Proceedings of ICEMGD 2026 Symposium: The Role of Blue Economy in Promoting Human Sustainable Development
DOI: 10.54254/2754-1169/2026.GT35647

# *Carbon Emissions Trading and Brown Divestment: Evidence on the Green Reallocation of Capital from China's Mutual Funds*

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***Abstract.*** Taking China's regional carbon emissions trading pilots as a quasi-natural experiment, we identify how the carbon market guides public mutual funds to reallocate capital. Based on a multi-period difference-in-differences (DID) analysis of A-share listed firms from 2008 to 2020, the paper finds that the policy effect is almost entirely concentrated in high-polluting firms. After the launch of carbon pilots, the long-term holdings of brown firms by public funds decreased by approximately 0.38 percentage points relative to clean firms, nearly one third of the mean value, while holdings of clean firms remained unchanged. The results are robust under stacked DID, Oster's bounds, and multi-level inference, whereas the average effect is fragile under conservative inference. In terms of mechanisms, the policy enables carbon risk to be recognized and priced through increased analyst coverage, and imposes compliance costs precisely on brown firms. The risk channel follows the same direction but has a limited magnitude. Divestment mainly takes the form of complete fund exit, and is independent of firm size. The debt financing cost of high-polluting firms rises relatively due to divestment, yet these firms increase investment and reduce emissions after the policy. This indicates that the phenomenon reflects repricing in the capital market, and the real capital of brown firms has not been drained. We find no evidence of active increase in holdings of green firms, and the green reallocation of capital takes the form of one-sided brown divestment.

***Keywords:*** carbon emissions trading, public mutual fund holdings, brown divestment, green reallocation of capital, multi-period difference-in-differences

## 1. Introduction

Peak carbon dioxide emissions and carbon neutrality are China's long-term national strategies, and carbon emissions trading is a core market-based tool to achieve this goal. Since 2013, China has launched regional carbon emissions trading pilots in provinces and municipalities including Beijing, Tianjin, Shanghai, Guangdong, Hubei, Chongqing, and Fujian, and established the national carbon emissions trading market in 2021. The core mechanism of the carbon market is to price emissions, internalizing the externality of corporate carbon emissions into measurable costs and constraints. A natural question is how carbon price signals transmit from the real economy to the capital market, and whether they will change investors' valuation and holdings of different firms, thereby affecting the flow of capital between green and brown assets. The capital market is the hub of resource allocation. The repricing of carbon risk by institutional investors may change capital flows earlier and more directly than firms' own adjustments. However, compared with the real effects of carbon trading on firms' emission reduction and innovation, direct evidence on this capital market transmission link is still lacking.

We answer this question from the perspective of public mutual fund holdings. Using China's three batches of regional carbon trading pilots, this paper finds that public funds systematically reduce their long-term holdings of high-polluting firms after the policy implementation, with a magnitude of about 13% of the sample mean. This reduction is almost entirely concentrated in brown firms, while holdings of clean firms remain unchanged. Carbon price signals do penetrate into the capital market; they change the distribution of capital between brown and clean firms, while the total amount of capital remains basically unchanged.

We examine whether and how carbon emissions trading guides public mutual funds to reallocate capital. For identification, this paper adopts a multi-period difference-in-differences design based on three batches of regional pilots launched in 2013 (Beijing, Tianjin, Shanghai, Guangdong), 2014 (Hubei, Chongqing), and 2016 (Fujian). A-share listed firms registered in pilot provinces are taken as the treatment group, and the sample covers 2008 to 2020. The sample ends in 2020 because the launch of the national carbon emissions trading market in July 2021 would affect all firms and undermine the purity of the control group. Our core identification statistic is the interaction term between the policy and firms' brown exposure, which isolates the difference in policy effects between brown and clean firms. The average treatment effect of the policy would average out this difference.

The main results are as follows. On average, long-term public fund holdings of treatment group firms decrease by approximately 0.14 percentage points after the pilot, equivalent to 13% of the mean (firm-level clustering $p = 0.039$). However, under more conservative inference methods such as provincial-level clustering and wild cluster bootstrap, the average effect is not robust. Therefore, this paper only takes it as an introduction and focuses on the interaction term. The interaction coefficient between the policy and brown exposure is −0.0038, which is significant in ten out of eleven identification, measurement, and inference specifications. It is −0.0035 under the clean stacked 2×2 design ($p = 0.003$), Oster's [1] selection bound gives $\delta = 0.81$, and the empirical p-value of the firm-level permutation test is 0.004. The policy effect for clean firms is almost zero. The reduction is targeted brown divestment: the policy only reduces holdings of brown firms, and does not comprehensively shrink holdings of all pilot firms.

Further, divestment mainly takes the form of complete fund exit. In the within-fund panel that only includes positive holdings, remaining holders do not reduce their positions; instead, they increase their holdings relatively. This indicates that the reduction is achieved by some funds clearing their brown positions. In terms of mechanisms, the policy makes carbon risk explicit and priced. Analyst coverage increases and optimistic bias decreases, while the pressure of compliance costs falls precisely on high-polluting firms, whose operating cost ratio rises by approximately 3.4 percentage points relatively. Information serves as the transmission channel, cost is the main driver, and the risk channel has a limited magnitude. High-polluting firms instead increase investment, expand fixed assets, and reduce emissions after the policy, yet they still face withdrawal of long-term capital. This shows that divestment is portfolio repricing of carbon risk in the capital market, and the real capital of brown firms has not been drained. We also find no active increase in holdings of green firms; green capital reallocation thus takes the form of one-sided brown divestment.

DOI: 10.54254/2754-1169/2026.GT35647

This study speaks to three strands of literature. The first is climate finance and the pricing of carbon risk: investors demand a return premium on pollution- and carbon-intensive firms [2-4], carbon tail risk is priced in the options market [5], and climate concerns have entered investor attention, hedging strategies, and equilibrium portfolio choice [6-9]. The second is responsible investing and the green preferences of institutional investors, who underweight controversial assets [10], move money toward sustainability [11], and transmit environmental preferences to their portfolio firms [12, 13]. The third is the evaluation of carbon emissions trading, which has concentrated on real outcomes such as low-carbon innovation [14]; the divestment theory of Heinkel et al. [15] predicts that exclusionary investment raises brown firms' cost of capital but has lacked direct holdings-level evidence. Relative to these strands, we provide causal, holdings-level evidence that an exogenous carbon price shock turns green preference into directed brown divestment by long-horizon investors.

## 2. Research design

### 2.1. Sample selection and data sources

This paper takes China's A-share listed firms from 2008 to 2020 as the research sample. The year 2008 is chosen as the starting point to leave a sufficiently long pre-treatment window before the launch of the three batches of pilots to test the parallel trend. The year 2020 is chosen as the end point because the launch of the national carbon emissions trading market in July 2021 would affect both the treatment and control groups simultaneously and undermine the purity of the control group. The treatment status of carbon emissions trading pilots is generated by matching the province where the firm is registered with the pilot list. Firms registered in Beijing, Tianjin, Shanghai, Guangdong (2013), Hubei, Chongqing (2014), and Fujian (2016) are classified as the treatment group, while firms in other provinces are the control group. The policy time point Post is defined according to the launch year of each province's pilot.[1]

The data come from CSMAR and CNRDS. The sample consists of non-financial, non-ST A-share firms from 2008 to 2020, with a final total of 32,002 observations. Definitions of main variables are shown in Table 1.

### 2.2. Variable definition and measurement

The dependent variable is the long-term public fund holding ratio (long_hold), that is, the proportion of shares held by long-term public mutual fund investors in a firm, with a value ranging from 0 to 1. Long-term and short-term public fund investors are classified by shareholding turnover rate. Long-term investors correspond to patient capital with low turnover and long holding periods. The practice of defining investor horizon by turnover rate has been established in research on institutional investor horizon [16]. We take long-term public fund holdings as the main dependent variable precisely because structural, long-cycle attributes such as carbon risk can only be

[1] In 2011, the National Development and Reform Commission approved the launch of carbon emissions trading pilots in Beijing, Tianjin, Shanghai, Chongqing, Guangdong, Hubei, and Shenzhen. The release of emission control lists and actual market opening of each pilot were concentrated between 2013 and 2014. Fujian was launched as an additional pilot in 2016. This paper groups Guangdong and Shenzhen as a single treatment unit, defines Post according to the actual launch year of each province, and compiles the pilot list based on public information of each pilot.

DOI: 10.54254/2754-1169/2026.GT35647

incorporated into valuation by capital with a long-term perspective, and long-term capital has the most identifiable response to it. Its mean is about 0.0103 and median is 0.0012, showing obvious right skewness, indicating that most firms have low long-term institutional holdings.

The core explanatory variable is the interaction term between the carbon trading policy and firm brown exposure, DID×HighPoll. Among them, DID is the multi-period difference-in-differences term, which equals 1 when the province where the firm is registered is a pilot province (Treat = 1) and the year is no earlier than the launch year of the province's pilot (Post = 1). HighPoll is a dummy variable for brown exposure, defined based on whether the firm's previous pollution emission equivalent is not lower than the sample median. A value of 1 indicates a high-polluting (brown) firm. The rationale for measuring brown exposure by pollution emissions is that firms with intensive pollution and carbon emissions bear higher environmental externalities, transition and regulatory risks, and have been systematically priced by the capital market: firms with heavier pollution are required to have higher expected returns [2], and returns of firms with higher carbon emissions also contain a significant carbon premium [3]. Brownness is defined by pollution equivalent before the pilot launch, which not only fits the targeted constraint of carbon trading on high-emission firms, but also avoids reverse definition of treatment intensity by post-policy emissions. The median dummy variable is adopted to clearly depict the difference between the clean and brown ends. To avoid measurement dependence, we also use specifications such as the top and bottom terciles of pollution equivalent, air pollution equivalent only, and continuous standardized exposure for robustness tests (see Section 4).

The selection of control variables refers to existing literature on factors influencing institutional holdings, covering financial and governance characteristics that may simultaneously affect firms' carbon exposure and institutional holdings. Definitions of main variables are as follows. The dependent variable is long-term public fund holding ratio long_hold (value 0–1), representing long-term institutional investor holdings. In robustness tests, it is replaced by short-term holding ratio short_hold, stable and trading institutional holding ratios stable_hold and trade_hold. Core explanatory variables include the multi-period DID term DID (Treat×Post, constructed according to the launch year of each province's pilot) and the brown exposure dummy HighPoll (equals 1 if the firm's pre-pilot pollution equivalent is not lower than the sample median). Control variables cover firm size Size (logarithm of total assets), asset-liability ratio Lev, return on total assets ROA, operating cash flow Cashflow, operating revenue growth rate Growth, Tobin's Q TobinQ, board size Board (logarithm), independent director ratio Indep, CEO-chair duality Dual, shareholding ratio of the largest shareholder Top1, ownership nature SOE (equals 1 for state-owned enterprises), and firm age FirmAge (logarithm). In addition, VAR_ADJ is adjusted stock price volatility (idiosyncratic risk), used as a baseline control variable. BigFirm is a dummy variable for firm size above the median, used for size heterogeneity grouping. Overall institutional holdings are highly correlated with the dependent variable and pose an over-control problem, so they are not included in the baseline control set.

Table 1. Variable definitions

| Variable | Definition |
|---|---|
| long_hold | Proportion of shares held by long-term public mutual funds (0–1) |
| DID | Treat × Post; equals 1 for firms registered in a pilot province from the pilot launch year onward |

DOI: 10.54254/2754-1169/2026.GT35647

Table 1. (continued)

| | |
|---|---|
| HighPoll | Equals 1 if the firm's pre-pilot pollution emission equivalent is not lower than the sample median |
| Size / Lev / ROA | Logarithm of total assets / asset-liability ratio / return on total assets |
| Other controls | Cashflow, Growth, TobinQ, Board (log), Indep, Dual, Top1, SOE, FirmAge (log) |
| Analyst coverage | Number of analysts following the firm (logarithm) |
| Number of research reports | Number of research reports in the current year (logarithm) |
| Media coverage / negative media coverage | Total media reports / negative reports |
| Institutional site visits | Number of on-site investigations by institutional investors |
| Forecast dispersion / optimistic bias / forecast error | Standard deviation of analysts' earnings forecasts / degree of optimism / absolute error |
| Total risk / idiosyncratic risk | Total stock price volatility / idiosyncratic volatility adjusted by market model |
| Operating cost ratio | Operating cost / operating revenue |
| Investment expenditure / fixed assets | Capital expenditure / total assets, fixed assets / total assets |
| Total pollution equivalent / air pollution equivalent | Firm's pollution emission equivalent, air pollution equivalent (logarithm) |
| Green patent applications / green patent grants / green patent share | Number of green patent applications / grants / proportion of total patents |
| Debt cost | Interest expense / interest-bearing liabilities |
| Carbon price | Annual volume-weighted average transaction price of the carbon market in the pilot province where the firm is located (100 yuan/ton) |

Note: Continuous variables are winsorized at the top and bottom 1% in the final sample. SOE is automatically omitted due to collinearity with firm fixed effects. Mechanism, further-analysis, and heterogeneity variables are only used in their corresponding tests. Idiosyncratic risk (VAR_ADJ) serves both as a baseline control variable and as a dependent variable in the risk channel.

## 2.3. Model specification

To identify the average impact of the carbon trading policy on long-term public fund holdings, we first set up the following two-way fixed effects difference-in-differences model:

$$long_hold_{i,t} = \alpha + \beta, DID_{i,t} + \gamma, Controls_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \tag{1}$$

where $i$ and $t$ denote firm and year, respectively. $\mu_i$ is the firm fixed effect, absorbing time-invariant firm characteristics. $\lambda_t$ is the year fixed effect, controlling for common time shocks to all firms. $\varepsilon$ is the random disturbance term. The coefficient $\beta$ measures the average effect of the policy on long-term holdings of treatment group firms. Considering there are only seven pilot provinces, standard errors are clustered at the firm level as the baseline. In robustness tests, we also report provincial-level clustering, two-way clustering at firm and year levels, wild cluster bootstrap with Webb weights [17], and permutation tests.

Our core identification statistic is the difference in policy effects between brown and clean firms; the average policy effect would average out this difference. For this purpose, we introduce the interaction term of brown exposure:

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$$long_hold_{i,t} = \alpha + \beta, DID_{i,t} + \delta, DID_{i,t} \times HighPoll_i + \gamma, Controls_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \tag{2}$$

The coefficient $\delta$ is the focus of this paper. If $\delta$ is significantly negative, it indicates that the policy reduces holdings of high-polluting firms significantly more than clean firms, that is, capital withdraws from brown assets in a targeted manner. $\beta$ measures the policy effect on clean firms, which is expected to be close to zero. The main effect of HighPoll is absorbed by the firm fixed effect.

## 3. Empirical results

### 3.1. Descriptive statistics

Table 2 reports the descriptive statistics of main variables. The dependent variable, long-term public fund holding ratio (long_hold), has a mean of 0.0103, a median of only 0.0012, and a maximum of 0.1172. The distribution is obviously right-skewed, indicating that most listed firms have very low long-term institutional holdings, while a small number of firms have relatively concentrated holdings. The right skewness shows that the allocation of long-term public fund capital among firms is already highly uneven, and the carbon trading policy may further change this structure. The mean of the core explanatory variable DID is 0.296, meaning that about 30% of firm-year observations are in pilot provinces with the policy already implemented.

Brown exposure HighPoll is negatively correlated with firm size: only about 37.8% of large firms are classified as high-polluting. This indicates that brownness is not synonymous with large firms; the two capture different dimensions of firms. For this reason, the brown divestment effect identified later based on pollution exposure is a phenomenon independent of firm size, and is not equivalent to size heterogeneity.

Table 2. Descriptive statistics and group balance of main variables

| Variable | N | Mean | Std. Dev. | Min | Median | Max |
|---|---|---|---|---|---|---|
| long_hold | 32,002 | 0.0103 | 0.0201 | 0.0000 | 0.0012 | 0.1172 |
| short_hold | 32,002 | 0.0190 | 0.0379 | 0.0000 | 0.0021 | 0.2067 |
| DID | 32,002 | 0.296 | 0.456 | 0 | 0 | 1 |
| Size | 32,002 | 22.091 | 1.287 | 19.810 | 21.899 | 26.100 |
| Lev | 32,002 | 0.423 | 0.209 | 0.050 | 0.416 | 0.891 |
| ROA | 32,002 | 0.044 | 0.063 | −0.219 | 0.041 | 0.220 |
| Cashflow | 32,002 | 0.047 | 0.071 | −0.166 | 0.047 | 0.244 |
| Growth | 32,002 | 0.168 | 0.397 | −0.561 | 0.107 | 2.499 |
| TobinQ | 32,002 | 2.008 | 1.263 | 0.870 | 1.599 | 8.366 |
| Board | 32,002 | 2.136 | 0.200 | 1.609 | 2.197 | 2.708 |
| Indep | 32,002 | 37.443 | 5.315 | 33.330 | 33.330 | 57.140 |
| Dual | 32,002 | 0.267 | 0.442 | 0 | 0 | 1 |
| Top1 | 32,002 | 0.350 | 0.150 | 0.088 | 0.330 | 0.748 |
| SOE | 32,002 | 0.402 | 0.490 | 0 | 0 | 1 |
| FirmAge | 32,002 | 2.825 | 0.360 | 1.609 | 2.890 | 3.497 |

DOI: 10.54254/2754-1169/2026.GT35647

Table 2. (continued)

| | | | | | | |
|---|---|---|---|---|---|---|
| VAR_ADJ | 32,002 | 1.365 | 1.083 | 0.216 | 1.090 | 7.211 |

Note: Descriptive statistics for the full sample. HighPoll is the median dummy variable for brown exposure (mechanism sample, mean approximately 0.5).

### 3.2. Baseline regression: average policy effect

Estimating Equation (1), the average effect of the carbon trading pilots on long-term holdings is negative: the DID coefficient is −0.0010 with fixed effects only and strengthens to −0.0014 once the full control set is added, significant at the 5% level under firm-level clustering ($p = 0.039$). Economically, this corresponds to a decline of about 0.14 percentage points in long-term public fund holdings of treatment group firms, roughly 13% of the sample mean of 1.03%. That the estimate grows rather than shrinks as controls are added suggests omitted observables would, if anything, attenuate the effect. By contrast, short-term and trading-type institutional holdings show no significant response, so the adjustment is concentrated among long-horizon investors. However, the average effect is not robust under more conservative inference: with only seven treatment provinces, the province-level clustered p-value is 0.235 and the wild cluster bootstrap p-value is 0.396, indicating highly heterogeneous policy effects. We therefore take the average effect only as a point of entry, and the following sections focus on the differential responses between brown and clean firms.

### 3.3. Core result: targeted brown divestment

The core finding is that the policy effect is almost entirely concentrated in brown firms. Table 3 reports the results after introducing the brown exposure interaction term: the coefficient of the interaction term DID×HighPoll is −0.0038, significant at the 1% level ($p = 0.002$). The DID coefficient for clean firms is almost zero and insignificant ($p = 0.998$). This indicates that after the carbon trading pilot, long-term public fund holdings of high-polluting firms decrease by an additional 0.38 percentage points relative to clean firms, nearly one third of the mean of long-term holdings, while holdings of clean firms remain basically unchanged. The reduction in capital is therefore targeted brown divestment, and does not extend to all pilot firms. This result is consistent with the theoretical expectation of targeted brown divestment. Juxtaposing this with the fragile average effect clarifies the point: the policy precisely imposes reduction on brown firms, and does not uniformly reduce long-term holdings of all treatment group firms. What is diluted by the average effect is precisely this structural targeted reallocation.

Table 3. Brown divestment: decomposition of long-term holding effects by pollution exposure

| | (1) Interaction model | (2) High-emission group | (3) Low-emission group |
|---|---|---|---|
| DID | −0.0000 (0.0008) | −0.0013 (0.0012) | −0.0012 (0.0008) |
| DID×HighPoll | −0.0038*** (0.0012) | | |
| Controls / firm·year FE | Yes | Yes | Yes |
| N | 31,613 | 15,812 | 15,801 |
| Within $R^2$ | 0.0292 | 0.0264 | 0.0335 |

The credibility of the brown divestment interaction term is the focus of identification in this paper, and the possibility that it is driven solely by chance or sample characteristics needs to be

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ruled out. This paper adopts a firm-level true permutation test: treatment status is randomly reassigned across firms 500 times, and the interaction coefficient is re-estimated each time to construct the distribution of pseudo-coefficients. The results show that the true coefficient falls in the extreme tail of the pseudo-coefficient distribution, with an empirical p-value of 0.004 (Figure 1), strongly ruling out the possibility that brown divestment is driven by random factors.

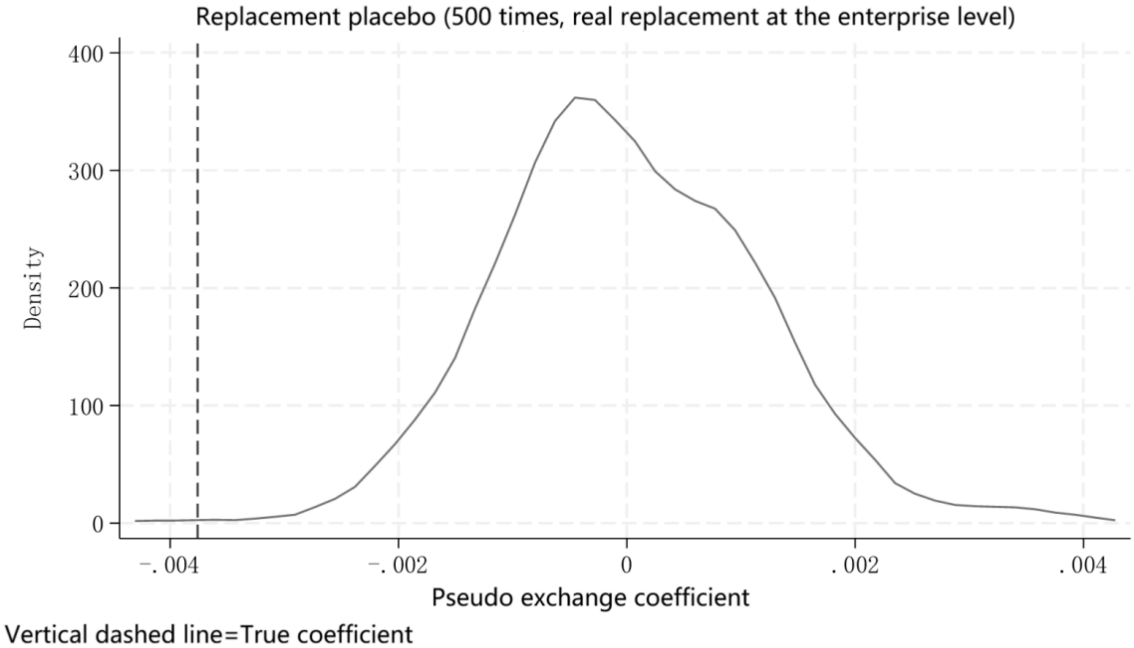


Figure 1. Firm-level permutation test for brown divestment interaction

## 3.4. Parallel trend test

For the DID estimator to yield causal estimates, long-term holdings of pilot and non-pilot firms must follow a common trajectory before the carbon market launches. If a pre-existing divergence already drove the two groups apart, the estimated policy effect would be confounded. We address this using an event study. Figure 2 shows that the coefficients for all pre-treatment periods are not significantly different from zero, indicating that there is no systematic trend difference in long-term holdings between the treatment and control groups before policy implementation, and the parallel trend assumption is satisfied. After policy implementation, the effect gradually deepens, showing a progressive dynamic. This is consistent with the intuition that institutional investors' recognition of carbon risk and position adjustment behavior take time to transmit: the construction and improvement of the carbon market cannot be accomplished overnight, and it is also a gradual process for investors to identify carbon risk, revalue, and adjust their holdings.

DOI: 10.54254/2754-1169/2026.GT35647

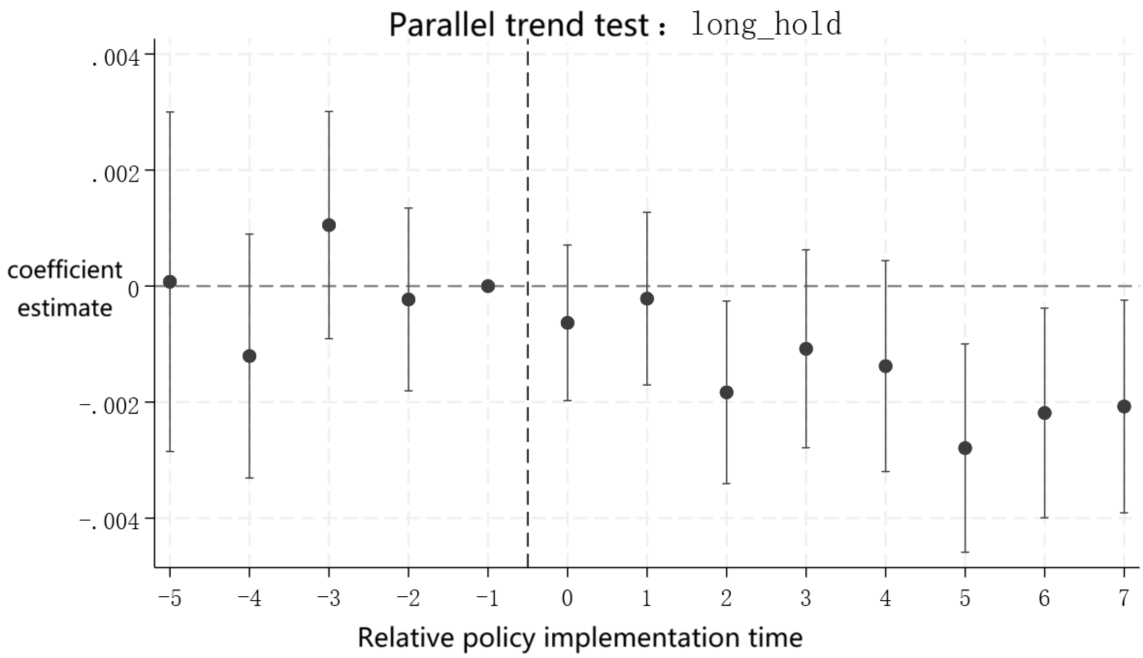


Figure 2. Parallel trend of long-term holdings (event study)

### 3.5. Robustness and identification strengthening

The brown divestment interaction is robust across three dimensions: identification, measurement, and inference.

Because staggered adoption can bias conventional two-way fixed-effects estimates under heterogeneous treatment effects [18], we implement the stacked DID design of Cengiz et al. [19]. For each batch of pilots, we match a group of pure control firms that have never been affected by the policy, and use clean 2×2 comparisons to avoid the bad comparison bias caused by already-treated groups serving as controls. Under the stacked design, the brown divestment coefficient is −0.0035 ($p = 0.003$), almost identical to the baseline estimate. The average term after stacking remains insignificant, once again confirming the contrast that the average effect is fragile while the interaction term is robust. We also use Oster's (2019) method to assess selection on unobservable factors, obtaining $\delta = 0.81$. This means that to explain the interaction coefficient as zero, selection on unobservable factors would need to be about 0.81 times that of observable control variables, slightly lower than the commonly used reference value of 1. It is relatively robust but not absolute, and we report this result as obtained. The dynamic effects given by the cohort-time average treatment effect estimator of Callaway and Sant'Anna [20] are also consistent with the above direction (Figure 4). The DID coefficients and their confidence intervals from six robustness tests are summarized in Figure 3, all falling below the zero line.

DOI: 10.54254/2754-1169/2026.GT35647

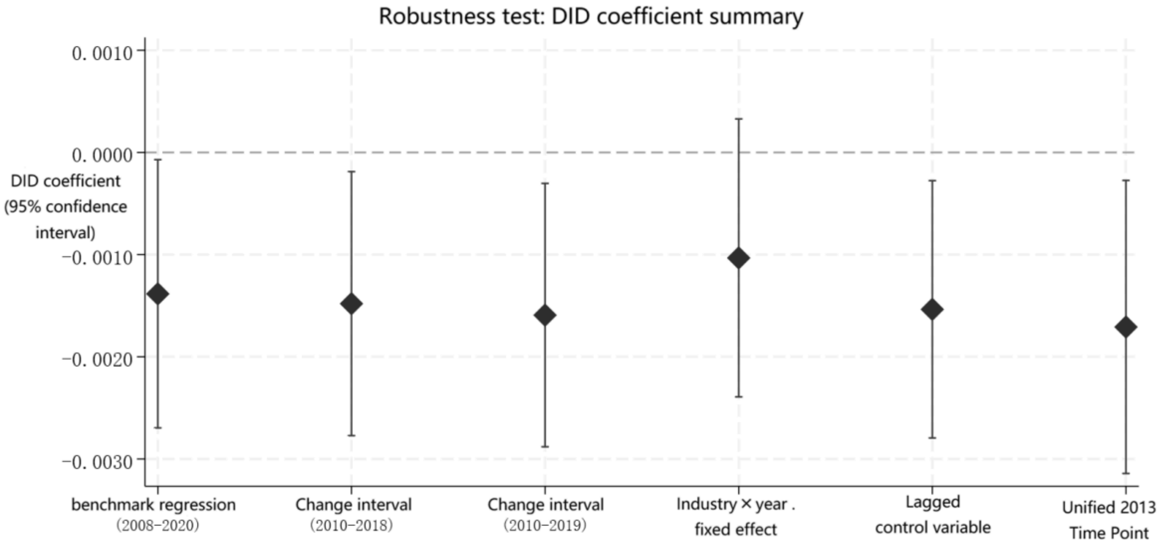


Figure 3. Summary of robustness DID coefficients

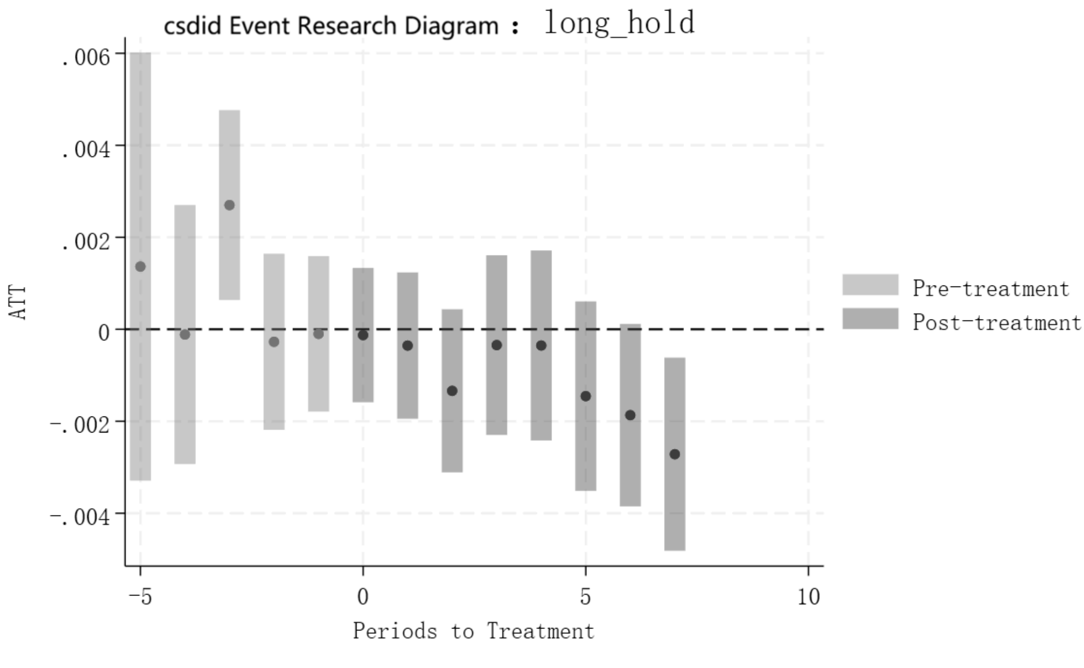


Figure 4. Callaway–Sant'Anna dynamic effects

In terms of measurement, the definition of brown exposure does not rely on a single specification. This paper replaces it with specifications such as the top and bottom terciles of pollution equivalent, air pollution equivalent only, and continuous standardized exposure. The interaction coefficients are stable between −0.0035 and −0.0043 and all are significant (Table 4). If a high-carbon definition based on industry labels is used instead, the interaction term becomes insignificant (+0.0008). This contrast shows that funds identify brown assets based on firms' own emission intensity, and general industry labels do not work: within the same industry, firms with different emission levels receive different treatment. This also echoes the phenomenon that ESG ratings are highly divergent and

DOI: 10.54254/2754-1169/2026.GT35647

inconsistent for the same firm. In comparison, firms' own measured pollution exposure is a cleaner brown measure less likely to be contaminated by specification noise. The brownness defined in this paper refers to firms' overall pollution-environmental exposure, not limited to narrow carbon emission intensity. Among Chinese listed firms, high-carbon and high-polluting firms are not the same group, and fund withdrawal follows the latter more closely.[2]

Table 4. Brown divestment: robustness of pollution exposure measurement

| | (1) Baseline median | (2) Top-bottom tercile | (3) Air only | (4) Continuous exposure | (5) Industry definition |
|---|---|---|---|---|---|
| DID×Brown | −0.0038*** (0.0012) | −0.0042** (0.0017) | −0.0043*** (0.0012) | −0.0035*** (0.0011) | 0.0008 (0.0015) |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Firm / year FE | Yes | Yes | Yes | Yes | Yes |
| N | 31,613 | 21,076 | 31,613 | 31,613 | 31,613 |

Note: Column (1) median dummy variable, (2) top and bottom terciles, (3) air pollution only, (4) continuous standardized, (5) industry high-carbon definition.

In terms of inference, this paper reports results from all levels of inference side by side, without selective reporting. Table 5 summarizes the results under different clustering and methods: firm-level clustering p = 0.002, provincial-level clustering p = 0.004, two-way firm and year clustering p = 0.015, and p = 0.001 after controlling for low-carbon city pilots. For inference problems caused by too few treatment clusters, we also report two more conservative tests: the empirical p-value of provincial permutation is 0.352, and the p-value of wild cluster bootstrap with Webb weights for seven treatment provinces is 0.059, still above the 10% threshold. Across eleven specifications in the three dimensions of identification, measurement, and inference, the brown divestment interaction term is significant in ten. The only insignificant result stems from the power issue that the treatment group loses more than half after excluding low-carbon cities. Other tests have consistent directions and are not listed one by one due to space limitations, including: (i) shortening the sample interval to exclude extreme years such as the financial crisis and COVID-19; (ii) lagging control variables by one period to alleviate reverse causality; (iii) changing the construction method of DID; (iv) using the number of shares held as the dependent variable; (v) propensity score matching; (vi) excluding neighboring provinces of pilots to rule out spillovers; (vii) testing expectation effects using the policy announcement year; (viii) excluding firms with registered address relocation. Under all the above specifications, the conclusion of brown divestment remains robust.

[2] We also attempt to directly measure brownness by firms' carbon emission intensity (listed firms' carbon performance data, voluntarily disclosed, covering about three quarters of the sample). Defining high-carbon firms as those with above-median pre-period mean, the resulting interaction term has the same sign as the baseline but is insignificant (−0.0016), and carbon emission intensity is almost uncorrelated with the pollution equivalent exposure in this sample (correlation coefficient approximately zero).

DOI: 10.54254/2754-1169/2026.GT35647

Table 5. Inference robustness: clustering levels and wild cluster bootstrap

| | (1) Firm clustering | (2) Provincial clustering | (3) Two-way clustering |
|---|---|---|---|
| DID×HighPoll | −0.0038*** (0.0012) | −0.0038*** (0.0012) | −0.0038** (0.0013) |
| Wild bootstrap p | | 0.059 | |
| Provincial permutation p | | 0.352 | |
| Controls | Yes | Yes | Yes |
| Firm / year FE | Yes | Yes | Yes |
| N | 31,613 | 31,613 | 31,613 |

Note: Columns (1)–(3) are inferences from the same interaction regression under firm, provincial, and two-way firm-year clustering, respectively. Wild bootstrap uses Webb weights with 9999 replications (31 provincial clusters). Provincial permutation is the empirical p-value from 500 random reassignments of treatment provinces.

## 4. Mechanism tests

After confirming that capital withdraws from brown firms in a targeted manner, a natural question arises: why do funds divest? This section answers this question from three channels: information, cost, and risk. Mechanism tests mainly focus on the reduced-form effects of the policy on channel variables, that is, testing whether the carbon trading pilot significantly changes a certain channel variable, and verifying it with the heterogeneity of the channel between brown and clean firms. To avoid the bad control problem, when testing a certain channel, this paper excludes the corresponding channel variable from the control set. The three channels show that brown divestment is dominated by information recognition and cost drivers, while risk plays a limited role. The policy turns brown exposure from ignored to priced. Compliance costs imposed precisely on brown firms are the main driver of divestment, while the magnitude of risk increase is insufficient and does not dominate the distribution. Divestment concentrates in brown firms precisely because cost and green screening target brownness. The above conclusions are based on reduced-form effects and inter-group heterogeneity identification, and do not rely on average mediation — under heterogeneous treatment effects, this approach identifies the underlying mechanisms more credibly than conventional mediation analysis.

### 4.1. Information channel

The policy first changes the information environment. Before the carbon pilot, carbon emissions were an attribute ignored by the market. After the pilot, compliance costs became a substantive determinant of cash flow, and carbon risk entered investors' awareness and was reflected in pricing and holdings. Table 6 shows that the logarithm of the number of analysts following treatment group firms increases by 0.061, and the logarithm of the number of research reports increases by 0.082, both significant at the 5% level. Table 7 shows that analysts' optimistic bias decreases significantly by 0.0018, and judgments become more prudent.

DOI: 10.54254/2754-1169/2026.GT35647

Table 6. Information channel: impact of carbon trading policy on market attention

| | (1) Analyst coverage | (2) Number of research reports | (3) Media coverage | (4) Negative media | (5) Institutional site visits |
|---|---|---|---|---|---|
| DID | 0.061** (0.027) | 0.082** (0.033) | 0.005 (0.020) | −0.001 (0.022) | −0.041 (0.040) |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Firm / year FE | Yes | Yes | Yes | Yes | Yes |
| N | 31,613 | 31,613 | 31,613 | 31,613 | 25,105 |
| Within $R^2$ | 0.200 | 0.207 | 0.122 | 0.098 | 0.062 |

Note: Dependent variables are various information channel variables. Media coverage and institutional site visits are insignificant.

Table 7. Information channel: analyst forecast quality

| | (1) Forecast dispersion | (2) Optimistic bias | (3) Forecast error |
|---|---|---|---|
| DID | −0.0002 (0.0003) | −0.0018** (0.0007) | −0.0001 (0.0006) |
| Controls | Yes | Yes | Yes |
| Firm / year FE | Yes | Yes | Yes |
| N | 17,094 | 20,443 | 20,443 |

Analyst coverage jumps in the year of policy implementation without gradual increase, which is worth exploring in itself. It shows that the capital market's revaluation of carbon risk does not lag behind the policy. The market does not need to wait until firms' carbon costs are fully reflected in financial statements before starting to revalue these firms. This contrasts with adjustments at the real level: firms' emission reduction investments and capacity adjustments often take years to complete, while the capital market reacts as soon as the policy is implemented. This immediacy of the information channel is the micro-foundation for the capital market to guide resource allocation ahead of the real economy, and also echoes our motivation for studying capital market transmission in its own right.

The information channel is important because it is the prerequisite for the other two channels to take effect. Changes in costs and risks can only be reflected in funds' holding decisions after being recognized and priced by the market. This is consistent with the findings of Servaes and Tamayo [21, 22] that non-financial attributes of firms only translate into value consequences when noticed by stakeholders. The increase in analyst coverage is precisely the process of brown exposure shifting from being ignored to being priced. Existing research also shows that the market reacts to information related to corporate social responsibility in terms of pricing, although there is sometimes underreaction to intangible attributes [23]. Therefore, the information channel here is only a conduit and does not constitute an independent preference shock in itself. Media coverage, negative media reports, and institutional site visits are all insignificant (see Table 6). This contrast shows that the pricing of carbon risk relies more on professional analysts' systematic tracking of firms' fundamentals; temporary hype in public opinion or changes in visit frequency have limited effects. The former directly changes valuation, while the latter does not necessarily. These null results are reported in Table 6 for completeness and are not used as arguments.

Analyst coverage is an appropriate proxy because it corresponds to the degree to which brown exposure is recognized and priced in the theoretical framework. In our model, brown exposure can

DOI: 10.54254/2754-1169/2026.GT35647

affect holdings only if it is seen by the market. Analysts' tracking and forecasting of firms are the observable manifestation of this salience. The decline in optimistic bias is particularly telling: it indicates that analysts have substantially lowered their judgments on the prospects of brown firms, not simply increasing the number of reports. This convergence of judgments is a sign that carbon risk is priced and the negative implications of brown exposure are absorbed by the market. Increased attention improves information quality: the market sees brown firms more clearly and more prudently.

## 4.2. Cost channel

The cost channel determines whether brown assets are worth holding. The carbon pilot requires emission-controlled firms to pay for emissions exceeding their quotas, and cost pressure falls precisely on emission-intensive brown firms. Table 8 shows that the policy reduces the average operating cost ratio by 0.013, reflecting overall efficiency improvement in pilot regions. However, the interaction term reveals differentiation: the cost ratio of clean firms decreases by 0.026, while that of brown firms rises relatively by about 3.4 percentage points, with the cost burden shifting toward brown firms.

Table 8. Risk and cost channels

| | (1) Idiosyncratic risk | (2) Total risk | (3) Operating cost ratio | (4) Cost ratio (interaction) |
|---|---|---|---|---|
| DID | 0.056** (0.023) | 0.102*** (0.025) | −0.013** (0.005) | −0.026*** (0.007) |
| DID×HighPoll | | | | 0.034*** (0.009) |
| Controls | Yes | Yes | Yes | Yes |
| Firm / year FE | Yes | Yes | Yes | Yes |
| N | 31,613 | 31,613 | 30,544 | 30,544 |
| Within $R^2$ | 0.021 | 0.018 | 0.041 | 0.042 |

To more rigorously characterize how the cost channel transmits to holdings, we further adopt a moderated mediation framework, using pollution exposure as the moderating variable, and estimate its confidence interval with 500 bootstrap replications of firm-level cluster resampling. The results show that the moderated mediation index is −0.000156 ($z = -2.92$, $p = 0.003$, bias-corrected 95% confidence interval [−0.000273, −0.000065], excluding zero), significant at the 1% level. In terms of decomposition, the conditional mediation on the clean firm side is significantly positive (0.000119, $p = 0.004$): the policy reduces costs for clean firms, which in turn supports holdings of them. The point estimate on the high-polluting firm side is negative but not significant alone. Therefore, the accurate statement is: the cost channel drives the relative reallocation of capital between clean and high-polluting firms, and the cost mediation on the high-polluting firm side alone does not hold independently. The dynamics of the cost channel are shown in Figure 5. Its trajectory is consistent with the above interaction effect, and the relative cost increase of high-polluting firms gradually emerges after the policy. This result supports the judgment that cost is the main driver of brown divestment.

DOI: 10.54254/2754-1169/2026.GT35647

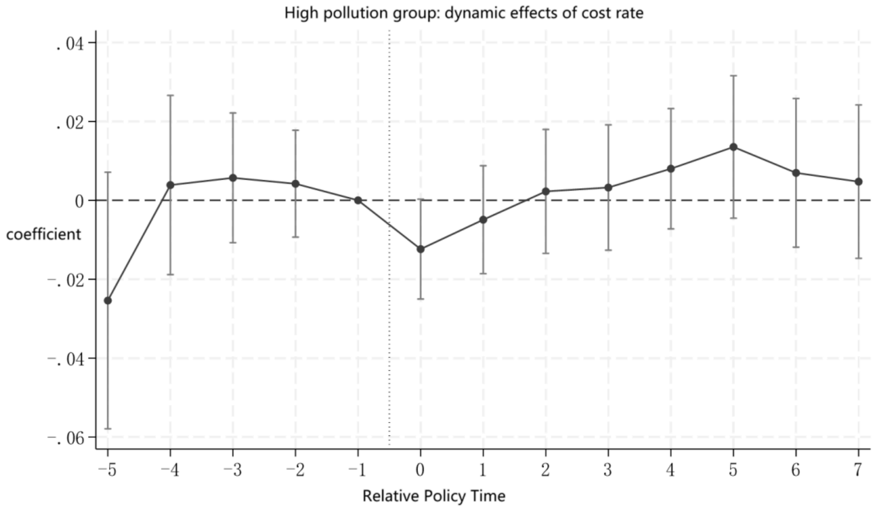


Figure 5. Dynamics of cost ratio in high-pollution group

This decomposition of moderated mediation also reveals the micro-level operation of brown divestment. The policy reduces costs for clean firms and increases costs for brown firms, so the cost channel generates conditional mediations in opposite directions at both ends: at the clean end, cost reduction supports holdings; at the brown end, cost increase suppresses holdings. Together, they form the cost basis for the relative reallocation of capital from brown to clean. The root of brown divestment is the asymmetric distribution of cost pressure between brown and clean firms. It is this asymmetry that drives the trade-off of capital between the two, and brown divestment does not occur independently on the brown firm side. This relative-reallocation perspective echoes our core narrative of green capital reallocation through brown divestment.

From the perspective of carbon market operation, this cost transmission has a clear institutional basis. The pilot sets annual quotas for emission-controlled firms. Quotas within the limit are free, and excess parts require payment. Firms with more emissions are more likely to face quota gaps. They either purchase quotas at market prices or invest additional funds in emission reduction, both of which directly erode profits. More importantly, this cost is structural and predictable, unlike one-time disturbances. This is why it can enter long-term investors' valuation framework and change their long-term holdings. Short-term, occasional cost fluctuations may not cause position adjustments by long-term capital, while compliance costs brought by carbon trading that accumulate continuously with emission intensity will be included in long-term capital's judgment of firms' long-term value. This logic is consistent with evidence that firms' environmental performance affects capital cost: firms with higher environmental externalities and weaker social responsibility often bear higher equity capital costs [24]. This also explains why the cost channel shows up so clearly for long-term holdings.

### 4.3. Risk channel

For the risk channel, the carbon pilot is accompanied by quota price fluctuations and compliance uncertainty, and brown firms also face stranded asset risks. Table 8 shows that the policy increases firms' idiosyncratic risk by 0.056 and total risk by 0.102, both significant at above the 5% level. However, the risk channel has a limited magnitude: the brown divestment effect remains almost unchanged after excluding idiosyncratic risk, indicating that the risk mediation effect is weak, and the distribution of divestment is characterized by brownness rather than risk level. Increased risk is more concentrated in small firms, while divestment is concentrated in large, brown firms. This apparent tension actually confirms that divestment is driven by cost and green screening — increased risk is a general consequence of the policy, while divestment is a selective response to brown exposure, and the two do not have to follow the same distribution. In the sample, carbon risk plays an indirect role mainly by raising compliance costs and depressing expected returns, and the contribution of simply amplifying uncertainty is limited. Conventional mediation tests exhibit suppression, so the reduced-form + heterogeneity approach is adopted, and the core conclusion is not affected.

## 5. Conclusions and policy recommendations

Evaluation of carbon trading policies has long focused on the real economy level. Taking regional carbon pilots as a quasi-natural experiment, we identify targeted divestment from high-polluting firms by public mutual funds based on multi-period difference-in-differences of A-share listed firms from 2008 to 2020, and answer the question of how carbon price signals change long-term capital allocation.

The research findings are as follows. First, changes in capital allocation are structural: the policy reduces long-term holdings of brown firms by an additional 0.38 percentage points relative to clean firms, nearly one third of the mean, and has no effect on clean firms. Brown divestment is robust under stacked DID and Oster's bounds, but the average effect is fragile under conservative inference. Second, in terms of mechanisms, the information channel enables brown exposure to be recognized and priced (increased analyst coverage, decreased optimistic bias), and the cost channel imposes compliance costs precisely on brown firms (operating cost ratio rises relatively by about 3.4 percentage points). The two constitute the main drivers, while the magnitude of risk increase is limited. Third, divestment mainly takes the form of complete fund exit, remaining holders do not reduce their positions, and the effect is independent of firm size. Fourth, reallocation is one-sided, and no active increase in holdings of green firms is found. Fifth, divestment is financial repricing rather than capital drainage: high-polluting firms instead increase investment and reduce emissions, and their debt cost rises relatively by about 0.71 percentage points, yet their transition investment is not crushed. The above findings depict a specific form of green capital reallocation: centered on withdrawal from brown assets, driven by fund exit and grounded in carbon cost pricing. Policy effects not only act on the real economy, but are also amplified through the capital market hub. Long-term capital completes reallocation ahead of the real economy, and the guiding role of the capital market in green transition is more direct than commonly believed.

Policy recommendations are as follows. First, unblock the financial transmission channel of the carbon market, and incorporate capital market reactions into policy evaluation design. Second, improve carbon information disclosure standards, enhance information comparability, and reduce investors' identification costs. Third, support transition with financial tools to avoid tightening financing conditions interrupting the transition process of high-emission firms. Fourth, cultivate patient capital and leverage the guiding role of long-term capital in green pricing and allocation.

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