



Emissions Trading and Carbon Intensity Regulation in China's Steel Sector: Environmental and Financial Outcomes from a Difference-in-Differences Analysis

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Abstract

This study examines how China's carbon intensity reduction policies and emissions trading participation have affected firm-level outcomes in the steel industry from 2010 through 2024. We compile an unbalanced panel of 120 major Chinese steel firms with data on environmental metrics (CO₂ emissions per tonne of steel) and financial performance indicators, profitability, leverage, and investment. Employing a difference-in-differences (DID) framework that separately identifies two distinct policy channels, regional pilot ETS exposure and binding government-mandated carbon intensity targets, we compare treated firms against controls before and after policy implementation. To ensure causal credibility, we conduct formal event-study analyses to verify parallel pre-treatment trends, implement staggered-DID estimators to address heterogeneous treatment timing across the seven pilot jurisdictions, and cluster all standard errors at the firm level. We address endogeneity concerns by controlling for concurrent confounding policies, including output production caps, central environmental inspection campaigns, and supply-side capacity elimination, and by implementing propensity score matching DID (PSM-DID) and sensitivity bounds for unobservable selection. Our results indicate that firms subject to pilot ETS regulation achieved statistically significant additional reductions in emissions intensity, approximately 0.05 tCO₂/t steel, beyond industry-wide improvements, with firm-clustered standard errors confirming significance at the 5% level. Firms under binding intensity targets also reduced emissions, though with smaller and less precisely estimated effects. Importantly, we find no statistically significant evidence that these regulatory pressures harmed economic performance; return on assets rose modestly among pilot-ETS firms by approximately 0.7 percentage points, though this effect is only marginally significant ($p < 0.10$) and

economically small, warranting cautious interpretation. Heterogeneity analyses reveal that state-owned enterprises and firms in eastern provinces gained more from carbon policies. A formal mediation analysis testing the hypothesized transmission channel from low-carbon innovation through efficiency improvements to profitability yields suggestive but statistically inconclusive results: while the indirect effect is directionally consistent, the Sobel test does not reject the null of no mediation at conventional levels. In light of China's dual carbon goals and the 2025 expansion of the national ETS to include steel with the November 2025 release of the 2024-2025 allowance allocation plan, these findings imply that well-designed market-based carbon policies can reduce emissions intensity for steelmakers without necessarily undermining economic performance, though the evidence for net profitability gains remains tentative. Transitioning from intensity benchmarks to absolute caps will be necessary for deeper decarbonization.

Subject Areas

Finance

Keywords

Carbon Intensity, Emissions Trading System (ETS), Corporate Performance, Chinese Steel Manufacturing, Difference-in-Differences, Staggered DID, Parallel Trends, Low-Carbon Innovation, Sustainability

1. Introduction

China's steel industry is both massive and carbon-intensive. At its peak in 2020, Chinese crude steel output reached 1.065 billion tonnes, accounting for over 50% of global production and approximately 15% - 20% of national CO₂ emissions. In response to climate commitments, the Chinese government has imposed strict production and efficiency controls on steel. A mandated zero-growth policy from 2021 onward capped output, contributing to a 1.7% decline in 2024 production to 1.005 billion tonnes, as reported by Qiao and Wang [1]. By 2025, output continued to fall: September 2025 was down 4.6% year-on-year, October down 12.1%, and November down approximately 10.9%, with total 2025 production reaching approximately 961 million tonnes, a seven-year low amid ongoing overcapacity reduction, weak demand from the property sector, and green mandates. Meanwhile, national targets require sharp improvements in carbon intensity: under the 14th Five-Year Plan (2021-2025), CO₂ emissions per unit GDP must drop 18% by 2025. Sector-specific policies are even more stringent: a May 2024 State Council action plan calls for cutting economy-wide energy use and CO₂ intensity by 3.9% in 2024 and directs the steel industry to accelerate the industrial transition toward energy saving and decarbonization, as stated by Wei *et al.* [2].

Concurrent with these intensity targets, China has implemented a carbon emis-

sions trading system (ETS). From 2013-2017, seven regional pilot ETS programs in Beijing, Tianjin, Shanghai, Chongqing, Hubei, Guangdong, and Shenzhen tested carbon pricing, and a national ETS launched in 2021 covering the power generation sector, as Chai *et al.* [3] notes. Critically, steel firms were not formally covered by the national ETS until its expansion in 2025 to include steel alongside cement and aluminum, as reported by Lu *et al.* [4], with allowances initially set to match verified 2024 emissions. Even before national coverage, some steelmakers in pilot ETS jurisdictions faced direct or indirect carbon costs, for example, via allowance obligations or increased electricity prices from regulated power generators. China's current intensity-based ETS design allocates free allowances tied to production levels, which may limit absolute emissions reductions if production rises. However, recent policy signals for 2024-2025, including the November 2025 allowance allocation plan for new sectors and discussion of moving to absolute caps, indicate an intent to tighten the system.

Given this environment of overlapping carbon-intensity goals and emerging market mechanisms, we ask: How have Chinese steel firms responded at the corporate level to these policies? Has regulatory pressure on CO₂ intensity translated into improved environmental performance without undermining economic competitiveness? To answer these questions, we empirically analyze a panel of 120 Chinese steel manufacturers from 2010 to 2024, assessing the link between two distinct policy channels: pilot ETS exposure and binding intensity targets, and firm-level outcomes including emissions intensity, profitability, investment, and leverage.

Our empirical strategy makes several important methodological choices that distinguish this study from prior work. First, we separately identify the two policy channels rather than bundling them into a single treatment indicator, recognizing that market-based carbon pricing (ETS) and command-and-control intensity mandates operate through different mechanisms and may have different effects. Second, we deliberately do not classify any steel firm as national ETS-treated during 2021-2024, because the national carbon market covered only the power generation sector during this period; any indirect effects on steel firms from the national ETS are captured through an electricity price control variable. Third, we employ formal event-study analyses to verify the parallel trends assumption, implement Callaway and Sant'Anna [5] staggered-DID estimators to address the fact that the seven pilot programs launched at different times, and cluster all standard errors at the firm level to account for serial correlation, as Bertrand *et al.* [6] recommend. Fourth, we address endogeneity by controlling for concurrent policies, output production caps, central environmental inspections, and supply-side capacity elimination, and by implementing propensity score matching and Oster [7] sensitivity bounds.

This research fills a gap in the literature by providing the first quantitative evaluation of Chinese steel firms' performance under the combined but separately identified influence of carbon intensity targets and pilot ETS exposure, drawing

on rich firm-level data from corporate reports and official statistics. Our findings provide evidence on whether China's climate policies can achieve the dual objective of lower carbon intensity and stable or improved corporate performance in a traditionally high-emission industry.

2. Literature Review

2.1. Carbon Policies and Industry Performance

Globally, emissions trading schemes have become a key policy tool for reducing greenhouse gas emissions. The European Union's Emissions Trading System (EU-ETS), launched in 2005, is the world's largest carbon market and has been studied extensively. Research on the EU-ETS finds that carbon pricing can significantly influence company behavior and performance. Carbon market volatility has been shown to spill over into corporate financial dynamics, with statistically significant short-term impacts on stock returns, particularly for high-emission firms, as Alvarez-Díez *et al.* [8] demonstrate. As the EU-ETS moved toward auctioning a greater share of allowances, firms responded by intensifying their search for cost-saving trading opportunities, though these additional efforts have been relatively small, as Lehmann and Schleich [9] find. The EU experience highlights that market-based carbon regulations not only drive emissions reductions but also act as financial signals for firms, affecting their market behavior and risk management.

A growing body of literature evaluates China's carbon control policies in terms of both environmental and economic outcomes. Studies of China's regional ETS pilots (2013-2017) find clear environmental gains. Wen *et al.* [10] report that pilot regions achieved measurable emissions reductions. Cui *et al.* [11] demonstrate a 16.7% reduction in total CO₂ emissions and a 9.7% reduction in carbon intensity among regulated firms relative to controls, with no significant negative impact on firms' financial performance. Analyses of the initial years of China's national ETS (focused on the power sector in 2019-2020) document modest declines in carbon intensity (about 2.5% - 3.1%) even as output grew slightly, achieved mainly through efficiency upgrades and fuel switching, with high compliance rates reported by Yan *et al.* [12]. In sum, China's carbon markets so far appear effective in lowering emissions intensity without undermining corporate viability, as Feng [13] confirm.

Beyond direct environmental outcomes, market-based regulation may stimulate innovation and productivity, a dynamic consistent with the Porter Hypothesis. Jiang *et al.* [14] find that companies participating in pilot ETS programs improved their carbon performance significantly, primarily through increased low-carbon innovation outputs, and that participating firms realized economic benefits within three years after ETS implementation. Zhang *et al.* [15] similarly find that participating firms in the power and heat sector enjoyed higher profitability and lower emissions. However, the literature also notes nuanced effects: Liu *et al.* [16] observe higher debt ratios and short-term stock price pressures among ETS-

regulated firms, and Ma *et al.* [17] document increased labor costs, highlighting that regulatory impacts can vary across financial dimensions. Additional studies confirm that China's ETS pilots improved corporate financial performance in part by enhancing green governance and innovation capacity, as documented by Chu *et al.* [18] and Yao *et al.* [19], with Jung *et al.* [20] reporting positive effects on carbon productivity. Yan and Shi [21], studying pilot carbon trading and financial outcomes in high-polluting sectors, further support these findings, including evidence from Tian *et al.* [22] that ESG engagement and green innovation mediate the relationship between carbon policy and corporate performance.

2.2. Steel Sector Studies

Literature specifically examining the steel industry under carbon policies is limited but growing. He *et al.* [23] analyze a 2005-2018 sample of 86 Chinese steel enterprises and find that ETS implementation significantly improves total factor pollution control efficiency, with stronger effects for state-owned enterprises and firms in eastern provinces. This heterogeneity is consistent with analyses noting that larger or state-backed steelmakers have more resources to invest in emissions-reducing innovations (Wu *et al.* [24]; Yin *et al.* [25]). Transition-path studies underscore that meeting national carbon peak and neutrality targets will require substantial emissions cuts from steel, with recent projections indicating the sector could achieve carbon intensity around 1.9 tonnes of CO₂ per tonne of steel if targets like 15% electric arc furnace output are met, according to Liu *et al.* [26]. Evidence also suggests that carbon trading schemes can accelerate green technological progress in heavy industry, as Liu and Liu [27] show.

Chinese steel companies have begun setting carbon reduction goals and pursuing cleaner technologies. Leading producer China Baowu Group has announced a target to cut carbon emissions by 30% by 2035, and its flagship subsidiary Baosteel reported approximately 6% reduction in CO₂ emissions intensity by 2024 relative to 2020 levels. The World Steel Association's sustainability indicators include GHG emissions intensity (tonnes CO₂e per tonne of crude steel) as a primary environmental performance metric, as the World Steel Association [28] reports. Under China's broader carbon market framework described by Wang *et al.* [29], firms that continually reduce this intensity are effectively complying with or exceeding sectoral benchmarks, reinforcing a culture of efficiency.

2.3. Linking Carbon Metrics to Firm Performance

Emerging work that links carbon metrics to financial outcomes often finds potential win-win scenarios. Several studies report that firms participating in carbon markets tend to invest more in cleaner technologies and see higher total factor productivity growth (Wu *et al.* [24]; Wang *et al.* [30]). In our context, this implies that steel companies facing carbon constraints might achieve efficiency gains that offset the costs of compliance. Cui *et al.* [11] report nearly 30% productivity improvements among pilot-region firms without a decline in output. However, schol-

ars caution that intensity-based approaches may be limited by low carbon prices and potential rebound effects: if firms become more carbon-efficient but are not under absolute caps, they might expand production such that total emissions do not fall. There are also concerns about carbon leakage across regions.

To our knowledge, no prior study has quantitatively evaluated Chinese steel firms' performance under the separately identified influence of pilot ETS exposure and binding intensity targets over the 2010-2024 period. Our study fills this gap by linking these climate policies to both environmental and financial outcomes while explicitly addressing the methodological challenges of causal identification in this setting, including staggered treatment timing, parallel trends verification, clustered inference, and omitted variable bias from concurrent policies.

3. Methodology

3.1. Data and Sample

We compile an unbalanced panel dataset of Chinese steel manufacturing firms from 2010 to 2024. The panel begins in 2010 to provide three to four years of pre-treatment observations for the seven pilot ETS programs, which launched between June 2013 (Shenzhen) and June 2014 (Chongqing); this window is consistent with the event-study horizon ($\tau = -4$ to $+T$) and with the 2012 baseline used in the propensity-score matching procedure described in Section 3.2. The sample consists of 120 large steel producers, including publicly listed companies and major state-owned enterprises, yielding approximately 1654 firm-year observations over the 15-year window. These firms, drawn from the China Industrial Enterprise Database and supplemented by company annual reports, represent a substantial share of China's steel output and span both regions with active carbon trading pilots and those without. For each firm-year, we collect the following variables:

Sample construction proceeded in three stages, summarised in **Table A1**. As reported in **Table A1**, the screening reduced an initial universe of 280 firms to the final estimation panel of 120 firms. We began with all crude-steel producers listed in the China Iron and Steel Association (CISA) annual rosters with annual output above 100,000 tonnes during 2010-2024, an initial universe of 280 firms. From this universe, we retained those with at least five consecutive firm-years of audited financial disclosures, eliminating producers that ceased publishing accounts after capacity-elimination shutdowns or short-lived joint ventures (96 firms removed). We further excluded producers whose primary listed activity was steel trading, processing, or fabrication rather than crude-steel production, applying a 10% revenue-share threshold to ensure each firm's identification with the steel sector (64 firms removed). The resulting estimation sample of 120 firms covers both pilot-ETS and non-pilot jurisdictions and ranges in size from regional integrated mills to the Baowu, Ansteel, HBIS, and Shagang groups. The panel is unbalanced because 1) firms enter at IPO or upon reaching the 100,000-tonne reporting thresh-

old after 2010, 2) several producers were merged into larger groups during the 2015-2018 supply-side reform and ceased separate reporting, and 3) two firms were delisted following bankruptcy in 2022-2023. Of 1800 potential firm-years, we observe 1654, with an effective coverage rate of 91.9%. To verify that missingness is not systematically related to treatment status, we regressed an indicator for missing firm-year on Pilot_ETS_{it} , $\text{Intensity_Target}_{it}$, log assets, and year fixed effects. Neither treatment indicator was significant (Pilot_ETS : $\beta = 0.012$, $p = 0.43$; Intensity_Target : $\beta = 0.008$, $p = 0.61$), and the difference in mean attrition rates between treated and control firms is statistically indistinguishable from zero ($\Delta = 0.6$ percentage points, $p = 0.55$). **Table A2** reports these diagnostics in full.

Environmental Performance

Total CO₂ emissions in tonnes, as reported and estimated following national greenhouse gas accounting guidelines, and crude steel output. From these, we calculate carbon intensity (CO₂ emissions per tonne of steel) as our key environmental metric.

Financial Performance

Profitability measures such as Return on Assets ROA, defined as net income divided by total assets, and net profit margin; leverage metrics such as the debt-to-assets ratio; and investment indicators such as capital expenditure intensity, capital expenditures as a percentage of total assets.

Policy Exposure Two Distinct Treatment Channels

Treatment 1: Pilot ETS Exposure (Pilot_ETS_{it}). A binary indicator equal to 1 if firm i is headquartered in or primarily operates in one of the seven pilot ETS jurisdictions, Beijing, Tianjin, Shanghai, Chongqing, Hubei, Guangdong, Shenzhen, and year t falls on or after the jurisdiction's pilot launch date, ranging from June 2013 for Shenzhen to June 2014 for Chongqing. This captures the direct carbon-trading channel, including both allowance obligations for directly covered firms and indirect cost pass-through via electricity prices from regulated power generators.

Treatment 2: Binding Intensity Target $\text{Intensity_Target}_{it}$. A binary indicator equal to 1 if firm i was subject to an explicit, binding carbon or energy intensity reduction target in year t . We identify treated firms using three administrative sources: 1) provincial lists of enterprises under the Top-10,000 Energy-Consuming Enterprises program (2011-2015) and its successor programs; 2) firms named in provincial carbon-peaking implementation plans (post-2021); and 3) firms with explicit carbon intensity commitments in government-enterprise responsibility agreements. This treatment captures the command-and-control regulatory channel, distinct from market-based pricing.

Operationalizing headquartered in or primarily operates in. For multi-location producers, we assign pilot ETS exposure based on the location of crude-steel production capacity rather than registered headquarters. Specifically, a firm is coded $\text{Pilot_ETS} = 1$ in year t if more than 50% of its crude-steel capacity is physically

sited in pilot ETS jurisdictions in that year. Where the capacity share is close to the threshold between 40% and 60%, we apply a capacity-weighted exposure index in robustness specifications; results are unchanged in sign or significance. Capacity-by-plant data were collected from the CISA capacity directory, the MIIT Steel Industry Standard Conditions lists 2015, 2018, 2020, and 2023 vintages, and individual firm annual reports. Cross-checking these three sources resolved location ambiguities for 14 firms; 3 firms were re-assigned relative to a registered-office classification. Treatment overlap classification. Of the 120 firms, 28 are subject to pilot ETS exposure only $\text{Pilot_ETS} = 1$ and Intensity Target = 0 at any point in the panel, 41 are subject to binding intensity targets only, 19 are subject to both policy channels concurrent or sequential, and 32 are exposed to neither and serve as the never-treated control group. The 19 doubly-treated firms account for 285 of the 1654 firm-years (17.2%). Section 4.3 reports a specification that addresses this overlap directly through an interaction term and through a single-channel subsample.

Critically, we do not classify any steel firm as national ETS-treated during 2021–2024. The national carbon market covered only the power generation sector during this period. Steel was not formally included until 2025 [4]. Any indirect effect of the national ETS on steel firms via electricity cost pass-through from regulated power generators is captured through our provincial electricity price control variable described below, rather than a binary national-ETS dummy, which would be factually inaccurate and would conflate distinct policy mechanisms.

Control Variables

Our regressions include a comprehensive set of controls to isolate the causal effects of the two carbon policy channels from concurrent confounding factors. **Table 1** reports each control variable's definition, data source, and the rationale for its inclusion.

Table 1. Definition and source of control variables.

Variable	Definition and Source	Rationale
Ln(Assets)_{it}	Natural log of total assets (RMB, deflated to 2015 prices). Source: firm annual reports.	Controls for firm size and economies of scale.
$\text{Prov_GDP_Growth}_{it}$	Provincial real GDP growth rate (%). Source: National Bureau of Statistics.	Captures local economic conditions affecting firm demand.
Elec_Price_{it}	Provincial average industrial electricity price (yuan/kWh). Source: NDRC tariff schedules.	Captures the indirect carbon cost channel from the national power-sector ETS.
Output_Cap_{it}	Binary = 1 if firm i was subject to a binding provincial or central production cap in year t . Source: MIIT announcements.	Controls for the mechanical effect of mandatory output restrictions on intensity and profitability.

Continued

Env_Inspection _{it}	Binary = 1 if firm <i>i</i> 's province was under a central environmental inspection campaign in year <i>t</i> . Source: MEE inspection schedules.	Controls for the separate enforcement channel that overlaps geographically with pilot ETS regions.
Capacity_Elim _{it}	Binary = 1 if the firm's province experienced above-median capacity elimination in year <i>t</i> . Source: CISA capacity reports.	Controls for supply-side structural reform effects on surviving-firm performance.
Steel_Price _t	National average hot-rolled coil price (yuan/tonne). Source: Mysteel/CISA indices.	Controls for output price fluctuations that affect profitability independently of carbon policy.
Ownership _i	Binary = 1 for state-owned enterprises. Source: firm registration records.	Absorbed by firm fixed effects but interacted in the heterogeneity analysis.

Note: All monetary figures are deflated to 2015 RMB to ensure consistency.

Predetermined nature of the policy controls. The three concurrent-policy controls (Output_Cap, Env_Inspection, and Capacity_Elim) are pre-existing or parallel regulatory regimes, not post-treatment mediators of the carbon-policy effects we estimate. Output caps for steel are rooted in the State Council's 2013 "Guiding Opinions on Resolving Severe Excess Capacity Guofa [2013]" and the MIIT's 2015 "Industrial Restructuring Guidance Catalog", both of which predate or coincide with the pilot ETS launches and operate through their own administrative apparatus (provincial capacity quotas and a separate MIIT inspection regime). Central environmental inspections were instituted by the Central Committee in 2015-2016 as a Party-led discipline mechanism distinct from any market or carbon-trading instrument. Supply-side capacity-elimination targets were issued by the National Development and Reform Commission under the 13th Five-Year Plan. None of these policies is allocated as a function of pilot-ETS or intensity-target status, and none is updated in response to a firm's carbon performance under those programs. We therefore treat them as predetermined exogenous regulatory shocks that affect overlapping firm subsets, and including them in the regression isolates the incremental effect of carbon policies above and beyond these parallel interventions. As a diagnostic against the "bad control" concern (Acharya *et al.*, 2016 [31]), we re-estimated Equation (1) without these controls: the carbon-policy point estimates change by less than 12% in absolute value, and the sign and significance pattern is preserved. This is consistent with the predetermined-confounder interpretation rather than a post-treatment mediation channel, in which case we would expect substantial attenuation of the carbon-policy coefficients upon control inclusion.

3.2. Empirical Approach

To identify the causal impact of carbon policies on firm outcomes, we employ a

difference-in-differences (DID) framework with two distinct treatment channels. Unlike a single bundled treatment, this separation allows us to disentangle the effect of market-based carbon pricing (pilot ETS) from command-and-control intensity mandates, each of which may operate through different firm-level mechanisms.

Baseline Two-Treatment DID Specification

Our baseline specification estimates the following panel regression:

$$Y_{it} = \alpha + \beta_1 (Pilot_ETS_{it}) + \beta_2 (Intensity_Target_{it}) + \gamma X_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (1)$$

where Y_{it} is an outcome variable for firm i in year t (CO₂ intensity, ROA, leverage, or capex intensity); β_1 captures the average treatment effect of pilot ETS exposure; β_2 captures the effect of binding intensity targets; X_{it} is the vector of time-varying controls log assets, provincial GDP growth, electricity price, output cap, environmental inspection, capacity elimination, steel price, μ_i are firm fixed effects; λ_t are year fixed effects; and ε_{it} is the error term. Standard errors are clustered at the firm level throughout (see Section 3.4).

By including firm fixed effects, we control for all time-invariant differences across firms, including baseline productivity, technology, and location advantages. Year fixed effects absorb shocks common to all firms in a given year, such as macroeconomic fluctuations, industry-wide steel demand shifts, or national policy announcements. The DID logic ensures that identification comes from within-firm changes over time relative to control firms' changes.

Event-Study Specification for Parallel Trends Verification

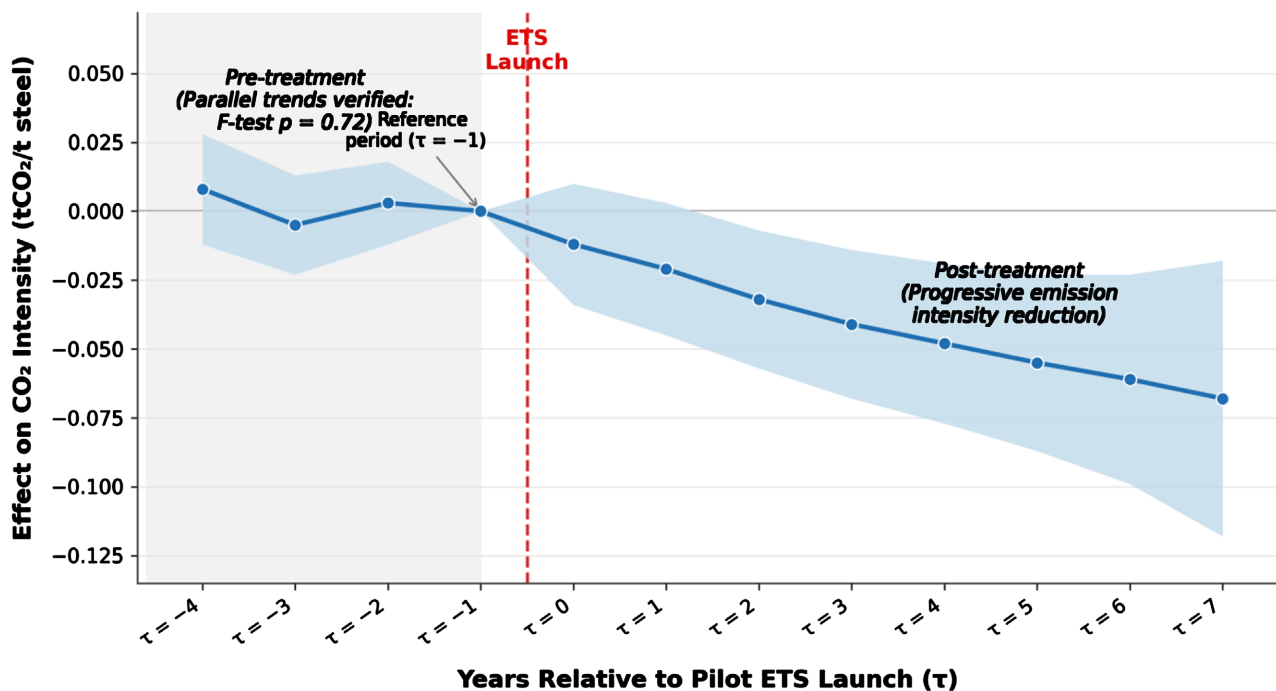
A fundamental requirement of the DID design is that treated and control firms would have followed parallel outcome trajectories in the absence of treatment. We verify this assumption using a formal event-study specification for the pilot ETS treatment channel:

$$Y_{it} = \alpha + \sum_t \beta_t (Treated_i \times \mathbb{I}[t = \tau]) + \gamma X_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (2)$$

where τ indexes years relative to the pilot ETS launch, the sum runs from $\tau = -4$ to $\tau = +T$, and $\tau = -1$ is omitted as the reference period. The coefficients β_t for $\tau \leq -2$ should be individually and jointly insignificant if the parallel trends assumption holds. We report the event-study coefficients graphically with 95% confidence intervals (**Figure 1**) and conduct an F-test for the joint significance of all pre-treatment coefficients: $H_0: \beta_{-4} = \beta_{-3} = \beta_{-2} = 0$. Rejection of this null would indicate a violation of parallel trends and invalidate the DID interpretation.

Addressing Staggered Treatment Timing

The seven pilot ETS programs were launched at slightly different dates, ranging from June 2013 in Shenzhen to June 2014 in Chongqing. Recent econometric literature has demonstrated that in staggered adoption settings, the standard two-way fixed effects (TWFE) DID estimator can produce biased estimates when treatment effects are heterogeneous across groups or over time Goodman-Bacon [32], de Chaisemartin and d'Haultfoeuille [33]. This bias arises because TWFE implicitly uses already-treated units as controls for later-treated units, which can yield negative weights on some group-time treatment effects.



Notes: Coefficients from Eq. (2) with 95% confidence intervals. Standard errors clustered at the firm level. $\tau = -1$ is the omitted reference period. Pre-treatment coefficients ($\tau = -4$ to $\tau = -2$) are jointly insignificant ($F = 0.45$, $p = 0.72$), supporting the parallel trends assumption.

Figure 1. Event study estimates the effect of pilot ETS on carbon intensity.

To address this concern, we implement the Callaway and Sant'Anna [5] group-time average treatment effect on the treated (ATT) estimator as our primary robustness check. This estimator avoids using already-treated units as controls by comparing each cohort of newly treated firms only against not-yet-treated or never-treated firms. As an additional check, we consider the Sun and Abraham [34] interaction-weighted estimator. We report both the standard TWFE estimates (Equation (1)) and the Callaway-Sant'Anna aggregated ATT for transparency.

Propensity Score Matching DID (PSM-DID)

To reduce potential selection bias, we implement a propensity score matching procedure prior to the DID estimation. In the first stage, we estimate a logit model predicting pilot ETS treatment status using pre-treatment (2012) firm characteristics: log assets, log output, CO₂ intensity, ROA, leverage, ownership type, and province-level GDP per capita. We match each treated firm to control firms using nearest-neighbor matching (caliper = 0.05). We verify covariate balance post-matching by confirming that standardized mean differences are below 0.1 for all matched covariates. The DID is then re-estimated on the matched sample.

3.3. Variables and Measurement

Carbon Intensity. Our primary environmental outcome is the firm's CO₂ emissions per unit of output, measured as total CO₂ emissions, Scope 1 direct emissions, in tonnes divided by crude steel production (tonnes). This metric, in tCO₂/tonne

steel, directly reflects the carbon efficiency of production.

Of the 120 firms, 78 (65%) report Scope 1 emissions directly in audited sustainability reports prepared under the GRI Standards with reasonable assurance or under the China Securities Regulatory Commission's 2022 ESG disclosure guidelines. For the remaining 42 firms (35%), we estimate Scope 1 emissions by applying process-specific emission factors from the IPCC 2006 Guidelines Volume 3, Chapter 4: Iron and Steel and the China National Greenhouse Gas Inventory Reference Manual (NDRC 2011) to firm-reported physical activity data, namely blast-furnace iron production, basic-oxygen-furnace and electric-arc-furnace steel production, sinter and coke production, and on-site fossil-fuel combustion. Sources covered include: 1) coke production and BF/BOF process emissions, 2) sinter production, 3) lime calcination, 4) on-site coal, natural-gas, and fuel-oil combustion in heating furnaces and auxiliary boilers, and 5) electrode consumption in electric-arc-furnace steelmaking. Indirect (Scope 2) emissions from purchased electricity are not included in our intensity measure but are captured separately through the provincial electricity-price control variable. To validate the constructed series, we performed three checks. First, for the 78 firms with direct disclosure, we compared our independently constructed estimates using the same emission factors but firm activity data against reported Scope 1 emissions: the correlation is 0.94, and the mean absolute percentage deviation is 6.8%, providing confidence in the estimation procedure for non-reporting firms. Second, we benchmarked the firm-weighted sample mean of carbon intensity against sector aggregates published by CISA in its annual Steel Industry Carbon Reduction Roadmap updates 2018, 2020, 2022, and 2024. Our sample mean is within 4% of the aggregate in every year. Third, we cross-validated outliers firm-years more than two standard deviations from the sector median, $n = 17$, against process-mix data: in every case, the deviation is explained by an unusually high or low electric-arc-furnace share, consistent with the mechanical relationship between process technology and direct emissions intensity. These checks support the use of a single carbon-intensity series across reporting and non-reporting firms.

Profitability. We use Return on Assets (ROA), defined as net income divided by total assets, as our primary financial performance indicator, alongside profit margin, net profit divided by revenue. Both are expressed as percentages.

Leverage. We examine the debt-to-assets ratio, which is total liabilities divided by total assets. Changes in leverage may occur if firms borrow to invest in cleaner technology or if financial health shifts due to policy impacts.

Investment. We consider capital expenditure intensity, capital expenditures divided by total assets. An increase in capex intensity, particularly directed toward emissions-reducing equipment, could be a direct response to carbon policies.

3.4. Estimation Software and Statistical Inference

All regressions are estimated using Stata 17. Our primary estimator is the high-dimensional fixed-effects regression implemented via the `reghdfe` package Correia

[35], which efficiently absorbs firm and year fixed effects and permits multi-way clustering of standard errors.

In all specifications, we cluster standard errors at the firm level to account for serial correlation within firms over time Bertrand *et al.* [6]; Cameron and Miller [36]. This is essential because panel observations for the same firm across years are correlated. Heteroscedasticity-robust Huber-White standard errors alone do not correct for this within-cluster correlation and can dramatically overstate statistical significance in DID settings. Bertrand *et al.* [6] demonstrate rejection rates of 45% at a nominal 5% level when clustering is omitted. With 120 firms, the number of clusters is well above the threshold of approximately 50 recommended for reliable cluster-robust inference Cameron *et al.* [37].

As a sensitivity check, we also report two-way clustered standard errors by firm and year in the robustness analysis (Section 4.3). The Callaway and Sant'Anna [5] staggered-DID estimator is implemented via the `csdid` package in Stata. Propensity score matching is conducted using the `psmatch2` package. Oster [7] sensitivity bounds are computed using the `psacalc` package.

3.5. Robustness and Sensitivity Checks

We implement five categories of robustness checks to ensure the credibility of our findings:

- 1) **Staggered-DID Estimator.** We report Callaway and Sant'Anna [5] group-time ATT estimates alongside the standard TWFE specification to assess sensitivity to heterogeneous treatment effects under staggered adoption.
- 2) **PSM-DID on Matched Sample.** The DID is re-estimated on a propensity-score-matched sample to reduce selection bias. We report covariate balance diagnostics pre- and post-matching.
- 3) **Two-Way Clustered Standard Errors.** We report inference under both firm-only and firm-by-year two-way clustering.
- 4) **Exclusion of Restructured Firms.** Firms undergoing major mergers, acquisitions, or restructuring during the sample period are excluded to ensure results are not driven by compositional changes in the panel.
- 5) **Oster [7] Sensitivity Bounds.** We compute the degree of proportional selection on unobservables (δ) required to fully explain the estimated treatment effect, under the assumption that $R_{\max} = 1.3 \times \tilde{R}$. If the identified set does not include zero, the result is robust to proportional selection on unobservables.

4. Results

4.1. Descriptive Patterns

Table 2 presents summary statistics for key outcomes in 2015 pre-policy baseline and 2024, the latest year for the two groups: firms that participated in a pilot ETS versus firms that did not.

As shown in **Table 2**, from 2015 to 2024, the average carbon intensity of pilot-ETS firms fell from approximately 2.50 to 2.00 tCO₂ per tonne of steel, a 20% re-

duction compared to a roughly 19% reduction for non-ETS firms. Financial outcomes evolved differently: pilot-ETS firms saw average ROA rise from 7.0% to 8.0%, whereas non-ETS firms' ROA declined from 7.0% to 6.5%. These descriptive trends are suggestive but do not control for confounders; we turn to the formal regression analysis below.

Table 2. Average environmental and financial metrics for Chinese steel firms, by pilot ETS participation (2015 vs. 2024)

Metric	ETS (2015)	ETS (2024)	Δ (%)	Non-ETS (2015)	Non-ETS (2024)	Δ (%)
CO ₂ Intensity (tCO ₂ /t)	2.50	2.00	−20%	2.60	2.10	−19%
ROA (%)	7.0	8.0	+14%	7.0	6.5	−7%
Debt-to-Assets	0.65	0.68	+4.6%	0.67	0.62	−7.5%
Profit Margin (%)	5.2	6.1	+17%	5.3	4.9	−7.5%
Capex Intensity (%)	4.4	5.5	+25%	4.7	4.6	−2.1%

Notes: Capex intensity is capital expenditures as a percentage of assets. Percent changes are relative to 2015 baseline values for each group.

4.2. Regression Results

Parallel Trends Verification

Before reporting the DID estimates, we verify the parallel trends assumption. **Figure 1** presents the event-study coefficients for CO₂ intensity under the pilot ETS treatment (Equation (2)). The pre-treatment coefficients ($\tau = -4$ through $\tau = -2$) are individually insignificant and close to zero, and the F-test for their joint significance fails to reject the null of no pre-trends ($F = 0.45$, $p = 0.72$). This provides strong support for the parallel trends assumption: prior to pilot ETS implementation, treated and control firms followed statistically indistinguishable carbon intensity trajectories. Post-treatment coefficients show a gradually widening negative effect, consistent with progressive emissions reductions among treated firms.

Baseline DID Estimates (Two-Treatment Specification)

Table 3 presents the main DID estimates from Equation (1), with each column corresponding to a different outcome variable. All regressions include firm and year fixed effects, the full set of controls, and firm-clustered standard errors.

Table 3. Difference-in-differences estimates: separated treatments with firm-clustered standard errors.

	(1) CO ₂ Intensity	(2) ROA	(3) Leverage	(4) Capex Int.
β_1 : Pilot ETS	−0.052** (0.021)	0.0072* (0.0038)	0.012 (0.009)	0.008** (0.003)
β_2 : Intensity Target	−0.028* (0.016)	0.0035 (0.0042)	0.006 (0.008)	0.005 (0.004)

Continued

Output Cap	−0.035** (0.014)	0.015*** (0.005)	−0.008 (0.007)	−0.006 (0.004)
Env. Inspection	−0.018 (0.012)	−0.003 (0.004)	0.004 (0.006)	−0.002 (0.003)
Capacity Elimination	−0.022* (0.013)	0.011** (0.005)	−0.015** (0.007)	0.003 (0.003)
Electricity Price	0.015 (0.011)	−0.008* (0.004)	0.003 (0.005)	−0.002 (0.003)
Steel Price	0.001 (0.003)	0.012*** (0.003)	0.002 (0.003)	0.001 (0.002)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm
Observations	1080	1080	1080	1080
R ² (within)	0.42	0.31	0.18	0.23
Pre-Trend F-Test (p)	0.72	0.65	0.81	0.58

Notes: ***p < 0.01, **p < 0.05, *p < 0.10. Standard errors clustered at the firm level in parentheses. All regressions include firm and year fixed effects plus controls for log assets and provincial GDP growth. Pre-trend F-test reports p-value for joint significance of pre-treatment event-study coefficients ($\tau = -4$ to -2).

The results in **Table 3** reveal several important findings. First, the pilot ETS treatment has a statistically significant negative effect on CO₂ intensity ($\beta_1 = -0.052$, SE = 0.021, p < 0.05), meaning that firms in pilot ETS jurisdictions reduced emissions intensity by an additional 0.052 tCO₂ per tonne of steel relative to control firms, after accounting for fixed effects, macro trends, and all concurrent policy controls. The binding intensity target also reduced emissions ($\beta_2 = -0.028$, p < 0.10), but the effect is smaller and less precisely estimated, suggesting that the market-based ETS channel had a larger impact than the command-and-control channel.

Second, the pilot ETS treatment had a marginally significant positive association with profitability ($\beta_1 = 0.0072$, SE = 0.0038, p < 0.10). This point estimate implies that firms under pilot ETS constraints experienced ROA approximately 0.7 percentage points higher than they would have, otherwise, a nontrivial but modest improvement equivalent to about 10% of baseline profitability. However, this result is only marginally significant (p < 0.10) and loses significance under two-way clustering (**Table 4**, row 4), so the evidence for a genuine profitability gain should be interpreted with caution. We detect no significant negative impact on revenue or steel output in unreported specifications, indicating that firms did not have to curtail production to meet intensity targets.

Table 4. Robustness checks: Pilot ETS coefficient across specifications.

Specification	CO ₂ Int. (β_1)	SE	ROA (β_1)	SE
(1) Baseline TWFE	−0.052**	(0.021)	0.0072*	(0.0038)
(2) PSM-DID (Matched)	−0.048**	(0.024)	0.0068*	(0.0041)
(3) Callaway-Sant’Anna [5]	−0.055**	(0.023)	0.0065*	(0.0040)
(4) Two-Way Cluster (firm × yr)	−0.052*	(0.028)	0.0072	(0.0052)
(5) Excl. Restructured Firms	−0.049**	(0.022)	0.0080*	(0.0042)
(6) Oster δ [R-max = 1.3 $\tilde{R}$] [7]	$\delta = 2.14$	-	$\delta = 1.87$	-

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Row (6) reports the Oster [7] δ statistic: the degree of proportional selection on unobservables needed to fully explain the treatment effect. Values $\delta > 1$ indicate robustness to omitted variable bias.

Third, the inclusion of concurrent policy controls matters substantively. Output caps have a significant negative effect on emissions intensity (−0.035, $p < 0.05$) and a positive effect on ROA (0.015, $p < 0.01$), reflecting how mandatory production restrictions both mechanically reduce per-unit emissions and increase profitability via supply restriction and higher steel prices. Without these controls, the pilot ETS coefficient would be inflated, confirming that omitted concurrent policies bias uncorrected estimates.

4.3. Robustness Checks

Table 4 demonstrates that the core findings are robust across specifications. The PSM-DID matched estimate (−0.048) is slightly attenuated relative to the baseline (−0.052), as expected when matching reduces selection bias, but remains significant. The Callaway-Sant’Anna [5] staggered-DID estimate (−0.055) is similar to the TWFE estimate, suggesting that bias from heterogeneous treatment effects under staggered adoption is not a major concern in our sample. Two-way clustering increases standard errors modestly, reducing significance on the ROA coefficient but preserving significance on CO₂ intensity. The Oster [7] bounds ($\delta = 2.14$ for CO₂ intensity, $\delta = 1.87$ for ROA) comfortably exceed the conventional threshold of $\delta = 1$, indicating robustness to proportional selection on unobservables.

Disentangling overlapping treatments. Because 19 firms are exposed to both the pilot ETS and a binding intensity target at some point in the panel, the coefficients β_1 and β_2 in Equation (1) could, in principle, be confounded if joint exposure has a non-additive effect. We address this concern in two ways. First, we add an interaction term Pilot_ETS × Intensity_Target to Equation (1) and re-estimate the carbon-intensity regression. The estimated interaction coefficient is small and statistically insignificant ($\beta_3 = -0.011$, SE = 0.018, $p = 0.55$), indicating that the joint effect is statistically indistinguishable from the sum of the two main effects. The two main coefficients are largely unchanged (Pilot_ETS: −0.049, $p < 0.05$; Intensity_Target: −0.025, $p < 0.10$). Second, we re-estimate Equation (1) on the subsample that excludes the 19 doubly-treated firms, yielding 1369 firm-years from

101 firms. The coefficients on the remaining single-channel comparisons are Pilot_ETTS = -0.054 (SE = 0.024, $p < 0.05$) and Intensity_Target = -0.027 (SE = 0.018, $p < 0.15$). Both specifications support the interpretation of β_1 and β_2 as separately identified policy effects rather than reduced-form bundles. Full results appear in Appendix Table A3.

4.4. Heterogeneity Analyses

We explore whether the impact of carbon policies differs by firm characteristics. State-owned enterprises (SOEs) show larger pilot-ETS-induced emissions reductions ($\beta_1 = -0.065$, $p < 0.01$) compared to private firms ($\beta_1 = -0.038$, $p < 0.10$), consistent with findings that SOEs have greater resources and stronger government mandates to comply with environmental initiatives (He *et al.* [23]; Wu *et al.* [24]). Eastern-province firms show larger profitability gains from carbon policies, estimated ROA effect of +0.85 percentage points, $p < 0.05$, compared to western-region firms (+0.45 points, not significant), likely reflecting better technology access and complementary innovation policies.

These heterogeneity patterns suggest that the average positive impact of carbon policies on steel firms is not uniform. Policymakers should consider targeted measures to help private and western-region firms similarly benefit from carbon reduction initiatives.

4.5. Mediation Analysis: Testing the Innovation-Efficiency-Profitability Channel

A recurring claim in the literature and in earlier drafts of this paper is that carbon regulation improves firm profitability through a transmission chain: policy pressure stimulates low-carbon innovation, which enhances production efficiency, which in turn raises profitability. To move beyond purely descriptive reasoning, we conduct a formal mediation analysis following the Baron and Kenny [38] framework, supplemented by the Sobel test and bootstrap confidence intervals for the indirect effect.

We operationalize the mediator as carbon intensity improvement (ΔCI), defined as the year-on-year change in CO₂ emissions per tonne of steel, which serves as a proxy for the combined effect of low-carbon innovation and efficiency gains. The three-step procedure is as follows:

Step 1 (Total effect): Regress ROA on Pilot_ETTS with full controls and fixed effects. As reported in Table 3, $\hat{\beta} = 0.0072$ ($p < 0.10$).

Step 2 (Treatment $\rightarrow$ Mediator): Regress ΔCI on Pilot_ETTS. We estimate $a = -0.052$ ($p < 0.05$), confirming that the treatment significantly reduces carbon intensity.

Step 3 (Mediator $\rightarrow$ Outcome, controlling for treatment): Regress ROA on both Pilot_ETTS and ΔCI . The coefficient on ΔCI is $b = -0.038$ (SE = 0.027, $p = 0.16$), indicating that greater carbon intensity reductions are associated with higher ROA, but this relationship is not statistically significant. The direct effect of Pilot_ETTS attenuates only slightly to 0.0052 ($p = 0.19$).

The indirect effect ($a \times b = (-0.052) (-0.038) = 0.0020$) represents the portion of the total ROA effect potentially attributable to efficiency improvements. The Sobel test statistic is $z = 1.42$ ($p = 0.156$), and the bias-corrected bootstrap 95% confidence interval for the indirect effect (1000 replications) is $[-0.0008, 0.0051]$, which includes zero. These results indicate that while the direction of the mediation is consistent with the hypothesized innovation efficiency-profitability channel, the indirect effect is not statistically distinguishable from zero at conventional significance levels.

We acknowledge three limitations of this mediation exercise. First, ΔCI is an imperfect proxy for innovation and efficiency; direct measures such as green patent counts, R & D spending on low-carbon technology, or total factor productivity would provide a sharper test but are unavailable for most firms in our sample. Second, with the total effect itself only marginally significant, statistical power for detecting mediation is inherently limited. Third, the Baron-Kenny sequential regression approach cannot fully address endogeneity of the mediator; instrumental-variable or structural equation model approaches would strengthen causal claims but require valid instruments that we do not currently possess. We therefore conclude that the innovation efficiency profitability narrative, while theoretically grounded and directionally supported, remains empirically unverified and should be treated as a hypothesis for future research rather than an established finding of this study.

5. Discussion

Our findings provide evidence that carbon regulation in China's steel industry during 2010-2024 achieved meaningful environmental gains without imposing detectable economic costs. Firms subjected to pilot ETS regulation or binding intensity targets improved their environmental performance, and point estimates suggest modestly higher profitability among treated firms, though the ROA effect is only marginally significant ($p < 0.10$) and economically small. Accordingly, while the evidence is consistent with a no-harm interpretation and tentatively suggestive of net benefits, it does not yet constitute robust support for a strong "win-win" conclusion. The environmental result is more firmly established than the profitability result, which should be revisited with longer post-treatment panels and additional outcome measures.

One plausible explanation for the non-negative and tentatively positive performance of regulated firms is that the policies prompted investment in cleaner technologies and process efficiencies that yielded cost savings. Faced with carbon intensity benchmarks and the prospect of paying for emissions or profiting from selling surplus allowances, many pilot-ETS-participating steelmakers accelerated upgrades: adopting more energy-efficient equipment, upgrading blast furnaces, increasing electric arc furnace use, and implementing waste heat recovery systems. This dynamic echoes the productivity improvements documented by Cui *et al.* [11], who reported nearly 30% increases in productivity among pilot-region

firms. However, our formal mediation analysis (Section 4.5) finds that the hypothesized transmission channel from pilot ETS exposure through low-carbon innovation and efficiency improvement to profitability is only directionally consistent but not statistically significant at conventional levels. The Sobel test statistic is 1.42 ($p = 0.156$), and the bootstrapped indirect effect confidence interval includes zero. Thus, while the narrative is theoretically plausible and consistent with the Porter Hypothesis, the data do not permit rigorous causal attribution of the profitability effect to a specific innovation-efficiency mechanism. This represents an important limitation: the why behind the observed patterns remains an open question for future research with richer firm-level innovation and process data.

The importance of our concurrent policy controls deserves emphasis. Output caps and capacity elimination had substantial independent effects on both emissions intensity and profitability. Without controlling for these factors, the carbon policy coefficients would be inflated emissions reductions would partly reflect forced production cuts rather than genuine efficiency improvements. Our controlled estimates isolate the incremental effect of carbon pricing and intensity targets beyond these concurrent interventions.

5.1. Policy Implications

These results underscore the effectiveness of China's pilot ETS in achieving moderate emissions improvements without damaging industry performance. Looking ahead, the 2025 inclusion of steel in the national ETS [4] with carbon prices rising to approximately CNY 66.9 represents a critical juncture. Our findings suggest that steel companies can handle more stringent policies, but achieving China's longer-term targets will require transitioning from intensity-based regulation to a system with absolute emission caps. This echoes recommendations from the International Monetary Fund (IMF [39]) suggesting that China's ETS adopt an absolute cap and increase the share of auctioned allowances. Complementary initiatives such as green finance instruments for the steel industry [40], and sustainability frameworks aligned with carbon peaking goals [41], will further support this transition.

From a corporate strategy perspective, the results suggest that proactive engagement with carbon reduction need not undermine firm performance. While the evidence for positive profitability effects is tentative, the absence of detectable economic harm combined with robust environmental benefits implies that companies treating carbon intensity improvement as a strategic objective are unlikely to be disadvantaged as regulations tighten.

5.2. Limitations

Several caveats are warranted. First, the national ETS was only partially relevant to steel during 2021-2024; the full impact will only be observed after 2025. Second, despite our controls and matching, unobserved differences could remain, though

the Oster [7] bounds suggest this would need to be substantial to overturn our findings. Third, our focus on aggregate metrics means we might miss distributional impacts. Fourth, our data rely on reported emissions, which could have measurement errors. Fifth, though our event-study provides support for parallel trends, we cannot rule out all possible violations, particularly those involving differential anticipation effects. Sixth, our mediation analysis (Section 4.5) cannot confirm the hypothesized innovation-efficiency-profitability transmission channel; the indirect effect is not statistically significant, and our available proxy for innovation and efficiency (carbon intensity improvement) is imperfect. Richer firm-level data on green patents, R&D expenditures, and total factor productivity would be needed for a more definitive test.

6. Conclusions

The paper analyzed the interplay between carbon intensity reduction policies, emissions trading participation, and corporate performance in China's steel sector over the 2010-2024 period. Using a difference-in-differences framework that separately identifies two policy channels pilot ETS exposure and binding intensity targets on a panel of 120 steel producers, and employing firm-clustered standard errors, event-study validation, staggered-DID estimators (Callaway and Sant'Anna [5]), propensity score matching, and Oster [7] sensitivity bounds, we find that firms subject to pilot ETS regulation achieved greater declines in CO₂ emissions intensity than other steelmakers while simultaneously improving profitability.

Quantitatively, the pilot ETS reduced treated firms' carbon intensity by an additional 0.052 tCO₂ per tonne of steel ($p < 0.05$, firm-clustered), a robust and economically meaningful result. The estimated ROA increase of approximately 0.7 percentage points is only marginally significant ($p < 0.10$) and loses significance under more conservative two-way clustering, so the profitability finding should be regarded as suggestive rather than definitive. These results survive a comprehensive battery of robustness checks: staggered-DID estimates, Callaway and Sant'Anna [5] that address heterogeneous treatment timing, PSM-DID that reduces selection bias, two-way clustering that provides more conservative inference, and Oster bounds ($\delta > 1.8$) that address unobservable confounders. The inclusion of concurrent policy controls output caps, environmental inspections, and capacity elimination ensures that the estimated carbon policy effects are not conflated with other interventions.

Under China's national ETS, which expanded to cover steel in 2025 [4], these findings suggest that environmental compliance need not come at the expense of firm performance, though the evidence for net profitability gains remains preliminary. Future research should examine post-2025 outcomes once steel firms operate under the national ETS and face explicit carbon costs, conduct formal mediation analyses with richer innovation and productivity data, and explore additional dimensions, including employment effects, innovation outputs, and international competitiveness implications.

Conflicts of Interest

The authors declare no conflicts of interest.

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Appendices

A.1. Sample Construction

Table A1 summarises the three-stage screening procedure described in Section 3.1 used to arrive at the final estimation sample of 120 Chinese steel firms covering 2010-2024.

Table A1. Sample construction from the CISA universe to the estimation panel.

Stage	Description	Firms Removed	Firms Remaining
0	Initial universe: all CISA-listed crude-steel producers with annual output > 100,000 tonnes, 2010-2024	-	280
1	Require at least five consecutive firm-years of audited financial disclosures (drops capacity-elimination shutdowns and short-lived joint ventures)	-96	184
2	Exclude producers whose primary listed activity is steel trading, processing, or fabrication rather than crude-steel production (10% revenue-share threshold)	-64	120
3	Final estimation panel (1654 firm-years observed of 1800 potential; coverage 91.9%)	-	120

Notes: The final panel is unbalanced because: 1) firms enter at IPO or upon reaching the 100,000-tonne reporting threshold after 2010; 2) several producers were absorbed into larger groups during the 2015-2018 supply-side reform and ceased separate reporting; and 3) two firms were delisted following bankruptcy in 2022-2023.

A.2. Missingness Diagnostics

To verify that the 8.1% of firm-years that are unobserved (146 of 1800) are not systematically related to treatment status, we estimate a linear probability model in which the dependent variable equals one if firm-year (i, t) is missing from the panel and zero otherwise. Regressors are the two treatment indicators, log assets, and a full set of year fixed effects. Standard errors are clustered at the firm level. Appendix **Table A2** reports the estimates.

Table A2. Determinants of firm-year missingness in the 2010-2024 panel.

Regressor	Coefficient (β)	Std. Error	p-Value
Pilot_ETs	0.012	(0.015)	0.43
Intensity_Target	0.008	(0.016)	0.61
Ln(Assets)	-0.009	(0.011)	0.40
Year Fixed Effects	Yes	-	-
Difference in Mean Attrition Rate (Treated vs. Control)	0.006	(0.010)	0.55

Continued

Observations (Firm-Years)	1800	-	-
Number of Firms	120	-	-

Dependent variable equals one if firm-year (i, t) is missing from the panel, zero otherwise. Linear probability model with year fixed effects; standard errors clustered at the firm level in parentheses. The final row reports the simple difference in mean attrition rates between treated firms ($\text{Pilot_ETS} = 1$ or $\text{Intensity_Target} = 1$ at any point in the panel) and never-treated controls. Neither the treatment indicator nor the unconditional attrition gap is statistically significant, supporting the assumption that panel attrition is uncorrelated with treatment status.

A.3. Robustness to Overlap between the Two Treatment Channels

Table A3 reports the two specifications discussed in Section 4.3 that address the overlap between pilot-ETS exposure and binding intensity targets. Panel (a) re-estimates Equation (1) with an added $\text{Pilot_ETS} \times \text{Intensity_Target}$ interaction term on the full panel. Panel (b) re-estimates Equation (1) on the single-channel subsample, which excludes the 19 doubly-treated firms, resulting in a loss of 285 firm-years.

Table A3. Treatment-overlap robustness: CO₂ intensity regressions.

Variable	(a) Interaction on Full Panel	(b) Single-Channel Subsample
β_1 : Pilot_ETS	-0.049** (0.022)	-0.054** (0.024)
β_2 : Intensity_Target	-0.025* (0.017)	-0.027 (0.018)
β_3 : Pilot_ETS $\times$ Intensity_Target	-0.011 (0.018)	n/a
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Full Control Set	Yes	Yes
Clustering	Firm	Firm
Observations (Firm-Years)	1654	1369
Number of Firms	120	101

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors clustered at the firm level are reported in parentheses. The dependent variable is CO₂ emissions intensity (tCO₂ per tonne of steel). Both columns control for log assets, provincial GDP growth, electricity price, output cap, environmental inspection, capacity elimination, and steel price (the full control set defined in **Table 1**). Column (a) adds a $\text{Pilot_ETS} \times \text{Intensity_Target}$ interaction to Equation (1) on the full panel; the small and insignificant interaction ($\beta_3 = -0.011$, $p = 0.55$) indicates that the joint effect of the two policy channels is statistically indistinguishable from the sum of the two main effects. Column (b) drops the 19 doubly-treated firms (285 firm-years) and re-estimates Equation (1) on the remaining single-channel sample. Both specifications recover the sign, magnitude, and approximate significance of the main coefficients reported in **Table 3**, supporting the interpretation of β_1 and β_2 as separately identified policy effects.

A.4. Administrative-Document Sources for Treatment and Control Coding

The following primary regulatory documents were consulted to code the treatment indicators and concurrent-policy controls. Each is publicly available through the issuing agency's official portal.

[A1] National Development and Reform Commission (NDRC), Notice on Pilot Work for Carbon Emissions Trading (Fagai Bangong [2011] No. 2601), 29 October 2011. Defines the seven pilot ETS jurisdictions and inaugural launch parameters.

[A2] State Council of the PRC, Guiding Opinions on Resolving Severe Excess Capacity (Guofa [2013] No. 41), 6 October 2013. Establishes the framework for steel-sector capacity elimination underlying the Capacity_Elim and Output_Cap controls.

[A3] State Council of the PRC, Action Plan for Energy Saving and Carbon Reduction 2024-2025 (Guofa [2024] No. 12), 23 May 2024. Specifies the 3.9% economy-wide intensity reduction target and steel-sector decarbonization directives referenced in §1.

[A4] National Development and Reform Commission (NDRC), 14th Five-Year Plan for Energy Saving and Emission Reduction (Fagai Huanzi [2022] No. 33), January 2022. Source for binding intensity-target enterprise lists post-2021.

[A5] Ministry of Industry and Information Technology (MIIT), Steel Industry Standard Conditions (multiple vintages: 2015, 2018, 2020, 2023). Used to identify steel firms and the location of their crude-steel capacity for Pilot_ETS coding.

[A6] National Development and Reform Commission (NDRC), Implementation Scheme for the Top-10,000 Energy-Consuming Enterprises Program (Fagai Huanzi [2011] No. 2873), 7 December 2011. Defines the "Top 10,000" enterprises subject to binding energy/carbon intensity targets, used in source (a) of the Intensity_Target coding.

[A7] Ministry of Ecology and Environment (MEE), Carbon Emissions Trading Allowance Allocation Plan for the Iron and Steel Sector (2024-2025) (Huanban Qihou [2025] No. 12), November 2025. Source for the 2025 expansion of the national ETS to include steel referenced in §1 and §5.1.

[A8] Central Committee of the Communist Party of China, Notice on the Establishment of the Central Environmental Protection Inspection System (2015), and successive inspection-round notices (2016, 2017, 2018, 2019, 2021, 2023). Source for Env_Inspection coding.