



Carbon Emissions Trading and Corporate Financial Performance: Lagged Effects and Transmission Channels in China's Pilot Markets

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Abstract

Whether carbon pricing erodes or strengthens corporate profitability remains contested, and the evidence for emerging economies is mixed. This study treats China's pilot carbon emissions trading scheme (CET) as a quasi-natural experiment and estimates its effect on the financial performance of listed firms. Using 9,134 firm-year observations of A-share companies for 2009–2022 and a difference-in-differences design with firm fixed effects, year fixed effects and province-specific time trends, we find that CET raises return on assets by 1.83 percentage points and return on equity by 2.87 percentage points. The gain is not immediate. Yearly estimates are statistically insignificant in 2013–2015 and become significant only from 2016, which is consistent with the innovation-offset argument of the Porter hypothesis. The effect is concentrated in large firms and is absent in small firms. Mechanism tests indicate that eased financing constraints carry about one tenth of the total effect, whereas technological innovation carries less than two per cent. The results survive parallel-trend, placebo, alternative-outcome and propensity-score-matching checks. Pilot carbon markets therefore rewarded regulated firms with a delay, and small firms need transitional support to share in the gains.

Keywords: Carbon Emissions Trading, Corporate Financial Performance, Porter Hypothesis, Financing Constraints, Difference-in-Differences

Introduction

China is the world's largest emitter of carbon dioxide, and it has chosen a market instrument as a central tool for controlling those emissions. In October 2011 the National Development and Reform Commission approved pilot carbon emissions trading schemes in Beijing, Tianjin, Shanghai, Chongqing, Hubei, Guangdong and Shenzhen. The pilot markets opened between 2013 and 2014, and a national market covering the power sector followed in July 2021; Zhang, Yu, and Karplus (2025) review its development. Existing evidence shows that the pilots cut energy use and carbon emissions in regulated industries (Hu, Ren, Wang, & Chen, 2020) and lowered industrial carbon emissions in the pilot regions (Zhang, Li, Li, & Guo, 2020). Much less settled is what the pilots did to the firms that had to comply.

Two views compete. The traditional view holds that environmental regulation is a cost. Regulated firms must buy allowances, install abatement equipment and build monitoring and reporting systems, and these outlays can lower productivity (Gray & Shadbegian, 2003). The opposing view, associated with Porter and van der Linde (1995), holds that well-designed regulation triggers innovation whose benefits can offset, or even exceed, compliance costs. A tradable-permit system should be the regulatory design most favourable to this second view, because it prices emissions and leaves firms free to choose how to abate (Coase, 1960; Dales, 1968).

The empirical record does not settle the matter. For the European Union Emissions Trading System, the most careful studies find emission cuts without measurable damage to firms' revenues, profits or employment (Dechezleprêtre, Nachtigall, & Venmans, 2023; Marin, Marino, & Pellegrin, 2018). For China, the findings diverge. Some studies report higher profitability and firm value among pilot firms (Chu, Dong, Liu, Ma, & Wang, 2024; Tang, Cheng, Guo, Ma, & Hu, 2022). Others report higher leverage and lower market value (Liu, Zhou, Lu, & Hu, 2021), a short-run fall in enterprise value (He, Wei, Fang, & Cao, 2022), or effects that change sign across industries and over time (Sun, Wang, Wang, & Zhang, 2022; Zhang & Liu, 2019).

Three gaps help to explain this divergence. First, many studies estimate a single average effect over a short post-policy window. If compliance costs arrive at once and innovation offsets arrive later, the estimated sign will depend on where the window ends. Second, the channels are rarely compared on a common footing. Innovation is the channel the Porter hypothesis emphasises, yet carbon regulation may also change a firm's access to finance, and here the evidence conflicts sharply: several studies find that the pilots raised borrowing costs and tightened financing constraints (Huang, He, Luo, & Shen, 2024; Ni, Jin, & Huang, 2022; Zhang & Qi, 2025), while another finds that constraints eased for high-emission firms (He & Zhang, 2024). Third, evidence on firm size concerns investment and green innovation (Liu & Liu, 2023; Zhao, Li, & Cheng, 2024) rather than profitability.

These gaps matter beyond the pilot regions. China's national carbon market began with the power sector in 2021 (Zhang, Yu, & Karplus, 2025) and is being extended to steel, cement and aluminium, so thousands more firms will soon face a carbon price. Regulators deciding how quickly to tighten the market, investors pricing carbon-exposed firms and managers planning compliance all need to know whether that price erodes or strengthens profitability, how long any adjustment takes and which firms gain. Most evidence on the Porter hypothesis under carbon pricing comes from the European Union, where firms operate with mature capital markets and long regulatory experience. Whether the same mechanisms work in an emerging economy with a young carbon market, extensive state ownership and bank-dominated finance remains an open question, and the pilots offer the only firm-level evidence for China with which to answer it.

This study addresses these gaps with a panel of 9,134 firm-year observations of Chinese A-share listed companies for 2009–2022. Using a difference-in-differences design, it asks three questions. Does the pilot scheme improve return on assets and return on equity, and when does any improvement appear? Does the effect differ between large and small firms?

Through which channel does it operate: technological innovation, financing constraints, or both?

The study makes three contributions. It traces the effect year by year from 2013 to 2020 and shows that the profitability gain is statistically indistinguishable from zero for the first three years and significant from 2016 onward. This timing offers one explanation for the mixed results of studies with shorter windows. It also places the innovation and financing channels in the same framework and finds that eased financing constraints carry several times more of the effect than innovation does, a result that qualifies the usual innovation-centred reading of the Porter hypothesis. Finally, it shows that the gain accrues to large firms only, which has direct implications for how the national market should treat smaller emitters. Taken together, these results add emerging-economy evidence to a literature dominated by European studies and extend the Porter hypothesis debate from whether carbon pricing pays to when it pays, for whom and through which channel. They also carry practical lessons. For regulators, the three-year adjustment period and the absence of gains among small firms bear on the pace of the national market's expansion and on the support smaller emitters will need. For investors, the earnings effect of carbon regulation should be assessed over several years rather than in the year of compliance. For managers, a firm's financial position, more than its patent output, determines how much of a carbon price it can turn into advantage.

The remainder of the paper reviews the literature and develops the hypotheses, describes the data and method, reports the results and robustness checks, examines heterogeneity and mechanisms, and concludes with policy implications and limitations.

Literature Review and Hypotheses

Theoretical background

Carbon emissions trading rests on the property-rights approach to externalities. Coase (1960) showed that, when rights are well defined and transaction costs are low, bargaining leads to an efficient allocation, and Dales (1968) applied this insight to pollution by proposing tradable emission permits. A cap fixes total emissions, and trading lets abatement take place where it is cheapest. Firms that can cut emissions cheaply sell allowances, and firms that cannot must buy them.

The Porter hypothesis adds a dynamic argument. Porter and van der Linde (1995) argue that regulation exposes inefficiencies in resource use and pushes firms to innovate, and that the resulting process and product improvements can offset compliance costs. Ambec, Cohen, Elgie, and Lanoie (2013) distinguish a weak version, in which regulation stimulates innovation, from a strong version, in which that innovation raises overall competitiveness. Ambec and Lanoie (2008) list the routes by which better environmental performance can pay: lower material and energy costs, lower cost of capital, access to new markets and product differentiation. The strong version is an empirical claim, and its timing matters. Compliance costs are immediate, whereas the returns to new processes and products take years to materialise.

Carbon trading and corporate financial performance

Evidence from the European Union generally shows that carbon trading did not harm regulated firms. Dechezleprêtre et al. (2023) find that the system cut emissions with no

negative effect on revenue, profits, employment or assets, and Marin et al. (2018) reach a similar conclusion across several performance measures. Oestreich and Tsiakas (2015) show that German firms that received free allowances earned higher stock returns in the early phases of the system.

Results for China are less uniform. Chu et al. (2024) find that the pilots raised profitability and firm value and lowered operating costs, and Tang et al. (2022) report a higher market value for pilot firms. In contrast, Liu et al. (2021) observe a higher asset-liability ratio and a lower market value, and He et al. (2022) find that enterprise value falls in the short run before the effect fades. Timing appears to be central. Zhang and Liu (2019) show that, in the chemical, paper and aviation industries, the effect on financial performance turns from negative to positive two to four years after the launch. Sun et al. (2022) also document effects that vary across the post-policy years.

These findings are consistent with a sequence in which compliance costs come first and offsets follow. In the early years of the pilots, allowance allocation rules were still being adjusted, firms were learning how to trade, and abatement investments had not yet paid back. The first hypothesis follows.

H1. The carbon emissions trading scheme improves the financial performance of pilot firms, and the improvement emerges with a lag after implementation.

Firm size

Large and small firms differ in their capacity to turn a carbon price into an advantage. Large firms can spread the fixed costs of monitoring, reporting and carbon-asset management over more output. They have more internal funds for abatement equipment and research, and they usually enjoy better credit terms. Small firms face the same compliance tasks with fewer resources and less bargaining power in allowance markets. Existing evidence on size relates to other outcomes: Liu and Liu (2023) find that the scheme promotes green innovation in firms with large capital and discourages it in small ones, and Zhao et al. (2024) find that the fall in investment under the scheme is greater for small firms. Whether the same asymmetry holds for profitability has not been established.

H2. The positive effect of the carbon emissions trading scheme on financial performance is stronger for large firms than for small firms.

Technological innovation as a channel

The first candidate channel is innovation. A carbon price raises the return to technologies that save energy or reduce emissions. Caeli and Dechezleprêtre (2016) show that the European system increased low-carbon patenting among regulated firms. For China, Cui, Zhang, and Zheng (2018) and Zhu, Fan, Deng, and Xue (2019) find that the pilots induced low-carbon innovation, and a simulation of thermal power enterprises by Wei, Zhu, and Tan (2022) shows that trading induces innovation once the carbon price is high enough. Innovation, in turn, has been linked to firm outcomes under the scheme: studies of total factor productivity identify innovation channels, including green patents (Wang, Yang, & Zhang, 2022), research and development inputs (Wu & Wang, 2022) and an innovation effect that outweighs the cost effect (Cheng & Meng, 2023).

H3. Technological innovation mediates the positive relationship between the carbon emissions trading scheme and corporate financial performance.

Financing constraints as a channel

The second candidate channel is finance. When external funds are costly, firms forgo profitable projects, and their investment depends on internal cash flow (Fazzari, Hubbard, & Petersen, 1988; Kaplan & Zingales, 1997). Carbon regulation could move financing constraints in either direction. It may tighten them, because lenders price in compliance costs and regulatory risk; Ni et al. (2022), Huang et al. (2024) and Zhang and Qi (2025) report higher debt costs or tighter constraints for pilot firms. It may also loosen them. Allowances are tradable assets that can generate revenue or serve as collateral. The scheme's monitoring, reporting and verification rules oblige firms to disclose emissions data, which narrows the information gap between firms and investors. Lenders also penalise environmental risk (Chava, 2014), so firms that manage carbon well may obtain funds on better terms. He and Zhang (2024) find that financing constraints eased for high-emission firms in pilot regions. The direction is therefore an empirical question. If constraints ease, firms can invest closer to the optimum and carry lower financing costs, and profitability should rise.

H4. The alleviation of financing constraints mediates the positive relationship between the carbon emissions trading scheme and corporate financial performance.

Methodology

Policy setting and identification

The pilot scheme provides a quasi-natural experiment. The pilot regions were designated by the central government in 2011, and trading began in 2013 and 2014. Firms covered by the pilots faced a binding carbon price, while comparable firms elsewhere did not. This contrast allows a difference-in-differences (DID) comparison of the change in financial performance of pilot firms with the change among non-pilot firms over the same years. Firm fixed effects remove time-invariant differences between the two groups, and year fixed effects remove shocks common to all firms (Bertrand, Duflo, & Mullainathan, 2004).

The treatment group consists of listed firms headquartered in the seven pilot regions, and the control group consists of listed firms headquartered in non-pilot provinces. Shenzhen is a sub-provincial city within Guangdong, so the two pilots are treated as one geographical unit. The policy indicator switches on in 2013, the year in which the first pilot markets opened. A common date is used for all pilot regions; for regions whose markets opened in 2014, this counts one pre-opening year as treated, which biases the estimate toward zero.

Because assignment is by location and not by the lists of regulated emitters, the estimate is an intention-to-treat effect. It averages over covered firms and other firms in the pilot regions, and it is therefore likely to understate the effect on covered firms.

Sample and data

The initial sample comprises Chinese A-share listed companies from 2009 to 2022, which gives four pre-policy years. The sample focuses on the carbon-intensive industries targeted by the pilots: electric power and heat, steel, cement, electrolytic aluminium, petrochemicals, chemicals, paper and aviation. Financial firms are excluded because their accounting rules

and capital structures differ from those of other firms. Firms under special treatment (ST) status are excluded because of their abnormal financial condition, and observations with missing values for the main variables are dropped. The final sample is an unbalanced panel of 9,134 firm-year observations.

Financial statement, ownership and governance data come from the China Stock Market and Accounting Research (CSMAR) database. Patent data come from the China Research Data Services (CNRDS) platform. Supplementary financial indicators come from the WIND database.

Baseline model

The baseline specification is:

$$ROA_{it} = \beta_0 + \beta_1 DID_{it} + \gamma' X_{it} + \mu_i + \lambda_t + \rho_p \cdot t + \varepsilon_{it} \quad (1)$$

ROA is the return on assets of firm i in year t . DID equals one for a pilot firm in the years after the policy takes effect and zero otherwise. X is a vector of firm-level controls. The terms μ and λ are firm and year fixed effects, and $\rho \cdot t$ is a province-specific linear time trend that absorbs smooth regional differences in economic development and policy stance. The coefficient β_1 measures the average effect of the scheme on pilot firms. Inference is based on robust standard errors.

To trace the timing of the effect, the single policy indicator is replaced by a set of interactions between the treatment-group dummy and year dummies for 2013 to 2020:

$$ROA_{it} = \beta_0 + \sum_{k=2013}^{2020} \beta_k (Treat_i \times Year_k) + \gamma' X_{it} + \mu_i + \lambda_t + \rho_p \cdot t + \varepsilon_{it} \quad (2)$$

Each β with subscript k gives the difference between pilot and non-pilot firms in year k relative to the pre-policy period. An event-study version that also includes pre-policy years, with 2012 as the reference year, is used to test the parallel-trend assumption (Beck, Levine, & Levkov, 2010).

Mechanism model

The two channels are examined with the stepwise procedure of Baron and Kenny (1986). Equation (1) gives the total effect. Equations (3) and (4) give the effect of the scheme on the mediator M and the effect of the mediator on performance conditional on the scheme:

$$M_{it} = \alpha_0 + \alpha_1 DID_{it} + \gamma' X_{it} + \mu_i + \lambda_t + \rho_p \cdot t + \varepsilon_{it} \quad (3)$$

$$ROA_{it} = \delta_0 + \delta_1 DID_{it} + \delta_2 M_{it} + \gamma' X_{it} + \mu_i + \lambda_t + \rho_p \cdot t + \varepsilon_{it} \quad (4)$$

The indirect effect is the product $\alpha_1 \times \delta_2$, and its significance is assessed with the Sobel (1982) test. M is either technological innovation or the financing-constraint index.

Variables

The dependent variable is return on assets (ROA), net income divided by total assets. Accounting-based measures are preferred to market-based ones because share prices in China's market are volatile and can depart from operating performance. Return on equity (ROE) is used as an alternative measure.

Technological innovation (Patent) is measured by the firm's green patent stock. It is the natural logarithm of one plus the firm's green patent applications over the preceding three years, depreciated at 15 per cent a year by the perpetual inventory method. Green patents are identified with the IPC Green Inventory of the World Intellectual Property Organization.

A stock is preferred to an annual flow because profitability depends on accumulated knowledge more than on one year's filings. Financing constraints are measured by the KZ index of Kaplan and Zingales (1997), following Lamont, Polk, and Saá-Requejo (2001):

$$KZ = -1.002 \frac{\text{CashFlow}}{\text{Assets}} + 0.283 Q + 3.139 \frac{\text{Debt}}{\text{Capital}} - 39.368 \frac{\text{Dividends}}{\text{Assets}} - 1.315 \frac{\text{Cash}}{\text{Assets}}$$

A higher value indicates tighter constraints.

The controls capture firm characteristics known to affect profitability: scale, age, ownership, cash generation, capital structure, operating efficiency and governance (Jiang & Kim, 2015; Shleifer & Vishny, 1997). Table 1 defines all variables.

Table 1

Variable definitions

Variable	Name	Measurement
ROA	Return on assets	Net income / total assets
ROE	Return on equity	Net income / shareholders' equity
DID	Policy indicator	1 for pilot firms after the policy takes effect, 0 otherwise
Patent	Technological innovation	ln(1 + green patent stock)
KZ	Financing constraints	KZ index; higher values mean tighter constraints
Size	Firm size	ln(total assets)
Age	Firm age	ln(1 + years since establishment)
Owner	Ownership type	1 if state-owned, 0 otherwise
Ocf	Operating cash flow	Net operating cash flow / total assets
ER	Equity ratio	Total liabilities / shareholders' equity
Sales	Asset turnover	Operating revenue / total assets
Largest	Ownership concentration	Shareholding of the largest shareholder
Idr	Board independence	Independent directors / board size

Results

Descriptive statistics

Table 2 summarises the variables. Mean ROA is 4.7 per cent and mean ROE is 8.3 per cent, and both vary widely across firms. The KZ index ranges from -10.39 to 8.83, so the sample includes firms with ample internal funds as well as severely constrained ones. State-owned firms account for 61.8 per cent of observations, and the largest shareholder holds 36.3 per cent of shares on average.

Table 2

Descriptive statistics (N = 9,134)

Variable	Mean	Std. dev.	Min	Median	Max
ROA	0.047	0.057	-0.644	0.036	0.478
ROE	0.083	0.108	-1.916	0.073	0.883
Patent	2.105	1.841	0.000	2.079	9.549
KZ	1.349	2.069	-10.390	1.620	8.834
Size	22.610	1.374	19.050	22.460	28.640
Age	2.820	0.338	0.693	2.890	3.689
Owner	0.618	0.486	0.000	1.000	1.000
Ocf	0.061	0.068	-0.470	0.058	0.484
ER	1.188	1.319	0.007	0.922	46.380
Sales	0.736	0.508	0.022	0.633	7.609
Largest	0.363	0.149	0.034	0.343	0.900
ldr	0.369	0.054	0.091	0.333	0.800

Baseline results

Table 3 reports estimates of Equation (1). The coefficient on DID is positive and significant at the 1 per cent level in every column. With firm and year fixed effects only, it is 0.0142. Adding the controls leaves it almost unchanged at 0.0135. Adding province-specific time trends raises it to 0.0196 without controls and 0.0183 with controls. The stability of the estimate across specifications suggests that it is not driven by observable firm characteristics or by smooth regional trends.

The full specification in column (4) implies that the scheme raised the ROA of pilot firms by 1.83 percentage points relative to non-pilot firms. This is a large effect: it equals about 39 per cent of the sample mean and about one third of a standard deviation. The first part of H1 is supported.

The controls behave as expected. Larger firms, firms with stronger operating cash flow, higher asset turnover and a larger controlling shareholder are more profitable. Older firms, state-owned firms and more indebted firms are less profitable. Board independence has no significant association with ROA.

Table 3

Carbon emissions trading and return on assets

	(1) ROA	(2) ROA	(3) ROA	(4) ROA
DID	0.0142*** (4.81)	0.0135*** (5.13)	0.0196*** (5.02)	0.0183*** (5.22)
Size		0.0092*** (5.66)		0.0088*** (5.35)
Age		-0.0220*** (-3.11)		-0.0184** (-2.45)
Owner		-0.0135*** (-3.15)		-0.0126*** (-2.85)
Ocf		0.2275*** (14.65)		0.2299*** (14.59)
ER		-0.0108*** (-6.41)		-0.0108*** (-6.19)
Sales		0.0225*** (5.18)		0.0208*** (4.71)

	(1) ROA	(2) ROA	(3) ROA	(4) ROA
Largest		0.0240*** (2.83)		0.0278*** (3.17)
Idr		-0.0186 (-1.45)		-0.0136 (-1.03)
Constant	0.0457*** (85.47)	-0.1120*** (-2.85)	0.0452*** (78.23)	-0.1160*** (-2.91)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Province time trends	No	No	Yes	Yes
Observations	9,134	9,134	9,134	9,134
R ²	0.5092	0.5976	0.5345	0.6168
Adjusted R ²	0.4637	0.5597	0.4641	0.5583

Note. t-statistics based on robust standard errors in parentheses. ***, ** and * denote significance at the 1, 5 and 10 per cent levels.

Dynamic effects

Table 4 reports estimates of Equation (2). The yearly coefficients are positive throughout, but they are small and statistically insignificant in 2013, 2014 and 2015. The coefficient becomes significant at the 10 per cent level in 2016 and at the 5 per cent level or better in every year from 2017 to 2020. With province time trends, the estimate rises from 0.0051 in 2013 to a peak of 0.0294 in 2018 and remains above 0.019 in 2019 and 2020.

The profitability gain therefore took about three years to appear. This pattern supports the second part of H1 and fits the sequence that the Porter hypothesis implies. In the first years, allowance allocation rules were still being refined, trading was thin and firms were learning to manage a new compliance obligation. Abatement investment and process changes made in those years could affect earnings only later.

Table 4

Dynamic effects of carbon emissions trading on return on assets

	(1) ROA	(2) ROA
Treat × 2013	0.0046 (0.79)	0.0051 (0.70)
Treat × 2014	0.0086 (1.33)	0.0023 (0.28)
Treat × 2015	0.0114 (1.60)	0.0109 (1.27)
Treat × 2016	0.0099* (1.81)	0.0135* (1.93)
Treat × 2017	0.0174*** (2.60)	0.0218*** (2.59)
Treat × 2018	0.0137** (2.16)	0.0294*** (3.24)
Treat × 2019	0.0139** (2.40)	0.0190** (2.46)
Treat × 2020	0.0169*** (2.98)	0.0224** (2.31)
Controls	Yes	Yes
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Province time trends	No	Yes
Observations	9,134	9,134
R ²	0.5981	0.6177

Note. t-statistics based on robust standard errors in parentheses. ***, ** and * denote significance at the 1, 5 and 10 per cent levels.

Robustness Checks

Parallel trends

The DID estimate is valid only if pilot and non-pilot firms would have followed the same profitability path without the policy. Figure 1 plots event-study coefficients with 2012 as the reference year. The coefficients for 2009, 2010 and 2011 are close to zero, and their 95 per cent confidence intervals all include zero, so there is no sign of diverging trends before the policy. After 2013 the coefficients rise gradually. The confidence intervals still include zero in 2013 to 2015 and move clearly above zero from 2017. The figure thus confirms both the identifying assumption and the lagged pattern in Table 4.

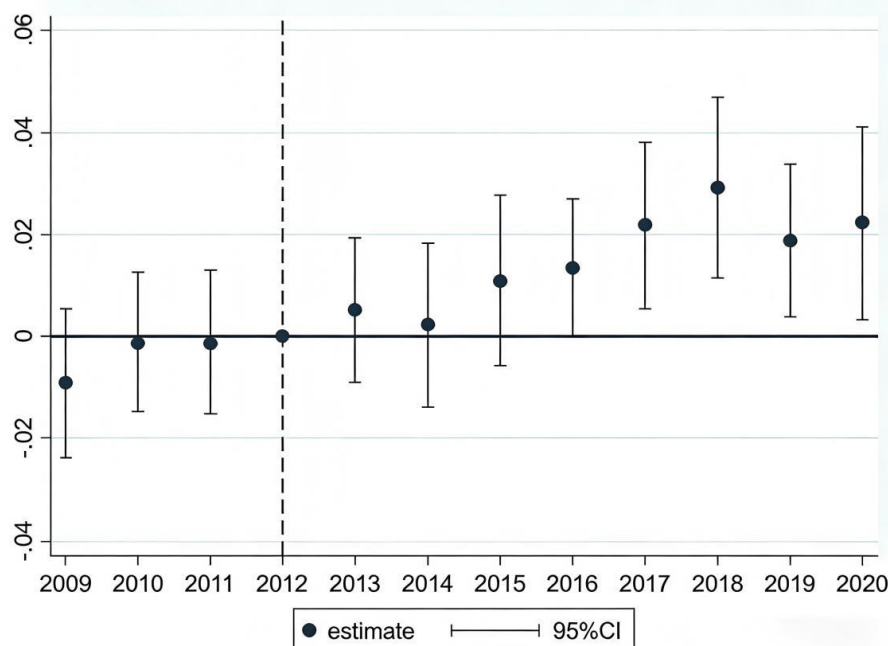


Figure 1 Parallel-trend test: event-study estimates of the effect on ROA with 95 per cent confidence intervals

Alternative dependent variable

Table 5 replaces ROA with ROE. The DID coefficient lies between 0.0249 and 0.0287 and is significant at the 1 per cent level in all four specifications. In the full specification the scheme raises ROE by 2.87 percentage points, about 35 per cent of the sample mean. The conclusion does not depend on how profitability is measured.

Table 5

Alternative dependent variable: return on equity

	(1) ROE	(2) ROE	(3) ROE	(4) ROE
DID	0.0249*** (3.83)	0.0254*** (4.24)	0.0281*** (3.48)	0.0287*** (3.90)
Controls	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Province time trends	No	No	Yes	Yes
Observations	9,134	9,134	9,134	9,134
R ²	0.3869	0.5069	0.4263	0.5367

Note. t-statistics based on robust standard errors in parentheses. *** denotes significance at the 1 per cent level.

Placebo test

Unobserved shocks that happened to coincide with the policy could produce a spurious effect. To assess this risk, treatment status and policy timing are assigned at random. In each round a pseudo-treatment group of the same size as the actual one is drawn from the sample, each pseudo-treated firm receives a random policy year, and the full specification is re-estimated. The procedure is repeated 1,000 times.

Figure 2 plots the 1,000 placebo coefficients against their p-values. The estimates are centred on zero and almost all of them fall between -0.015 and 0.015 . The actual estimate of 0.0183 , marked by the vertical dashed line, lies in the extreme right tail of the distribution. A random assignment of treatment is therefore very unlikely to generate an effect of the size observed.

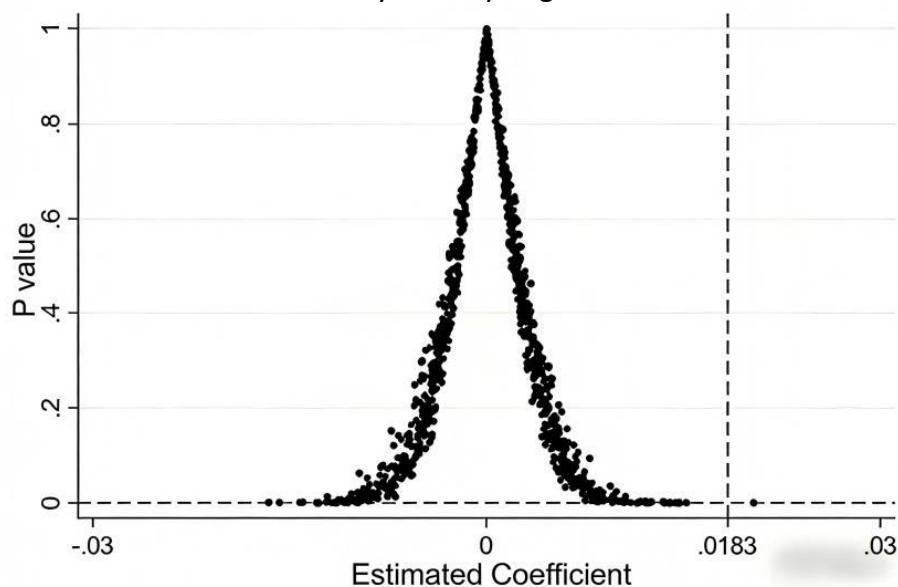


Figure 2 Placebo test: distribution of estimated coefficients from 1,000 random assignments

Propensity score matching

Pilot firms are not a random draw. Before matching they are larger, more often state-owned and more cash-rich than non-pilot firms (Table 6). To reduce this selection on observables, each pilot firm is matched with up to three non-pilot firms by nearest-neighbour propensity score matching with a caliper of 0.05, using all control variables as covariates (Caliendo & Kopeinig, 2008; Rosenbaum & Rubin, 1983).

Matching removes the observable differences. The standardised bias in firm size falls from 64.0 per cent to -2.2 per cent, the bias of every covariate is below 7 per cent after matching, and no difference in means remains significant.

Table 6

Covariate balance before and after matching

Variable	Bias before (%)	p-value before	Bias after (%)	p-value after	Bias reduction (%)
Size	64.0	0.000	-2.2	0.641	96.5
Age	12.5	0.000	5.5	0.261	56.1
Owner	34.2	0.000	3.1	0.515	90.9
Ocf	17.6	0.000	-2.7	0.589	84.5
ER	15.8	0.000	-2.7	0.564	82.9

Variable	Bias before (%)	p-value before	Bias after (%)	p-value after	Bias reduction (%)
Sales	4.0	0.283	0.8	0.868	79.9
Largest	21.9	0.000	-6.8	0.163	69.0
Idr	7.7	0.024	-3.4	0.518	55.8

Table 7 re-estimates the model on the matched sample of 2,646 observations. The DID coefficient ranges from 0.0170 to 0.0204 and stays significant at the 1 per cent level. In the full specification it is 0.0179, almost identical to the baseline estimate of 0.0183. Selection on observable characteristics does not explain the result.

Table 7

Estimates on the propensity-score-matched sample

	(1) ROA	(2) ROA	(3) ROA	(4) ROA
DID	0.0179*** (4.52)	0.0170*** (4.88)	0.0204*** (3.04)	0.0179*** (2.92)
Controls	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Province time trends	No	No	Yes	Yes
Observations	2,646	2,646	2,646	2,646
R ²	0.6216	0.6906	0.6747	0.7308

Note. t-statistics based on robust standard errors in parentheses. *** denotes significance at the 1 per cent level.

Heterogeneity and Mechanisms

Firm size

To test H2, firms are classified as large or small according to whether their total assets exceed the sample average, and the full specification is estimated for each group with Size omitted from the controls.

Table 8 shows a clear asymmetry. For large firms the DID coefficient on ROA is 0.0206 and significant at the 1 per cent level. For small firms it is 0.0067, about one third as large, and statistically insignificant. The ROE results point the same way: 0.0289 and significant at the 10 per cent level for large firms, 0.0152 and insignificant for small firms. The profitability gain from carbon trading accrues to large firms, and H2 is supported.

Several features of large firms can account for this. Fixed compliance costs weigh less on a larger output. Larger firms have more internal funds and technical staff to invest in abatement early. They also have better access to credit, which matters given the role of financing constraints shown below. The result extends to profitability the size asymmetry that Liu and Liu (2023) report for green innovation and Zhao et al. (2024) report for investment.

Table 8

Heterogeneity by firm size

	(1) Small firms, ROA	(2) Large firms, ROA	(3) Small firms, ROE	(4) Large firms, ROE
DID	0.0067 (0.69)	0.0206*** (2.79)	0.0152 (0.78)	0.0289* (1.80)
Controls	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

	(1) Small firms, ROA	(2) Large firms, ROA	(3) Small firms, ROE	(4) Large firms, ROE
Province time trends	Yes	Yes	Yes	Yes
Observations	3,545	3,547	3,545	3,547
R ²	0.5959	0.7282	0.5323	0.6223

Note. t-statistics based on robust standard errors in parentheses. Controls exclude Size. The two subsamples together contain 7,092 observations, fewer than the 9,134 in the full sample. *** and * denote significance at the 1 and 10 per cent levels.

Technological innovation

Panel A of Table 9 tests H3. The scheme raises innovation: the DID coefficient in the Patent equation is 0.2649 and significant at the 1 per cent level, which corresponds to a green patent stock roughly 30 per cent larger for pilot firms. Innovation in turn is positively related to profitability. When Patent is added to the ROA equation, its coefficient is 0.0012 and significant at the 5 per cent level, and the DID coefficient falls slightly from 0.0183 to 0.0180. The pattern is the same for ROE.

The channel exists, but it is narrow. The indirect effect is $0.2649 \times 0.0012 = 0.0003$, which is about 1.7 per cent of the total effect. A Sobel test computed from the reported estimates gives $z = 1.71$ ($p = 0.087$), so the indirect effect is significant only at the 10 per cent level. H3 receives weak support.

Financing constraints

Panel B tests H4. The scheme lowers the KZ index: the DID coefficient is -0.2326 and significant at the 5 per cent level, so financing constraints eased for pilot firms relative to non-pilot firms. Tighter constraints are strongly associated with lower profitability. The KZ coefficient in the ROA equation is -0.0081 and significant at the 1 per cent level, and the DID coefficient falls from 0.0183 to 0.0164.

The indirect effect is $(-0.2326) \times (-0.0081) = 0.0019$, or about 10.3 per cent of the total effect. The Sobel statistic is $z = 2.19$ ($p = 0.029$). The ROE results are similar. H4 is supported.

Two points follow from the comparison. First, the financing channel carries roughly six times as much of the effect as the innovation channel. Within this sample, the pilots improved profitability more by improving firms' financial position than by raising patent output. Second, the two channels together account for only about 12 per cent of the total effect. Most of the gain runs through routes not measured here, such as energy savings, process improvements that are never patented, and revenue from surplus allowances.

Table 9

Mechanism tests

	(1) Mediator	(2) ROA	(3) ROE
Panel A: Mediator = Patent			
DID	0.2649*** (3.40)	0.0180*** (5.14)	0.0281*** (3.81)
Patent		0.0012** (1.98)	0.0023* (1.75)
R ²	0.8097	0.6171	0.5370
Panel B: Mediator = KZ			
DID	-0.2326** (-2.23)	0.0164*** (4.87)	0.0266*** (3.66)
KZ		-0.0081*** (-11.94)	-0.0090*** (-6.49)
R ²	0.7499	0.6381	0.5442
Controls, firm and year fixed effects, province time trends	Yes	Yes	Yes
Observations	9,134	9,134	9,134

Note. Column (1) reports Equation (3) with the mediator as the dependent variable; columns (2) and (3) report Equation (4). t-statistics based on robust standard errors in parentheses. ***, ** and * denote significance at the 1, 5 and 10 per cent levels.

Discussion

The results support the strong version of the Porter hypothesis for China's pilot carbon markets, with two qualifications about timing and channel.

On timing, the gain appears only from the fourth year of the scheme. This helps to reconcile the conflicting findings in earlier work. An estimate that draws mainly on the early years captures compliance costs without the later offsets, which may be one reason why some studies find negative or insignificant effects (He et al., 2022; Liu et al., 2021) while others find positive ones (Chu et al., 2024). The industry-level reversal after two to four years reported by Zhang and Liu (2019) matches the firm-level pattern found here. Evaluations of carbon markets that rely on the first few years of data are therefore likely to understate the benefits to regulated firms.

On channel, innovation matters less than the Porter hypothesis would suggest, and finance matters more. The pilots raised patenting substantially, in line with Cui et al. (2018) and Zhu et al. (2019), but patents explain under 2 per cent of the profitability gain. One reading is that patents are a slow and noisy route to earnings, and that their payoff lies beyond the sample period. Another is that most of the efficiency improvement under the scheme came from operational changes that are never patented.

The financing result needs careful interpretation because it differs from studies that find higher debt costs for pilot firms (Huang et al., 2024; Ni et al., 2022). The difference may lie in what is measured. Those studies examine the price and availability of external debt. The KZ index is a composite of cash flow, cash holdings, dividends, leverage and market valuation, so a lower KZ value may reflect a stronger internal financial position as much as easier borrowing. Lenders may charge pilot firms more while those firms nevertheless become less

dependent on external funds. The finding here agrees with He and Zhang (2024), and separating the internal and external components is a task for future work.

The size result adds a distributional dimension. A policy that raises the profitability of large firms and leaves small firms unaffected will widen the gap between them. As the national market extends to more sectors and smaller emitters, this asymmetry becomes more important.

Conclusion

This study used China's pilot carbon emissions trading scheme as a quasi-natural experiment to estimate the effect of carbon trading on corporate financial performance. Four conclusions emerge from a panel of 9,134 firm-year observations for 2009–2022.

First, the scheme raised the return on assets of pilot firms by 1.83 percentage points and their return on equity by 2.87 percentage points. The result holds under parallel-trend, placebo and propensity-score-matching checks. Second, the gain was delayed: it was insignificant from 2013 to 2015 and significant from 2016 onward. Third, the gain was confined to large firms. Fourth, eased financing constraints transmitted about one tenth of the effect and technological innovation less than one fiftieth, which leaves most of the effect to channels not examined here.

Policy implications

- **Judge carbon markets over a sufficiently long horizon.** Firms needed about three years to convert compliance into profit. Regulators extending the national market to new sectors should expect a similar adjustment period and should avoid weakening the scheme in response to early cost complaints.
- **Give small firms transitional support.** Small firms bear the compliance burden without the profitability gain. Simplified monitoring and reporting procedures, shared verification services and technical assistance for abatement would lower their fixed costs.
- **Link carbon markets with green finance.** Because the financing channel is the stronger of the two, measures that let firms use allowances as collateral and that feed verified emissions data into bank credit assessment are likely to amplify the benefits of trading.
- **Do not rely on patent counts as the measure of success.** Patenting rose, but it explains little of the financial gain. Support for process upgrading and energy management may yield more than support aimed only at formal research output.

Limitations and future research

Several limitations should be noted. The sample contains listed companies only, so the results may not hold for unlisted and very small firms. The design assigns a common policy date, although the pilot markets opened at different times in 2013 and 2014; future work should apply estimators designed for staggered adoption (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021). Year-specific estimates are reported only to 2020. The paper therefore does not show how the effect evolved in 2021 and 2022, when the national market began to cover power generators in non-pilot provinces as well. Other environmental and financial policies introduced during the period, such as green credit guidelines and central environmental inspections, cannot be fully separated from the scheme, although province-specific trends absorb part of their influence.

The mechanism tests are also limited. The stepwise approach identifies associations consistent with mediation but does not establish that the mediators are exogenous, and the KZ index contains cash-flow components that are related to profitability by construction (Hadlock & Pierce, 2010). The size comparison rests on separate subsample regressions that together cover 7,092 of the 9,134 observations, and no formal test of the difference between the coefficients is reported, so the size result should be read with caution. Finally, about 88 per cent of the effect remains unexplained by the two channels. Firm-level data on energy use, abatement investment and allowance transactions would allow future studies to examine those remaining routes.

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