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Suppression Without Spillovers? The Relative Effects of Carbon Trading Pilots and Platform-Host Cities on Green Investment in Guangdong

Biying Tang

Faculty of Finance, City University of Macau, Macao, China
F23090106691@cityu.edu.mo

Abstract. Against the backdrop of climate governance, carbon emissions trading markets have become an important market-based instrument for China to achieve its "dual carbon" goals, yet the direction of their impact on green investment remains contested, and direct evidence on spatial spillovers is lacking. Using city- aggregated data on listed companies' environmental investment in 12 prefecture-level cities in Guangdong Province from 2010 to 2020, this paper treats the Guangdong carbon trading pilot as a quasi- natural experiment with Guangzhou and Shenzhen as the treated group, and identifies effects using two- way fixed-effects multi-period DID and SLX models. We find that, relative to non-platform cities within the province, listed companies' environmental investment in Guangzhou and Shenzhen is significantly suppressed (did = -1.618, bootstrap $p < 0.001$); dynamically, the effect exhibits a pattern of "immediate shock — brief rebound — renewed negative in the medium-to-long run (marginally significant)." Across all three spatial weight matrices, no significant spatial spillover is detected, although this null result cannot rule out the existence of spillovers. Juxtaposed with existing evidence that carbon markets improve corporate ESG performance, our findings suggest that the policy may induce an ESG "word– deed decoupling." Owing to the province-wide coverage of the provincial carbon market and to disclosure constraints, the evidence in this paper is suggestive at the scale of the Pearl River Delta city cluster.

Keywords: carbon emissions trading, green investment, spatial spillover, difference-in-differences, SLX model

1. Introduction

As a core instrument of market-based environmental regulation, carbon emissions trading markets internalize environmental costs through the price signal of allowances; in 2021, China's national carbon market was officially launched [1]. However, expanding trading volume does not necessarily imply an improved allocation of abatement resources — whether carbon markets can incentivize firms to translate abatement commitments into substantive green investment is the central question for evaluating their policy effectiveness.

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Existing research has not reached a consistent conclusion on the direction of the carbon market's green- investment effect: there is evidence of significant promotion [2], reports of intensified symbolic compliance [3, 4], and theoretical models showing a "double-threshold" relationship between carbon prices and firms' optimal green investment [5]. In their framework, the threshold is bounded by the shadow price of emissions (marginal abatement cost): when the carbon price is below the shadow price, a rising carbon price raises firms' optimal green investment; above it, the opposite holds. Evidence on spatial spillovers is concentrated on carbon emissions and green innovation, with directions similarly unsettled [6-7]; as for green investment — the material basis of abatement — direct evidence on its spatial transmission is still lacking. This paper attempts to fill this gap.

Using the Guangdong carbon trading pilot as a quasi-natural experiment, and drawing on panel data for 12 prefecture-level cities from 2010 to 2020, this paper applies multi-period DID and SLX models to answer two questions: What is the direction of the carbon market's impact on local listed companies' environmental investment? Does this impact cross administrative boundaries and generate spatial spillovers?

We find that the carbon trading pilot exerts a significant relative suppression on local green investment (did = -1.618, bootstrap $p < 0.001$); dynamically, the effect follows a pattern of "immediate shock — brief rebound — renewed negative in the medium-to-long run (marginally significant)." No significant spatial spillover is detected under any of the three spatial weight matrices, in contrast to existing findings of significant spillovers in the dimensions of carbon emissions [6, 9] and green innovation [10]. The marginal contributions of this paper are: focusing on the Pearl River Delta city-cluster scale, reporting at the city level the empirical pattern of "local relative suppression without significant diffusion" of the carbon trading pilot's effect on green investment, and providing indirect spatial-dimension evidence for the possibility of an ESG "word–deed decoupling."

2. Institutional background, literature review, and research hypotheses

2.1. Institutional background

In 2011, the National Development and Reform Commission approved "two provinces and five cities" to pilot carbon emissions trading: Shenzhen, as an independent pilot city, launched trading first in June 2013; the Guangdong provincial carbon market launched in December of the same year, with the Guangzhou Carbon Emission Rights Exchange as the provincial trading platform. Its core mechanism is allowance allocation based on emissions-intensity benchmarks (the benchmarking method; the theoretical model of Xu et al. [11] likewise adopts such an intensity-based setting), making it a typical market- incentive environmental regulation [10]. It should be noted that the compliance coverage of the Guangdong provincial carbon market is not limited to Guangzhou but covers key emitting enterprises across the province; the implications for the interpretation of the treatment variable are discussed in Section 3.1. Cities in the Guangdong–Hong Kong–Macao Greater Bay Area are highly interconnected through industrial chains and factor flows, so examining whether carbon market policies generate cross-city spatial spillovers in this highly integrated region is of strong practical relevance.

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2.2. Literature review

2.2.1. Carbon markets and green investment: the direction debate

Li et al. [2], using A-share listed company data, find that the carbon trading pilot significantly promoted corporate green investment, supporting the optimistic "Porter Hypothesis" view. In contrast, several studies find that after joining carbon trading, regulated firms face intensified funding, risk, and competitive pressure, with ESG greenwashing rising significantly [3, 4, 12] — that is, firms may substitute symbolic compliance for substantive investment. A third perspective emphasizes the conditionality of policy effects: there is a "double-threshold" relationship between carbon prices and green investment [5], and policy effects exhibit significant heterogeneity [13]. In sum, whether carbon markets promote or crowd out green investment remains unsettled in the existing evidence, which constitutes one starting point for the empirical tests in this paper.

2.2.2. Spatial spillovers of environmental policy

Spatial spillovers of environmental policy follow two opposing paradigms — "befriending one's neighbors" versus "beggar-thy-neighbor" [14]. Spatial econometric methods have been widely used to characterize the cross-regional transmission of carbon emissions and their socioeconomic impacts [15], and their combination with quasi-experimental designs has likewise been widely applied to low-carbon policy evaluation [16, 17]. Existing evidence indicates that the spatial effects of environmental policy have clear distance-decay boundaries [19, 18], and that the direction of spillovers depends on whether "proximity" is defined as geographic or economic adjacency [20] — which establishes the methodological position of the SLX model and suggests that the choice of spatial weights may systematically affect estimation conclusions.

Focusing on carbon market pilots, spatial evidence is concentrated in the emissions and innovation dimensions with mixed directions: positive spillovers [6, 9, 10] and negative siphoning [8, 7] have both been reported; direct evidence on the spatial transmission of green investment is still lacking.

2.2.3. Carbon markets, ESG performance, and substantive green transition

The micro-level literature generally reports that carbon trading pilots improve corporate ESG performance [22, 23], but such "improvement" is concentrated mainly in ESG ratings and disclosure, and its correspondence with substantive green investment does not necessarily hold: several studies find that carbon trading instead intensifies corporate ESG greenwashing, and ESG rating disagreement implies that ESG signals themselves may contain noise [25, 24]. This possibility of "word–deed decoupling" offers a transmission path for spatial spillovers in which "words may cross boundaries while deeds need not," whose actual direction awaits empirical testing.

2.3. Theoretical analysis and research hypotheses

Building on the above literature, this paper develops hypotheses at two levels: local effects and spatial effects. Regarding local effects, the cost-squeeze mechanism points toward suppression: first, compliance costs such as allowance surrender, monitoring and verification, and compliance reporting are immediate and certain; under financing constraints, green investment — with its long payback period and high deferability — is often the first to be crowded out; second, carbon prices ran persistently low during the pilot stage, so the price signal was insufficient to form effective

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incentives [5]; third, firms have strategic room to substitute symbolic compliance for substantive investment [3, 4]. Accordingly:

H1 (Local Relative Suppression Hypothesis): Relative to non-platform cities within the province, listed companies' environmental investment in the carbon-trading-platform host cities (Guangzhou and Shenzhen) exhibits a significant negative relative effect.

Regarding spatial effects, local suppression may remain primarily an intra-pilot-city effect: first, green investment is attached to physical plants and does not easily transmit across boundaries; second, pre-tests show no significant spatial autocorrelation in sample cities' green investment (Moran's I = -0.015, p =0.233), lacking the output basis for spatial transmission; third, the pilot policy takes registration-location enterprises as the compliance unit and imposes no direct compliance constraint on firms in non-pilot cities [8]; fourth, what ESG demonstration can transmit across boundaries is mainly disclosure behavior rather than physical investment. Accordingly:

H2 (Spatial Spillover Absence Hypothesis): The impact of the carbon trading pilot on listed companies' environmental investment is mainly manifested as a local effect, without forming significant spatial spillovers.

3. Research design

3.1. Sample and data

The study period is 2010–2020, with Guangzhou and Shenzhen as the treated group (Shenzhen as an independent pilot city and Guangzhou as the host city of the provincial trading platform) and the other prefecture-level cities in the province with available data as the control group. It should be noted that the compliance coverage of the Guangdong provincial carbon market is not limited to Guangzhou; the control group therefore more accurately represents cities that neither established an independent city-level pilot nor host a trading platform, rather than cities entirely unaffected by carbon markets. The final sample is an unbalanced panel of 12 prefecture-level cities in Guangdong Province (49 city–year observations; fixed-effects estimation automatically drops singleton observations, leaving 48 effective observations under the city-fixed-effects specification and 47 under the two-way fixed-effects specification). By city, the number of observed years ranges from 1 to 10 (Shenzhen 10, Foshan 7, Guangzhou 6, Zhongshan 5, Dongguan and Yunfu 4 each, Huizhou and Jieyang 3 each, Meizhou, Shantou and Zhuhai 2 each, Jiangmen 1). Only 12 of Guangdong's 21 prefecture-level cities have listed companies disclosing environmental investment during the sample period; the 9 missing cities are all located in eastern, western, and northern Guangdong, so the sample is systematically tilted toward the Pearl River Delta city cluster.

Two further points should be noted. First, ending the sample in 2020 is a deliberate design choice: after the national carbon market launched in July 2021, the pilot's net effect is no longer separately identifiable. Meanwhile, both the Shenzhen and Guangdong provincial markets launched within calendar year 2013, so the assignment of the policy year at annual frequency may involve measurement error; this error tends to attenuate estimates toward zero, meaning negative effects are more likely understated than overstated. Second, constrained by disclosure coverage, the conclusions of this paper should be understood as suggestive evidence at the scale of the Pearl River Delta city cluster. Listed companies' environmental investment data come from the CSMAR database and are aggregated to the city–year level by registration location; city-level macroeconomic data come from the China City Statistical Yearbook; city latitude and longitude data are used to construct the spatial weight matrices.

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3.2. Variable definitions

Table 1 reports the definitions and measurement of the main variables.

Table 1. Variable definitions and measurement

Variable type	Variable name	Operational definition
Dependent variable	ln_green_inv	log(1 + total environmental investment of listed companies in the city (RMB 10,000))
Core explanatory variable	did	treat × post (Guangzhou/Shenzhen = 1; 2013 and after = 1; the post-year assignment involves annual measurement error)
Spatial lag term	w_did	Product of the spatial weight matrix W and did
Control variable	ln_gdp	Log of city GDP
	indus_str	Value added of the secondary industry / GDP

Note: Listed companies' environmental investment is measured by the amounts disclosed in the CSMAR database, aggregated to the city–year level by registration location, with log(1+x) taken to mitigate right- skewness. This indicator is a proxy for urban green investment, not the total green investment of the whole society.

Descriptive statistics in Table 2 show that the treated group (Guangzhou and Shenzhen) has higher environmental-investment scale and economic size and a lower secondary-industry share than the control cities; the two groups differ systematically, and this paper mitigates their influence through two-way fixed effects and control variables.

Table 2. Descriptive statistics of main variables

Variable	Meaning	N	Mean
green_inv	Total environmental investment (RMB 10,000)	49	21433
ln_green_inv	log(1 +environmental investment)	49	8.11
did	treat × post	49	0.245
ln_gdp	log(GDP)	49	17.9
indus_str	Secondary industry value added / GDP	49	0.458
ln_green_inv: control group	Other 10 cities	33	7.08
ln_green_inv: treated group	Guangzhou, Shenzhen	16	10.2

Note: The sample comprises 49 city–year observations for 12 cities in Guangdong Province, 2010–2020; environmental investment is the registration-location aggregate of listed companies' disclosed amounts from CSMAR; the treated group is Guangzhou and Shenzhen (16 observations), and the control group is the other 10 prefecture-level cities (33 observations).

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3.3. Model specification

3.3.1. Baseline model: two-way fixed-effects multi-period DID

This paper constructs the following two-way fixed-effects multi-period difference-in-differences model:

$$\ln GreenInv(i,t) = \alpha + \beta_1 \cdot did(i,t) + \beta_2 \cdot \ln_gdp(i,t) + \beta_3 \cdot indus_str(i,t) + \mu_i + \lambda_t + \varepsilon(i,t) \quad (1)$$

where did is the policy dummy, equal to 1 once a pilot city enters the trading period and 0 otherwise; μi and λ_t are city and year fixed effects respectively, the latter absorbing annual exogenous shocks common to all cities (such as the 2015 new Environmental Protection Law and the 2017 central environmental inspections). Standard errors are clustered at the city level; given the small number of clusters (12 cities), inference is supplemented by wild cluster bootstrap correction (Rademacher weights, 999 replications). The coefficient β_1 is the net effect of the carbon market pilot on local green investment.

3.3.2. Spatial model: SLX

To test whether the policy effect transmits across administrative boundaries, this paper introduces the spatial lag of the policy variable into equation (1), constructing a spatially lagged X (SLX) model:

$$\ln GreenInv(i,t) = \alpha + \beta_1 \cdot did(i,t) + \beta_2 \cdot (W \times did)(i,t) + \beta_3 \cdot \ln_gdp(i,t) + \beta_4 \cdot indus_str(i,t) + \mu_i + \lambda_t + \varepsilon(i,t) \quad (2)$$

where W is the spatial weight matrix, and the coefficient β_2 on (W×did) captures the spatial spillover effect. The SLX model is chosen over the full spatial Durbin model (SDM) for three reasons: it is more robust in small samples, its coefficients have intuitive economic meaning, and it has been widely used to identify spatial spillovers of environmental regulation [19, 20]. It should be noted that the global Moran's I test shows no significant spatial autocorrelation in sample cities' green investment (I = -0.015, p = 0.233); the rationale for adopting SLX is that the policy shock itself may transmit across boundaries [20, 14], and its identification does not presuppose spatial autocorrelation of the dependent variable.

3.3.3. Spatial weight matrices

Following Zhuang and Zhang [26] and Jin and Shen [20], this paper constructs three spatial weight matrices — inverse-distance, k-nearest-neighbor (k=3), and economic-distance (all row-standardized) — to test the sensitivity of the conclusions to the weight specification (see Table 3).

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Table 3. Spatial weight matrix specifications

Weight type	Construction	Test purpose
Inverse-distance weight (baseline)	Reciprocal of the latitude–longitude distance between cities	Baseline specification, assuming spillovers decay with geographic distance
k-nearest-neighbor weight (robustness)	The 3 geographically nearest cities are taken as neighbors with equal weights, then row-standardized	Tests whether results depend on a particular adjacency structure
Economic-distance weight (robustness)	Reciprocal of the absolute gap in mean total GDP over the sample period between cities, then row-standardized	transmit through the economic-similarity channel Identifies whether spillovers

Note: All three spatial weight matrices are row-standardized; the k-nearest-neighbor weight uses equal weights for the nearest 3 cities.

3.4. Identification strategy and robustness design

This paper implements an exploratory placebo test: two cities are randomly drawn from the sample cities as pseudo-treated cities, repeated 500 times, and the position of the true coefficient in the distribution of pseudo-coefficients provides reference evidence; the sampling frame does not exclude the true treated cities and is for exploratory reference only.

4. Empirical results

4.1. Baseline regression results

Table 4 reports the estimation results of equation (1). Whether or not year fixed effects are controlled, the did coefficient is significantly negative; after adding year fixed effects, the core coefficient remains significant at the 1% level under wild cluster bootstrap correction, indicating that the baseline conclusion is robust to few-cluster correction and is not driven by common time shocks, but more likely reflects the net effect of the carbon trading pilot and platform-host cities. Regarding economic significance, given that the dependent variable is log(1+x), that the sample is based on disclosure coverage, and that it is subject to headquarters-economy effects, the coefficient should not be mechanically converted into a percentage investment decline; this paper interprets it as a negative semi-elasticity with a clear direction but a magnitude requiring cautious evaluation. Accordingly, H1 is supported.

Table 4. Baseline regression results: carbon trading pilot and local green investment

Variable	(1) City FE only	(2) Two-way FE
did	-1.901** (0.809)	-1.618*** (0.445)
wild cluster bootstrap p	—	< 0.001
Control variables	YES	YES
City fixed effects	YES	YES
Year fixed effects	NO	YES

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Table 4. (continued)

Observations	48	47
R^2	0.843	0.904

Note: The dependent variable is ln_green_inv; city-level cluster-robust standard errors in parentheses; *, **, *** denote significance at the 10%, 5%, and 1% levels respectively; the core coefficient in column (2) is additionally corrected by wild cluster bootstrap (Rademacher weights, 999 replications), bootstrap $p < 0.001$.

4.2. Dynamic effect analysis

Figure 1 reports event-study estimates with the year before the policy (t = -1) as the base period, which are used to characterize the time path of the policy effect rather than as a complete parallel-trends test; periods t = -3 and t = -2 are not identifiable because control cities have no observations, period t = 4 has no observations in the sample, and unreported years are not plotted. The estimates show that the policy effect exhibits a dynamic pattern of "immediate shock — brief rebound — renewed negative in the medium-to-long run": the negative shock is strongest in the current period of implementation, turns briefly positive in period +1 (significant at the 5% level), is insignificant in periods +2 and +3, and turns negative again in periods +5 to +7 but only at the 10% marginal significance level. This pattern is compatible with an interpretation of "immediate compliance-cost squeezing followed by adaptive adjustment," but this paper does not directly test the period-specific mechanisms, and the above reading should be regarded as a cautious, speculative description; it also offers a time-dimension perspective for reconciling the "promotion" versus "crowding-out" debate in the existing literature.

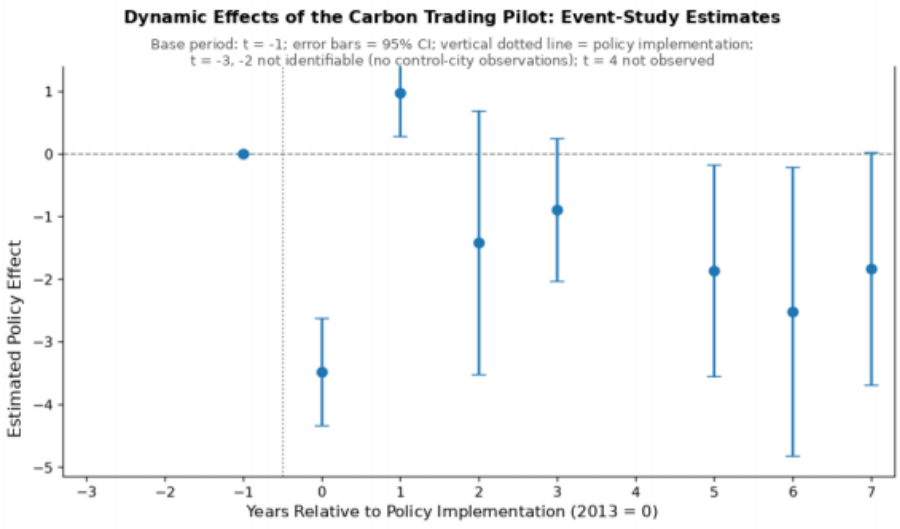


Figure 1. Dynamic effects of the carbon trading pilot policy: event-study estimates

Note: *, **, *** denote significance at the 10%, 5%, and 1% levels respectively; vertical lines are 95% confidence intervals; periods t = -3 and t = -2 are not identifiable because control cities have no observations, and period t = 4 has no observations in the sample; these are not plotted.

4.3. Spatial spillover effects

Table 5 reports the estimation results of equation (2) under the three spatial weight matrices. The did coefficients are significantly negative under all three weights, with magnitudes close to the baseline

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regression, indicating that the local relative suppression effect does not depend on the spatial model specification; the w_did coefficients are all insignificant, and under the baseline inverse-distance weight the wild cluster bootstrap p-value is 0.695, so the null hypothesis of "no spatial spillover" cannot be rejected. Judging from the 95% confidence intervals, the interval is relatively narrow under the economic- distance weight, so the null result is relatively informative there, while under the first two weights the data have limited power to discriminate the size of spillovers. Accordingly, this paper finds no evidence of significant spatial spillovers, consistent with the direction of H2; however, constrained by sample size and statistical power, this null result cannot be interpreted as proving that spatial spillovers do not exist.

Table 5. SLX model estimation results: spatial spillover effects

did (direct effect)	-1.930*** (0.380)	-1.874*** (0.333)	-1.407*** (0.407)
w_did (spillover effect)	-1.499 (2.438)	-1.422 (1.858)	-1.436 (0.917)
w_did bootstrap p	0.695	—	—
Control variables	YES	YES	YES
Two-way fixed effects	YES	YES	YES
Observations	47	47	47
R^2	0.904	0.904	0.905

Note: The dependent variable is ln_green_inv; city-level cluster-robust standard errors in parentheses; *, **, *** denote significance at the 10%, 5%, and 1% levels respectively; the bootstrap p-value for w_did is based on the inverse-distance weight model (Rademacher weights, 999 replications); all spatial weight matrices are row-standardized.

4.4. Robustness checks

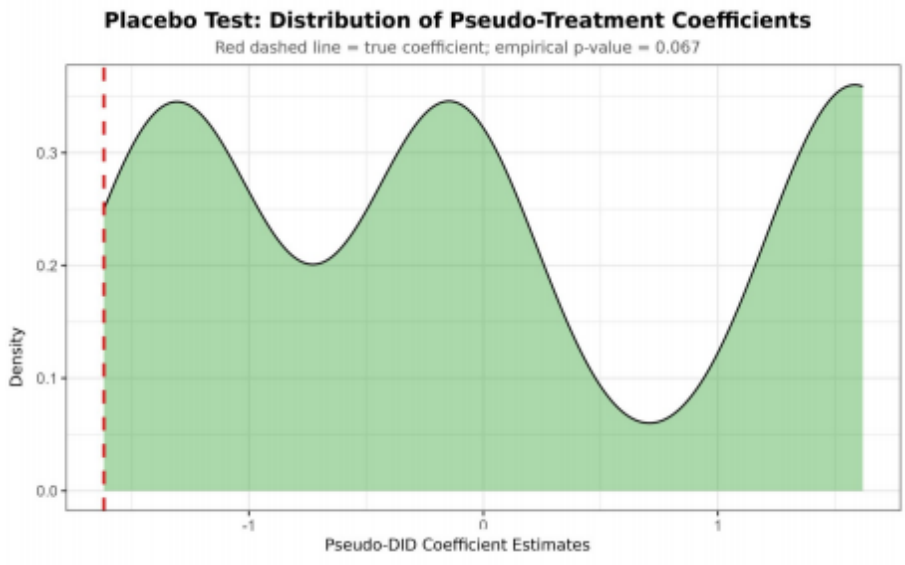


Figure 2. Exploratory placebo test: distribution of pseudo-coefficients (500 random draws)

The exploratory placebo test shows that the distribution of pseudo-coefficients is centered at 0, with the true coefficient located in the tail of the distribution (empirical p = 0.067, Figure 2). The baseline coefficient is unchanged after dropping cities with fewer than 2 observations; when

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Guangzhou is dropped and only Shenzhen serves as the treated group, the did coefficient is -1.113** (SE = 0.369), consistent in direction; when Shenzhen is dropped, did is absorbed by fixed-effect collinearity and is not identifiable, so this paper does not interpret it. Overall, the direction of the results is broadly consistent under some alternative specifications; given that there are only two treated cities and the placebo test is exploratory in design, the above robustness evidence should be interpreted cautiously and should not be regarded as comprehensive robustness.

5. Discussion

5.1. ESG "word–deed decoupling": a mechanism interpretation of our findings

Juxtaposing our findings with the carbon-market–ESG literature yields a noteworthy inference: existing evidence indicates that carbon trading pilots improve corporate ESG performance [22, 21], whereas this paper finds that the same policy significantly relatively suppresses listed companies' environmental investment at the city level. The coexistence of the two suggests that carbon markets may induce an ESG "word–deed decoupling" — firms improve their environmental image at the disclosure and rating level while reducing substantive green capital expenditure. This inference is consistent with research on greenwashing mechanisms [3, 4, 12] and also suggests that ESG ratings may have limitations in identifying substantive transition [25]. It should be emphasized that this paper does not directly introduce ESG rating data; the above inference is a resonant conjecture based on existing literature, not direct test evidence from this paper.

From the spatial dimension, ESG disclosure and rating signals are naturally mobile across boundaries, while substantive green investment is attached to plants and local factors; if what the demonstration channel transmits is mainly "words," then at the level of physical investment no significant spillover may be observed, and ESG greenwashing itself has also been shown to be a short-term strategic behavior [3]. In addition, our null result contrasts with the significant spillover evidence in the dimensions of carbon emissions [6, 9] and green innovation [10], suggesting that policy externalities may differ across output dimensions; Yu et al. [8], based on a national prefecture-level city sample, likewise find that the pilot has no significant impact on neighboring non-pilot cities, consistent with our results.

5.2. Policy implications

Our findings carry three implications for policymakers: first, carbon market design needs to be complemented by green-investment incentive instruments such as green credit and investment subsidies; second, regional green coordination cannot rely solely on spontaneous demonstration and diffusion by the market, and may require more proactive cross-regional institutional arrangements; third, the use of ESG ratings needs to introduce hard indicators such as green capital expenditure for cross-validation.

6. Conclusion

6.1. Research conclusions

Using city-aggregated data on listed companies' environmental investment in 12 prefecture-level cities in Guangdong Province from 2010 to 2020, this paper applies multi-period DID and SLX models to examine the impact of the carbon trading pilot on local green investment and its spatial spillovers. The main conclusions are threefold: first, listed companies' environmental investment in

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the platform cities (Guangzhou and Shenzhen) is significantly suppressed relative to non-platform cities within the province, and H1 is supported; second, no significant spatial spillover evidence is found under any of the three spatial weight matrices, consistent with the direction of H2 but insufficient to confirm the absence of spillovers; third, the policy effect exhibits a staged pattern of "immediate shock — brief rebound — renewed negative in the medium-to-long run (marginally significant)." These findings suggest that carbon markets may induce an ESG "word–deed decoupling" (as a possible explanation, not a directly tested mechanism), and that their impact is not observed to transmit significantly across boundaries.

6.2. Limitations and future research directions

This study has three types of limitations, and its conclusions should be understood as suggestive evidence. The first is identification limitations: the standard parallel-trends test cannot be fully implemented owing to scarce pre-policy observations, there are only two treated cities, and the Guangdong provincial carbon market covers key emitting enterprises across the province, so the results should be interpreted as the difference between Guangzhou/Shenzhen and other cities within the province, and causal conclusions need to remain cautious. The second is sample and measurement limitations: the sample covers only 12 cities with 47 effective observations, the 9 cities in eastern, western, and northern Guangdong are missing, and the conclusions apply mainly to the Pearl River Delta city cluster; the 2013 policy-year assignment may involve measurement error (tending to attenuate toward zero); environmental investment is aggregated by registration location, headquarters-economy effects may systematically bias the Guangzhou–Shenzhen aggregates upward, and alternative channels such as relocation of the aggregation location or crowding-out through market competition cannot be distinguished. The third is mechanism limitations: the ESG "word–deed decoupling" has not been directly tested. Future research can exploit the larger samples and finer-grained data generated by the expansion of the national carbon market, and test ESG performance and green investment within a unified framework at the firm level.

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