

Market-Based Environmental Regulation and Corporate Financial Performance: An Extension Study of China's Emissions Trading Scheme Pilots

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Abstract. This study re-examines the effect of China's emissions trading scheme (ETS) pilots on corporate financial performance and is positioned as an extension of the recent quasi-experimental evidence on this question. Using an extended panel of Chinese A-share listed firms from 2010 to 2024 and a difference-in-differences (DID) design, the study identifies a significant positive effect of the ETS pilots on return on assets (ROA). The result remains significant when Tobin's Q replaces ROA, when industry fixed effects are added, and when the COVID-19 years are excluded. Compared to previous research, this study makes three contributions: first, it broadens the study window to a full post-policy decade; second, it investigates the financing-constraint channel; and third, it maps where the policy effect stops operating—the boundary conditions. The positive effect is driven by private and high-innovation firms; it is insignificant for state-owned and foreign firms and turns significantly negative for low-innovation firms. Mechanism analysis indicates that the ETS alleviates financing constraints, thereby supporting firms' investment capacity, operating efficiency, and profitability. The findings suggest that market-based environmental regulation can generate both environmental and economic benefits, provided that complementary financing and innovation support reach firms facing stronger adjustment constraints.

Keywords: emissions trading scheme, corporate financial performance, difference-in-differences, financing constraints, innovation capacity

1. Introduction

Under mounting pressure to address climate change, emissions trading schemes (ETS) have become one of the most widely adopted market-based environmental regulatory instruments, designed to reduce carbon emissions while improving the efficiency of resource allocation [1, 2]. China, the world's largest carbon emitter, launched regional ETS pilot programmes in Beijing, Tianjin, Shanghai, Chongqing, Hubei, and Guangdong beginning in 2013, with the explicit aims of regulating corporate carbon emissions through market mechanisms and laying the institutional foundations for a national carbon market [1]. In this institutional setting, the ETS policy may affect

not only firms' environmental behaviour but also their financial performance, by reshaping production costs, investment decisions, and operational strategies [3, 4].

Whether carbon trading helps or hurts corporate financial performance remains an open empirical question. Existing studies have concentrated largely on the emission-reduction effects and environmental outcomes of the ETS, and the evidence on financial performance is inconclusive [2, 5, 6]. One line of argument holds that carbon trading raises compliance costs and imposes operational constraints, thereby eroding profitability [6, 7]. A competing view, rooted in the Porter hypothesis, holds that well-designed environmental regulation can encourage technological innovation and improve resource allocation efficiency, enhancing long-run financial performance [2, 8]. Moreover, most firm-level DID evaluations of the Chinese pilots rely on sample windows ending around 2017–2019. Because the financial benefits of environmental regulation are widely believed to materialise only in the medium to long run, these shorter windows may fail to capture the full performance consequences of the policy. Three questions therefore remain insufficiently answered: what the longer-run effect of the pilots on financial performance is, what the channel through which any effect operates is, and what the types of firms for which the policy works or backfires are.

To address these questions, this study is designed as an extension of the recent firm-level evidence of Chu et al. [5]. It re-estimates the effect of China's ETS pilot policy on corporate financial performance using an extended sample of A-share listed firms covering 2010 to 2024, and then goes beyond the benchmark specification in three directions: by testing the financing-constraint channel as a plausible mechanism, by examining heterogeneity across ownership structures and innovation capacity, and by assessing robustness over an observation window that spans a full decade of post-policy outcomes. Following the regional pilot design, firms registered in Beijing, Tianjin, Shanghai, Chongqing, Hubei, and Guangdong are defined as the treatment group, firms in other regions serve as the control group, and the post-policy period begins in 2013. The interaction between treatment and post-policy indicators serves as the key explanatory variable in a difference-in-differences framework with firm and year fixed effects.

The results indicate that the ETS pilots significantly improve corporate financial performance. The DID coefficient on ROA has increased from 0.0544 (without fixed effects) to 0.2514 (with firm and year fixed effects), significant at the 1% level across all specifications. The finding survives three robustness checks: replacing ROA with Tobin's Q (coefficient 0.3535, significant at 5%), adding industry fixed effects (0.2522, 1%), and excluding the pandemic years 2020–2022 (0.3252, 1%). Heterogeneity analysis shows that the positive effect is driven by private firms (0.6639) and high-innovation firms (0.2639); it is statistically insignificant for state-owned enterprises (−0.0025) and foreign firms (0.0019), and significantly negative for low-innovation firms (−0.0471). Mechanism analysis suggests that the policy works in part by alleviating financing constraints (0.0143, significant at 5%).

As an extension study, the paper contributes to the literature in three ways. First, it broadens the evidence window: by extending the follow-up to 2024, it demonstrates that the positive impact on financial performance found in the shorter periods still holds and is strengthened over 10 years, which is consistent with the long-term innovation-mediated explanation of the Porter hypothesis rather than a short-lived compliance cost explanation. Secondly, it expands the mechanism research: to the best of our knowledge, this is one of the first studies that demonstrates the impact of the financing-constraint mechanism and shows that taking part in carbon trading improves external financial conditions for firms, enabling them to increase investments and profitability [9, 10]. Finally, it expands the understanding of boundary conditions, because the findings regarding the

negative effect of the policy on low-innovation firms imply requirements for complementary support measures that would need to be implemented while launching China's national carbon market.

Section 2 reviews the literature and develops the hypotheses; Section 3 details the data, variables and model; Section 4 reports the results; Section 5 discusses the implications; Section 6 concludes.

2. Literature review and hypothesis development

2.1. The ETS and corporate financial performance

The financial implications of emissions trading are not clear-cut from a theoretical point of view; existing empirical studies reveal this uncertainty. In terms of costs, introducing a carbon pricing mechanism enables the internalization of an externality that previously affected society in general. Companies operating under regulations are required to monitor emissions, verify them, and surrender allowances, while in many cases they are obliged to purchase additional quotas on the market. All these activities raise marginal compliance and administrative costs and can restrict output decisions on the part of companies, a pattern that is particularly evident for energy-intensive companies and can negatively influence profitability [6, 7]. On the benefits side, the Porter hypothesis states that effective environmental regulation generates innovation offsets: firms react to carbon pricing by upgrading technology, enhancing energy efficiency, and diverting resources to value-enhancing endeavours, enabling them to profit from innovation [1, 2, 8]. Firm-level evidence is mixed [2, 5, 6], and most quasi-experimental samples end well before the 2021 national market launch, leaving the long-run balance unresolved. Since the compliance costs of emissions trading systems are front-loaded while the innovation offsets take time to accumulate, we argue that the possible effect may become clearer over a more extended period of observation. Because of this reasoning, we present our first hypothesis now:

Hypothesis 1 (H1). ETS pilot participation raises the financial performance of firms in pilot regions.

2.2. Ownership heterogeneity

Both the cost channel and the innovation channel are likely to be influenced by ownership structures. Typically, state-owned entities have more favorable access to government services, cheap loans, and implicit assurances, which, in turn, protect them from market shocks and diminish their sensitivity to carbon pricing. However, at the same time, their various non-profit objectives unrelated to profits may reduce their response to the incentives [11]. On the other hand, privately-owned companies are subjected to tighter financial constraints and are influenced by market conditions significantly more; thus, price changes affecting them affect their investment decisions more sharply, and their managers have stronger incentives to capture the innovation offsets. Foreign enterprises present a different situation because they already operate under stricter environmental regulations imposed in their home countries and possess advanced environmental technologies and practices. Hence, it is logical to argue that the Chinese pilots may not provide them with any considerable benefits.

We therefore hypothesise:

Hypothesis 2a. The policy's positive effect is strongest among private firms.

Hypothesis 2b. For state-owned and foreign firms, the effect is statistically indistinguishable from zero.

2.3. Innovation capacity heterogeneity

Thus, if the positive effect of the ETS is a result of technological upgrading (according to the Porter hypothesis [8]), this effect will depend on a firm's level of innovation capability. Highly innovative firms are able to translate carbon pricing pressure into process improvements, green products, and new sources of revenue, thus being able to obtain innovation offsets. By contrast, companies with low innovation abilities do not have such an escape route: for them, the ETS remains a serious compliance and adjustment burden, and the associated costs may not be recoverable, plausibly depressing their financial performance.

Hypothesis 3a. The positive effect is more pronounced among firms with high innovation capacity.

Hypothesis 3b. For low-innovation firms, the policy effect is weak or negative.

2.4. The financing-constraint channel

The connection between ETS and financial performance can be established via another path—the capital market. To run an ETS, companies must disclose their emissions, which eases the information asymmetry facing outside investors. A carbon allowance can be considered a tradeable instrument and can help a company enhance its financial position. Companies that transact on carbon markets receive a reliable green signal, making it easier for them to obtain bank loans, green bonds, and sustainable finance. Constrained firms underinvest in capital and R&D [9, 10], so easing these constraints should feed through to performance. This idea leads to our fourth hypothesis:

Hypothesis 4 (H4). The ETS pilot policy alleviates firms' financing constraints, and this alleviation is an important channel through which the policy improves financial performance.

3. Research design

3.1. Sample and data

Our sample combines CSMAR and Wind data for A-share firms over the 2010–2024 period. The pilot rollout in 2013–2014 [12, 13] splits the sample geographically: firms from the six pilot jurisdictions—Beijing, Tianjin, Shanghai, Chongqing, Hubei, and Guangdong—form the treatment group, whereas firms located elsewhere serve as the control group. The typical screenings are done: we exclude special treatment (*ST) or loss-listed (PT) firms, drop financial-sector firms, and discard the firm-years with missing values for key variables. These screens leave 14,605 firm-year observations; the regression sample after absorbing firm and year fixed effects (and dropping singletons) comprises 13,881 observations spanning 1,820 firms.

One feature of the sample deserves explicit comment. The concentration of Chinese listed corporations is great in the economically advanced coastal regions and central pilot provinces - alone, Beijing, Shanghai and Guangdong make up a considerable number of total A-share listings - thus treated firms constitute 82.11% of firm-year observations in our study sample (see Table 2). The control group is therefore smaller than that in most traditional DID approaches; this lack of balance is acknowledged as a limitation in Section 6, but it does not undermine the design, because identification rests on within-firm changes over time, though caution is warranted when extrapolating to all Chinese firms.

3.2. Variable definitions

Table 1 summarises the definitions of all variables used in the analysis.

Table 1. Variable definitions

Variable type	Variable name	Definition
Dependent variable	ROA	Net profit scaled by total assets
	Tobin's Q	Market value over (total assets minus total liabilities); the robustness-check performance measure
Independent variable	Treat	One for firms headquartered in any of the six pilot jurisdictions (Beijing, Tianjin, Shanghai, Chongqing, Hubei, Guangdong); zero otherwise
	Post	One from 2013 onwards; zero before
	DID	Product Treat·Post
	Size	Natural logarithm of total assets
Control variables	Mbratio	Book-to-market ratio
	Tagr	Annual expansion of total assets
	Leverage	Liabilities as a share of assets
	Npgr	Year-on-year profit growth
	Indep	Independent directors as a share of the board
	Top1	Largest shareholder's equity stake
	Top10_HHI	HHI computed over the top ten shareholders' stakes
	Age	Log of years since founding
Mechanism variable	SA_index	Financing-constraint proxy [14], sign-flipped so that larger values mean easier access to finance: $SA_index = -1 \times (-0.737 \times Size + 0.043 \times Size^2 - 0.040 \times Age)$
Grouping variables	Ownership	Ultimate-controller records in CSMAR sort firms into SOE, private, and foreign categories
	Innovation	R&D spending over operating revenue above the annual industry median marks high-innovation firms; the rest are low-innovation

3.3. Descriptive statistics

Table 2 summarises the data. First, ROA averages 0.0477—a net return of about 4.8% on assets, while the large standard deviation (0.5965) and the wide range between −48.32 and 108.37 reflect how widely profitability varies across the A-share market; the extreme tails are driven by a small number of distressed or restructured firms. Second, Tobin's Q averages 2.26 but reaches 1,752.7 at the top end, indicating a strongly right-skewed valuation distribution. Third, the mean leverage ratio of 0.43 is in line with other A-share samples, and the mean value of Top1 (34.20%) together with a Top10_HHI mean of 0.43 confirms that ownership remains concentrated, although the average proportion of independent directors (37.65%) sits just above the one-third threshold required by the China Securities Regulatory Commission. Finally, the mean of Treat (0.8211) and of Post (0.8665) confirms that most observations belong to pilot-region firms and to the post-2013 period, so that the DID variation is generated mainly by the interaction of these two dimensions rather than by either one alone.

Table 2. Descriptive statistics

Variable	Mean	Std. dev.	Min	Max
ROA	0.0477	0.5965	−48.3159	108.3657
Tobin's Q	2.2649	12.1551	0.6113	1752.7050
Treat	0.8211	0.3833	0	1
Post	0.8665	0.3401	0	1
DID	0.6640	0.4724	0	1
Size	22.0790	1.4217	13.0760	29.8291
Mratio	0.6245	0.2508	0.0006	1.6360
Tagr	0.2136	0.8213	−1	72.5214
Leverage	0.4342	0.9128	−0.1947	178.3455
Npgr	−0.5216	276.8868	−19115.20	45174.36
Top1	34.1952	15.2327	0.29	100
Top10_HHI	0.4254	0.1979	0.1010	0.9901
Indep	0.3765	0.0559	0	1
Age	2.0914	0.9685	0	3.5264

3.4. Model specification

We estimate a difference-in-differences (DID) model along the lines of Chu et al. [5], with ROA as the outcome:

$$ROA_{it} = \lambda_0 + \lambda_1 DID_{it} + \gamma X_{it} + \mu_i + \eta_t + \varepsilon_{it} \quad (1)$$

Here the subscripts i and t index firms and years. λ_1 is the parameter of interest—the DID estimate of the policy effect; X_{it} gathers the controls from Table 1; μ_i sweeps out everything stable about a firm over the sample (management, location, industry heritage); η_t sweeps out economy-wide shocks in year t ; and ε_{it} is the residual. Inference relies on firm-clustered standard errors throughout. Firm and year fixed effects are included in the preferred specification, with intermediate specifications reported for transparency.

4. Empirical results

4.1. Benchmark regression results

Table 3 reports the benchmark regression results. Column (1) includes no fixed effects, Column (2) controls for firm fixed effects, and Column (3)—our preferred specification—controls for both firm and year fixed effects. The coefficient of DID rises from 0.0544 in Column (1) to 0.0984 in Column (2) and to 0.2514 in Column (3), and is significantly positive at the 1% level in all three specifications. This pattern is informative in its own right: the increase between Columns (1) and (2) shows that between-firm comparisons understate the policy effect, because pilot-region firms differ systematically from non-pilot firms in ways that mask the true impact; once each firm is compared with its own history, the estimated effect more than doubles. The transition from Column (2) to Column (3) helps to separate the impact of macroeconomic shocks from other factors, which results

in an estimation that is based solely on data from within a firm for a particular year. The coefficient of 0.2514 indicates a significant improvement in ROA due to the ETS participation, which is in excess of the average ROA value that is equal to 0.0477 and affirms the position that the results are relevant for underperforming firms, which have a larger pool of potential improvements to realize than firms from other performance categories.

The behavior of the control variables is essentially as predicted. Size has a consistent significant negative relationship with ROA at the 1% significance level for all specifications, which aligns with the theory of diseconomies of scale and the established fact that larger Chinese firms operate in mature, lower-margin industries. Leverage also shows a significant negative relation at the 1% level, reflecting the financial risk of high indebtedness. Age has a significant positive coefficient at the 1% level, suggesting that older companies with a body of experience and reputation do better financially. Npgr shows a positive coefficient at the 10% level, while Top1, Top10_HHI, Indep, Mbratio, and Tagr are not statistically significant, echoing the weak governance–performance links documented for Chinese listed firms [11].

Table 3. Benchmark regression results (dependent variable: ROA)

Variable	(1) No FE	(2) Firm FE	(3) Firm & Year FE
DID	0.0544*** (0.0185)	0.0984*** (0.0204)	0.2514*** (0.0542)
Size	−0.1152*** (0.0129)	−0.1916*** (0.0169)	−0.1838*** (0.0181)
Leverage	−0.2423*** (0.0486)	−0.3015*** (0.0534)	−0.3089*** (0.0540)
Npgr	0.0000934* (0.0000496)	0.0000920* (0.0000498)	0.0000911* (0.0000498)
Age	0.0740*** (0.0157)	0.1006*** (0.0193)	0.0951*** (0.0242)
Top1	0.0015 (0.0013)	−0.0001 (0.0015)	−0.0001 (0.0015)
Top10_HHI	−0.1402 (0.0896)	−0.1303 (0.0951)	−0.1257 (0.0964)
Indep	0.0741 (0.1604)	0.0366 (0.1730)	0.0494 (0.1737)
Mbratio	−0.0320 (0.0414)	−0.0098 (0.0440)	−0.0600 (0.0528)
Tagr	0.0055 (0.0111)	0.0153 (0.0114)	0.0152 (0.0115)
Constant	2.5398*** (0.2615)	4.2196*** (0.3500)	3.9551*** (0.3919)
Firm FE	No	Yes	Yes
Year FE	No	No	Yes
R ²	0.0027	0.5033	0.5041

Table 3. (continued)

Number of observations	14,605	13,881	13,881
Number of firms	2,544	1,820	1,820

Notes: firm-clustered standard errors in parentheses. Stars: *10%, **5%, ***1%.

4.2. Robustness tests

4.2.1. Parallel trend test

DID identification requires that, had the pilots not occurred, treated and control firms would have moved in tandem. Figure 1 tests this directly. The six lead indicators (before_6 through before_1) produce small point estimates hovering around zero, and none is statistically distinguishable from zero at conventional levels—each 95% interval comfortably spans the origin. Nor is there any drift in the leads that would hint at diverging pre-trends. We therefore treat the common-trends requirement as met and read the baseline estimates as causal.

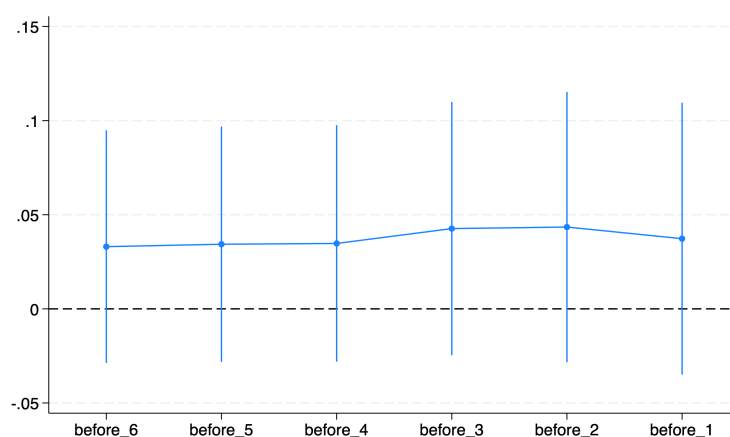


Figure 1. Parallel trends test: event-study estimates

Note. Event-study estimates of pre-treatment leads (before_6 to before_1) with 95% confidence intervals. The omitted category is the treatment year. All confidence intervals include zero, supporting the parallel-trends assumption.

4.2.2. Additional robustness tests

Three further robustness tests are reported in Table 4. First, we replace ROA with Tobin's Q, a market-based gauge of performance expectations [15]. The DID coefficient in Column (1) is 0.3535 and significantly positive at the 5% level, indicating that the market value channel corroborates the accounting channel: investors also revise their valuations of pilot-region firms upward following the policy. Second, we add industry fixed effects in Column (2) to absorb shocks and policy effects that are common to firms in the same industry; the 0.2522 estimate (1% significance) rules out an industry-composition artefact. Third, we drop 2020-2022, since pandemic-era credit and output disruptions could contaminate the financing channel; the 0.3252 estimate in Column (3) rules out a pandemic-era artefact. Across all three checks the DID estimate stays positive and significant, so the benchmark result does not hinge on the performance measure, industry composition, or the pandemic window.

Table 4. Robustness tests

Variable	(1) Tobin's Q	(2) Industry FE	(3) Excluding 2020–2022
DID	0.3535** (0.1770)	0.2522*** (0.0546)	0.3252*** (0.0772)
Constant	30.9554*** (1.2797)	3.6722*** (0.4152)	5.7097*** (0.5689)
Controls	Yes	Yes	Yes
Industry FE	No	Yes	No
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
R ²	0.5126	0.5278	0.5078
Observations	13,881	13,804	9,395

Notes: firm-clustered standard errors in parentheses. Stars: *10%, **5%, ***1%.

4.3. Heterogeneity analysis

4.3.1. Ownership heterogeneity

Table 5 reports whether the policy effect varies with ownership structure; the grouping follows the ultimate-controller classification described in Table 1. For SOEs (Column 1), the DID coefficient is -0.0025 and statistically insignificant: the ETS pilots have no detectable effect on the financial performance of state-owned firms. This is consistent with Hypothesis 2b and plausibly reflects the softer budget constraints and preferential financing access of SOEs, which blunt both the cost pressure and the market incentives created by carbon pricing. For private firms (Column 2), the 0.6639 estimate (1% significance) supports Hypothesis 2a; the magnitude is more than twice the full-sample estimate, confirming that private firms are the main beneficiaries of the policy. We note that the within R^2 of this subsample regression is low (0.0087), which indicates that most of the variation in private firms' ROA is idiosyncratic or absorbed by the fixed effects; we read this column for the sign and significance of DID, not for its fit. For foreign firms (Column 3), the coefficient is 0.0019 and insignificant, also consistent with Hypothesis 2b; because the foreign-firm subsample contains only 446 observations, however, the null here likely reflects the small sample rather than a true zero.

4.3.2. Innovation capacity heterogeneity

Columns (4) and (5) of Table 5 split the sample by innovation capacity, defined relative to the annual industry median of R&D intensity (Table 1). For high-innovation firms, DID is 0.2639 (1% significance), supporting Hypothesis 3a: firms with the capacity to respond to carbon pricing through technological upgrading and green innovation obtain the largest benefits, exactly as the innovation-offset logic predicts [8]. For low-innovation firms, the coefficient is -0.0471 and significantly negative at the 10% level, supporting Hypothesis 3b: for these firms, the ETS operates mainly as a compliance and adjustment cost that they cannot offset through innovation, and their financial performance suffers accordingly. The contrast between the two subsamples—beneficiaries

versus victims of the same policy—is one of the most policy-relevant findings of this study, and it is summarised in the discussion below.

Table 5. Heterogeneity analysis

Variable	(1) SOE	(2) Private	(3) Foreign	(4) High innovation	(5) Low innovation
DID	−0.0025 (0.0035)	0.6639*** (0.1604)	0.0019 (0.0205)	0.2639*** (0.0605)	−0.0471* (0.0243)
Controls	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	3,879	5,384	446	12,018	1,488
R ²	0.2275	0.0087	0.1653	0.5055	0.7102

Notes: firm-clustered standard errors in parentheses. Stars: *10%, **5%, ***1%.

4.4. Mechanism analysis

Finally, we examine the financing-constraint channel proposed in Hypothesis 4. The modified index SA is the variable analyzed in Table 1. A higher index value means weaker constraints. The result of DID analysis in Table 6 is 0.0143 (significant at 5%)—participation in the pilots visibly relaxes firms' financing constraints. From an economic perspective, this finding makes sense. Carbon trading requires verified disclosure of emissions, which closes the information gap between firms and financiers; allowances are effective marketable assets, and participation itself serves as a credible green signal that helps firms obtain bank credit. This is important because financially constrained firms do not invest enough in physical capital and R&D [9, 10]. The SA-index result therefore links carbon trading, investment capacity, and ROA.

One procedural detail requires explanation: the mechanism regression is estimated on the pre-pandemic subsample of 9,395 firm-year observations, mirroring Column (3) of Table 4. This point is important because the impact of the pandemic should be excluded from the analysis since it brings a large number of complications. The very high R² (0.9643) is unsurprising: the SA index is a mechanical function of a firm's own size and age, which the firm fixed effects absorb.

Table 6. Mechanism analysis: the financing-constraint channel (dependent variable: SA_index)

Variable	(1) SA_index
DID	0.0143** (0.0059)
Constant	−3.1126*** (0.0432)
Firm FE	Yes
Year FE	Yes
Controls	Yes
Observations	9,395
R ²	0.9643

Notes: firm-clustered standard errors in parentheses. Stars: *10%, **5%, ***1%.

5. Discussion

We see three contributions. To begin with, we move the balance of proof towards the innovation-offset interpretation of the Porter hypothesis. Earlier firm-level studies reported mixed results, but most used short post-policy windows [2, 5, 6]. Our analysis of a longer time period supports the view that compliance costs arrive early while the financial benefits accumulate over time [8-10]. Second, the mechanism evidence points to a capital-market pathway alongside the technology channel in the literature [8-10]. Third, the heterogeneity analysis shows that the effect is a distribution, not a single number: positive for private and innovative firms, absent for SOEs and foreign firms, negative for low-innovation firms.

These results have practical policy implications. The unfavorable impact on low-innovation companies suggests that a universal approach should not be used: for companies that are unable to innovate, carbon pricing is purely an expense, and their decline is neither economically efficient nor feasible politically. As China consolidates its national carbon market, policymakers may need to combine allowance allocation with targeted assistance for this group (namely transition finance, technology adoption subsidies, and capacity-building programmes) to allow expansion of the innovation pathway as opposed to pure cost bearing. Conversely, the neutrality of the effect for SOEs suggests that their preferential access to state support cushions them from market signals; gradually hardening their budget constraints would allow the ETS to bite where it should. For private and high-innovation firms, our results imply that carbon-market participation is already paying a financial dividend, which strengthens the case for deepening market liquidity and expanding industry coverage. Theoretically, the findings support the view that market-based environmental regulation, unlike command-and-control instruments, can reconcile environmental and economic objectives—but only for firms equipped to respond to price signals.

6. Conclusion

This study has examined the impact of China's emissions trading scheme on corporate financial performance, deliberately positioning itself as an extension of the recent quasi-experimental literature. Using panel data on Chinese A-share listed firms from 2010 to 2024 and a difference-in-differences approach, we find that the pilots raise corporate financial performance significantly, and that this finding is robust to replacing ROA with Tobin's Q, adding industry fixed effects, and excluding the COVID-19 period. Extending beyond the benchmark result, we show that the positive effect is concentrated among private firms and high-innovation firms, is insignificant for state-owned and foreign firms, and turns significantly negative for low-innovation firms; and we provide evidence that the policy operates in part by alleviating financing constraints, thereby enhancing firms' investment capacity, operating efficiency, and profitability.

Several limitations qualify these conclusions and point to future research. First, treated firms account for 82% of our sample because Chinese listed companies are concentrated in the pilot regions; although within-firm identification remains valid, future work could rebalance the comparison using propensity-score matching or weighting schemes. Second, the pilot regions were not chosen at random, so unobserved regional trends correlated with both pilot selection and financial performance could bias our estimates despite the fixed-effects design. Third, the pilots were launched in a staggered fashion between 2013 and 2014, whereas we apply a common 2013 cutoff; estimators robust to staggered treatment timing would be a valuable extension. Fourth, the SA index is a size-and-age-based proxy and may not capture all dimensions of financing constraints; future studies could complement it with interest-expense-based or survey-based measures. Fifth, our

descriptive statistics reveal extreme tails in several variables, and winsorisation was not applied; replication with tail-adjusted data would test the sensitivity of the magnitudes, if not their signs. Given the consistency across specifications, outcomes, and subsamples, the conclusion is robust: market-based environmental policies can lead to both the environmental and the economic benefits, but the distribution of these benefits is highly contingent upon firm ownership and innovation capabilities.

For the policymakers, there are two key takeaways. First, the evidence is in favour of continued expansion of China's carbon market, whose financial dividends are already apparent a decade after its launch. Second, the fact that low-innovation companies bear the burden of this transition serves as an argument in favour of ensuring that financing and support for innovations are included in the carbon market framework so that the responsibilities associated with this transition are distributed not only efficiently but also fairly according to the types of firms involved.

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