour productivity between the treated and untreated groups in the pre-treatment period. The coefficients for $After_{i,t}^{0}$, $After_{i,t}^{1}$, and $After_{i,t}^{2}$ are all positive, indicating that firms in the treatment group produced higher labour productivity than the control group after being treated in the post-treatment periods compared to the year before the ETS was introduced, albeit lacking statistical significance for the coefficient on $After_{i,t}^{1}$.

The results in Table 5 provide some initial evidence, therefore indicating that pre-treatment trends are not biasing our estimates. However, we recognise we are limited in having only two pre-treatment periods at our disposal. Later, we report the results from a placebo test, which helps to further exclude the possibility that other unobservable factors might have biased our estimation results.

5.2. Test for SUTVA

Another critical assumption for unbiased estimation of treatment effects is the stable unit treatment value assumption (SUTVA). For this assumption to hold, the exogenous shocks of an ETS should only affect regulated firms, with no spillover impacts on unregulated firms. Failing

Table 5
Testing results of parallel trends assumption.

	DID	PSM-DID
	(1)	(2)
$Before^{-2}$	−0.0159	−0.0165
	(0.0142)	(0.0142)
$After^{0}$	0.0454***	0.0454***
	(0.0153)	(0.0153)
$After^{1}$	0.0250	0.0248
	(0.0207)	(0.0207)
$After^{2}$	0.0560**	0.0566**
	(0.0288)	(0.0288)
No. of Obs.	52,796	52,754
Control variables	YES	YES
Firm FE	YES	YES
Year FE	YES	YES
Industry by year FE	YES	YES
Region by year FE	YES	YES

Note: Standard errors clustered at the firm level are presented in parentheses. *** and ** stand for 1% and 5% significance levels, respectively. FE indicates the fixed effects.

to satisfy this assumption may bias our estimated treatment effects. There are a number of reasons why this assumption may not hold, and it is a common issue facing studies in this area. First, an increase or decrease in the production of regulated firms may lead to an opposite change in that of nearby unregulated firms for a given demand function. Second, regulated power firms may pass through their extra compliance costs through rising electricity prices, which may also impact unregulated firms in the same regions. Third, to be exempted from the ETS, large emitters who have not been regulated in pilot regions may maintain their emissions below the threshold by reducing production size (Marin et al., 2018).

Unfortunately, there is no standard way in which we can test this assumption. Considering that China's ETS covers a small percentage of firms in seven pilot regions, we conjecture that the most likely spillovers are among firms in the pilot regions themselves. While we cannot formally test for the violation of the SUTVA assumption, we repeat our baseline DID and PSM-DID analyses, this time excluding the unregulated firms in the pilot regions from our control groups. If the SUTVA is held, we expect no significant changes in estimates pertaining to the impact of ETS. This is because if our control group includes any kind of spillover and the SUTVA is violated, we would overestimate or underestimate the counterfactual estimates and bias our estimates of ETS impacts. As seen in Table 6, excluding unregulated firms in the pilot regions does not lead to any significant change to our estimates reported in Table 3.

5.3. Estimation results based on alternative matching approaches

In Table 3 (Column 7), we outlined the results from the DID model, where we relied on one-to-three NN matching with a calliper to ensure that our treatment and control groups had similar distributions of baseline characteristics, much like what would be observed under random assignment. Here we test the sensitivity of our results to different matching approaches, namely radius matching and kernel matching methods.[16] As we can see in Table 7 below, our results remain qualitatively the same under both matching approaches.

Overall, whether we rely on a parametric DID model or a nonparametric matching approach, we find no evidence for any harmful impact of the ETS on the labour productivity of regulated firms. Indeed, we observe a modest positive impact across all specifications. In the following sections, we test whether these findings hold first in a placebo test relying on simulated data and second when using a different dataset consisting just of firms listed on the stock exchange.

Table 6
Estimation results after excluding the unregulated firms in pilots.

	DID	PSM-DID
	(1)	(2)
$ETS_{i,t}$	0.0418**	0.0422**
	(0.0166)	(0.0166)
Constant	2.6381***	2.6386***
	(0.3843)	(0.3918)
Control variables	YES	YES
Firm FE	YES	YES
Year FE	YES	YES
Industry by year FE	YES	YES
Region by year FE	YES	YES
No. of Obs.	45,217	45,198

Note: Standard errors clustered at the firm level are presented in parentheses. *** and ** stand for 1% and 5% significance levels, respectively. FE indicates the fixed effects.

Table 7
Estimation results of alternative PSM-DID approaches.

	Radius	Kernel
	(1)	(2)
$ETS_{i,t}$	0.0483***	0.0483***
	(0.0163)	(0.0163)
Constant	2.5110***	2.5110***
	(0.3834)	(0.3834)
Control variables	YES	YES
Firm FE	YES	YES
Year FE	YES	YES
Industry by year FE	YES	YES
Region by year FE	YES	YES
No. of Obs.	52,754	52,754

Note: Standard errors clustered at the firm level are presented in parentheses. *** stands for the 1% significance level. FE indicates the fixed effects.

5.4. Placebo test

To exclude the possibility that other unobservable factors might drive our estimation results, we implemented a placebo test (see Cai et al., 2016). In this test, instead of using regulated firms as our treatment group, we randomly assigned firms to a treatment group. Specifically, as our sample covers 1320 treated firms, we randomly select 1320 firms from the sample to act as our treatment group, with the remainder serving as our control group. We also randomly assign the treatment timings of these randomly selected treated firms at the same time. Next, we repeated our DID approach specified in Eq. (1), but using this artificially selected treatment group, and repeated this process 500 times. In essence, we have 500 estimates relating to the impact of the ETS on these 'false' treatment groups. These estimates are plotted in Fig. 2, and we can see that the estimates as a whole converge to zero. The horizontal dashed line refers to a p-value of 0.1, and the vertical dashed line indicates our true PSM-DID estimation results (i.e., 0.0483). As shown in Fig. 2, the corresponding p-values of most coefficients are above 0.1 and on the left side of our 'true' estimation results.

5.5. Further evidence based on other microdata

To further test the robustness of our estimation results, we repeat our analysis relating to the impact of the ETS on regulated firms, but this time using a smaller, more readily available dataset consisting of firms listed on China's Shenzhen and Shanghai stock exchanges. Our key outcome variable here is the total factor productivity (TFP), measured by the method proposed by Levinsohn and Petrin (2003).[17] TFP reflects efficiency in production from a given set of inputs rather than a single input factor (Syverson, 2011). We use TFP instead of labour productivity because of the difference in data availability between the listed firm database and our primary database.

After identifying the treated and untreated industrial firms from the listed firm panel dataset using the same sampling processes as our

[16] Instead of using only several NN within the calliper, radius matching uses as many untreated firms as within the specified range of propensity score (i.e., calliper) to match with each treated firm (Becker and Ichino, 2002; Caliendo and Kopeinig, 2008). Kernel matching matches treated firms with a weighted average of all untreated firms with weights in the inverse proportion of the difference between the treated and untreated groups in terms of propensity score (Becker and Ichino, 2002; Caliendo and Kopeinig, 2008).

[17] The method proposed by Levinsohn and Petrin (2003) is one of the dominant approaches to calculating TFP and helps address the simultaneity problems in the production function estimation by using the intermediate inputs as a proxy for the unobservable shocks. In practice, we use outputs (sales), labour inputs (the number of employees), capital inputs (fixed assets), and intermediate inputs (including sales expense, operating cost, financial expense, and management expense excluding wage bill and current depreciation and amortisation) to calculate TFP following the Stata command from Petrin et al. (2004).

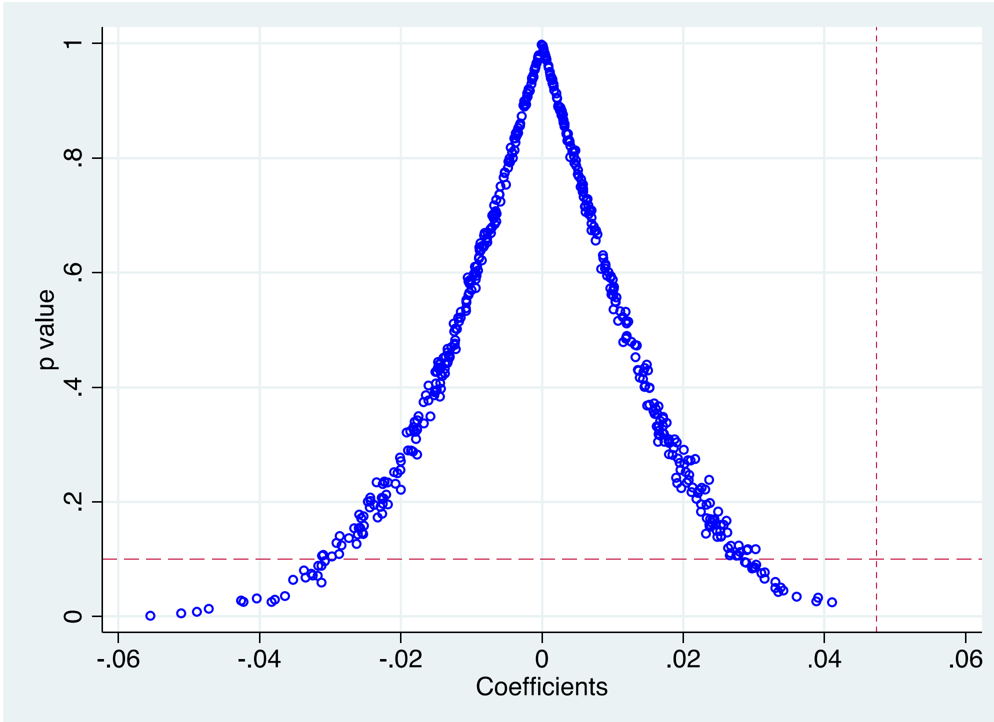

Fig. 2. Results of placebo test.

baseline analysis, we end up with a sample of 57 treated firms (277 observations) and 360 untreated firms (1775 observations). The estimation results from both a DID model and a PSM-DID model are illustrated in Table 8. Similar to our analysis using the Chinese Industrial Firm Database, we do not find any evidence for a negative impact of the ETS on regulated firms. Indeed, like what we observed for labour productivity, we observe a statistically significant positive estimated impact overall, which again provides evidence in favour of the strong version of the Porter hypothesis.

6. Conclusion

Across both developed and developing economies, there is increasing recognition of the importance of reducing GHG. ETS has been put forward as an important tool for facilitating this in a least-cost fashion or even as a mechanism for fostering both increases in productivity and reductions in emissions. However, most evidence relating to the effectiveness of an ETS comes from developed western economies or is limited to the use of listed firm data and aggregate data from developing economies.

Table 8
Impacts of CO_2 ETS on TFP.

	DID	PSM-DID
	(1)	(2)
$ETS_{i,t}$	0.0886**	0.0947**
	(0.0408)	(0.0412)
Constant	2.0672	2.3653*
	(1.7448)	(1.7934)
Control variables	YES	YES
Firm FE	YES	YES
Year FE	YES	YES
Industry by year FE	YES	YES
Region by year FE	YES	YES
No. of Obs.	2052	2019

Note: Standard errors clustered at the firm level are presented in parentheses. * and ** stand for 1% and 5% significance levels, respectively. FE indicates the fixed effects.

Our research addresses this research gap by examining the impact of the recent implementation of a CO_2 ETS in China on the labour productivity of industrial firms using newly released firm-level data collected by the Chinese statistical offices. Apart from documenting the overall impact for regulated firms, by taking advantage of our relatively large sample size, we also estimate how the estimated impact of ETS varies according to firm size, age, and ownership structure.

We can say that overall, our analysis, using a time-varying DID model both with and without propensity score matching, consistently presents robust evidence to suggest that the initial implementation of CO_2 ETS in China did not harm and indeed likely enhanced the labour productivity of regulated firms. This finding is robust to alternative datasets and matching approaches, as well as a placebo test where we assigned firms to the treatment and control groups randomly and repeated our baseline DID approach 500 times.

Our analysis also provides initial evidence, which suggests that the benefits from the ETS for labour productivity are concentrated on smaller and younger firms. This potential heterogeneity may have implications for any future widespread implementation of ETS across China. In policy terms, these results illustrate that market-based environmental tools can reduce pollution while simultaneously boosting the competitiveness of small and new firms in a developing economy.

More importantly, the findings of our heterogeneity analysis provide important insights into firm age and size differences as key underlying mechanisms in understanding why emission trading boots firms' labour productivity in China. Specifically, younger firms benefit from their inclination towards innovation (particularly radical innovation) and organisational agility (Coad et al., 2016; Yamakawa et al., 2011), whereas smaller firms gain advantages through their greater flexibility and the diseconomies of scale in compliance costs (Becker et al., 2013; Aragón-Correa et al., 2008). These age- and size-specific mechanisms, while not exhaustive and exclusive, offer important avenues for future research to explore extensively why ETS in China appears to have positive effects on firms' productivity.

CRediT authorship contribution statement

Rushi Chen: Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Resources, Software, Validation, Visualization, Writing – original draft, Writing – review & editing. **Peter Howley:** Conceptualization, Methodology, Supervision, Writing – review & editing. **Effie Kesidou:** Conceptualization, Methodology, Project administration, Supervision, Writing – review & editing.

Declaration of competing interest

All authors declare that they have no conflicts of interest.
There are no financial conflicts of interest to disclose.

Appendix A. Methods to exclude problematic observations

To exclude potential problematic observations, following the approach employed by Fang et al. (2020) and Wang et al. (2021), we dropped observations if the following conditions had been met:

1. Values for key variables (including employment, total liabilities, total assets, and gross output) are zero or missing, and firms for which we do not have data both before and after the implementation of the ETS.
2. Total assets are less than fixed assets or liquid assets.
3. Current depreciation is greater than accumulated depreciation.
4. The number of employees is fewer than 20, and sales turnover is <20 million RMB.

Specifically, dropping observations that meet condition 1 helps us eliminate any problems arising from missing values. In addition, we drop observations if they belong to 2 and 3 above because such values signal misreported data. To further exclude misreported and misclassified issues, observations that satisfy condition 4 are dropped simply because the Chinese Industrial Firm Database is only meant to cover firms above this designated size (i.e., those whose sales turnover has been over 20 million since 2011). As such, these observations are likely misclassified.

Appendix B. Matching quality after PSM

Table B.1
Balance tests before and after PSM.

Variable	Match statue	Mean		*%bias*	%reduct |bias|	*t*-test	
		Treated	Control			t	$p > \mid t \mid$
AGE	U	12.276	10.681	25.2***		12.85	0.000
	M	12.28	12.124	2.5	90.2	0.83	0.406
SIZE	U	13.395	13.061	20.5***		14.00	0.000
	M	13.397	13.442	−2.8	86.5	−0.99	0.324
DEBT	U	0.578	0.608	−10.6***		−5.05	0.000
	M	0.578	0.578	0.1	99.4	0.02	0.983
OWN	U	0.338	0.220	26.6***		13.63	0.000
	M	0.338	0.343	−1.0	96.1	−0.35	0.726
#Obs.	U	5389	47,407				
	M	5388	47,366				

Notes: *** indicates the 1% significance level. U refers to unmatched, and M refers to matched.

Appendix C. Estimation results without winsorizing the data

Table C.1
The DID regression results without winsorizing the data.

	DID						PSM-DID
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$ETS_{i,t}$	0.0515***	0.0570***	0.0480***	0.0496***	0.0492***	0.0494***	0.0501***
	(0.0172)	(0.0175)	(0.0167)	(0.0167)	(0.0167)	(0.0167)	(0.0167)
SIZE			0.3875***	0.3871***	0.3850***	0.3850***	0.3836***
			(0.0197)	(0.0197)	(0.0198)	(0.0198)	(0.0198)
AGE				−0.0877***	−0.0885***	−0.0882***	−0.0886***
				(0.0199)	(0.0200)	(0.0202)	(0.0203)
DEBT					−0.0641*	−0.0639*	−0.0821**
					(0.0351)	(0.0351)	(0.0371)
OWN						−0.0344	−0.0339
						(0.0478)	(0.0478)
Constant	6.8716***	6.5442***	1.6841***	2.6025***	2.6714***	2.6770***	2.7052***
	(0.0038)	(0.1576)	(0.2805)	(0.3644)	(0.3671)	(0.3677)	(0.3679)
No. of Obs.	52,796	52,796	52,796	52,796	52,796	52,796	52,724
Firm FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Industry by year FE	NO	YES	YES	YES	YES	YES	YES
Region by year FE	NO	YES	YES	YES	YES	YES	YES

Note: Standard errors clustered at the firm level are presented in parentheses. ***, **, and * stands for 1%, 5%, and 10% significance levels, respectively. FE indicates the fixed effect.

Appendix D. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.eneco.2024.107376.

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