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The Impact of Green Finance Reform and Innovation Pilot Zones on Urban Carbon Emission Intensity: Evidence from a Difference-in-Differences Approach

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Abstract. Cities must cut carbon emissions per unit of output to meet the dual carbon goals, and green finance is expected to help. We study China's Green Finance Reform and Innovation Pilot Zones, launched in 2017, using a panel of 287 prefecture-level cities over 2013-2022. A difference-in-differences design with two-way fixed effects shows that treated cities lowered carbon emission intensity by about 12% relative to untreated ones, with larger reductions in later years. Two mechanisms drive this outcome: credit expands and becomes more available to low-carbon projects, and industry upgrades toward greener production instead of contracting. The effect appears almost exclusively where local financial systems are relatively developed. These results offer city-level causal evidence on green finance and argue for tailoring future reform to local financial conditions.

Keywords: carbon emission intensity, green finance pilot zone, difference-in-differences, green transformation, credit allocation

1. Introduction

Climate policy under the dual carbon goals depends on moving capital out of carbon-intensive activities and into green ones. China took a major step in 2016, when the People's Bank of China and six other ministries issued the Guidelines for Establishing the Green Financial System, followed in 2017 by State Council approval of the Green Finance Reform and Innovation Pilot Zones (hereafter, the pilot zones) in Huzhou, Quzhou, Guangzhou, the Ganjiang and Gui'an New Areas, and three cities in Xinjiang [1, 2]. The zones deploy green credit standards, environmental disclosure rules, interest subsidies, and bank-enterprise matchmaking to redirect lending toward green uses. Whether this package actually lowers urban carbon emission intensity remains untested, and answering that question is a prerequisite for judging the reform.

Research on green finance has so far focused on provinces or individual firms, estimating how green credit and green bonds affect emissions or emission intensity [3-6]; studies of the pilot zones themselves are also emerging [7-9]. Two gaps remain. First, city-level causal evidence is scarce, even though prefecture-level cities implement the policy on the ground. Second, little is known about the channels at work or about whether local financial development conditions the effect. We address both gaps.

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We exploit the 2017 pilot-zone rollout as a quasi-natural experiment. Our panel combines a city-level carbon emission inventory with the China City Statistical Yearbook for 287 prefecture-level cities over 2013-2022, and a difference-in-differences (DID) design identifies the policy's effect on carbon emission intensity. We then trace two channels, credit allocation and green upgrading within industry, and test whether financial development moderates the effect.

Treated cities cut carbon emission intensity by about 12% relative to controls, and the gap grew over the post-treatment period. Lending expanded significantly in pilot cities, while industry became greener rather than smaller: the number of above-designated-size industrial firms rose even as industrial SO2 intensity fell. Two competing channels, tertiarization and green patenting, find no support. The effect is concentrated in financially developed cities. We make three contributions. First, we provide causal evidence at the prefecture level, below the provincial and firm levels that dominate the literature. Second, we identify credit expansion and within-industry greening as the operative channels, showing that abatement is achieved by cleaning industry rather than shrinking it. Third, we show that financial development is a precondition for the policy to work, which supports differentiating the reform across regions.

The paper proceeds as follows. Section 2 gives the institutional background and hypotheses, Section 3 the research design, Section 4 the results, and Section 5 the conclusions and policy implications.

2. Policy background and research hypotheses

2.1. Policy background

State Council approval in 2017 created the first pilot zones: Huzhou and Quzhou (Zhejiang), Guangzhou (Guangdong), the Ganjiang New Area (Jiangxi), the Gui'an New Area (Guizhou), and Hami, Changji, and Karamay (Xinjiang). A second batch followed in 2019. Our treatment group comes from the 2017 batch but excludes the two new areas, whose statistical caliber is incompatible with prefecture-level data, and Hami and Changji, which lack usable pre-policy data, leaving Huzhou, Quzhou, Guangzhou, and Karamay. Four instruments define the policy: green credit standards that price environmental risk into lending decisions, compulsory environmental disclosure, interest subsidies for green loans, and government-bank-enterprise matchmaking. All four shift the relative cost of capital against high-carbon projects, which is the starting point for our hypotheses.

2.2. Baseline effect: the pilot policy and carbon emission intensity

The policy operates by repricing capital: high-carbon projects pay more for funds, green projects pay less. Because firms depend on external finance for production and investment, such repricing changes behavior. Banks bound by green credit rules and disclosure requirements curtail lending to high-carbon borrowers. Affected firms must then downsize or retrofit, while green firms invest and expand with cheaper credit. Capital thus moves toward low-carbon uses, the energy mix cleans up, and carbon emission intensity falls. Hypothesis H1: pilot cities experience a significant reduction in carbon emission intensity relative to non-pilot cities.

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2.3. Mechanism 1: credit resource allocation

The credit market is the policy's most immediate target [10, 11]. Green credit quotas, central bank relending, and interest subsidies lower the cost and risk of green lending for banks, expanding the supply of credit tilted toward low-carbon uses [3]. Mandatory environmental disclosure further reduces the information gap between banks and borrowers, so low-carbon projects obtain funding more easily. Ample and greener credit relaxes financing constraints on abatement investment and lowers emission intensity [12]. Hypothesis H2a: the policy reduces carbon emission intensity by expanding credit supply for low-carbon projects.

2.4. Mechanism 2: within-industry green transformation

Greener credit need not shrink industry. Cheap green funds lower entry barriers for green industrial projects, while incumbent polluters can regain financing by retrofitting their technology instead of exiting. This is the mechanism behind the Porter hypothesis, in which well-designed regulation induces innovation that offsets compliance costs [6, 13]. Green entry and incumbent retrofitting together raise the share of clean capacity within industry, cutting emissions per unit of output. This channel fits the policy's design better than a shift toward services, since the instruments target real-sector greening. Hypothesis H2b: the policy reduces carbon emission intensity through green upgrading within industry.

2.5. The moderating role of financial development

Policy effectiveness also depends on the local financial environment [14]. Where financial systems are developed, institutions are numerous, green products are available, and policy signals translate readily into lending and abatement. Where finance is underdeveloped, the instruments find few takers and little changes. The same policy input should therefore yield larger emission cuts in financially advanced cities. Hypothesis H3: financial development strengthens the policy's emission reduction effect.

3. Research design

3.1. Model specification

We estimate the following two-way fixed effects DID model:

$$C_{ct} = \alpha_0 + \alpha_1 P_{ct} + \beta X_{ct} + \mu_c + \lambda_t + \varepsilon_{ct} \quad (1)$$

where C_ct denotes log carbon emission intensity in city c, year t; P_ct equals one for pilot cities from 2017 onward and zero otherwise; X_ct contains the controls; μ_c and λ_t are city and year fixed effects; and α_1 is the policy's average treatment effect. The following event study model serves both to test parallel trends and to trace the effect's dynamics:

$$C_{ct} = \alpha + \sum_k \beta_k D_{ct}^k + \beta X_{ct} + \mu_c + \lambda_t + \varepsilon_{ct} \quad (2)$$

where D_ct(k) indexes years relative to treatment, omitting k = -1 as the base period. For H3, we add to model (1) an interaction between the policy dummy and standardized financial development:

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$$C_{ct} = \alpha_0 + \alpha_1 P_{ct} + \alpha_2 P_{ct} \times Fin_c + \alpha_3 Fin_c + \beta X_{ct} + \mu_c + \lambda_t + \varepsilon_{ct} \quad (3)$$

A significantly negative α_2 supports the moderation hypothesis. Standard errors are clustered by city, and inference is further checked with the wild cluster bootstrap, since only four cities are treated.

3.2. Variable definitions

Table 1. Variable definitions

Variable type	Symbol	Definition
Dependent variable	lnci	Log of carbon emission intensity: ln(CO2 emissions (tons)/GDP (10,000 yuan))
Key explanatory variable	did	Pilot city x year 2017 and thereafter
Control variable	lnpgdp	Log of GDP per capita
	gov	Government intervention: general budgetary fiscal expenditure/GDP
	lnpop	Log of registered population
Mechanism variable	lnloan	Log of year-end loan balance of financial institutions (credit allocation)
	lnfirm	Log of the number of above-designated-size industrial enterprises (within-industry green transformation)
	lnso2i	Log of industrial SO2 emission intensity: ln(industrial SO2 emissions/GDP), corroborating variable
Moderating variable	fin	Financial development: (deposit balance + loan balance)/GDP, standardized in regressions

3.3. Data and sample

We merge three sources: a city-level CO2 emission inventory (2025 release), the China City Statistical Yearbook panel, and city-level green patent records. Excluded are the four municipalities and Lanzhou, a 2019 second-batch pilot. The four treated cities with usable data are compared against 283 control prefecture-level cities. The sample runs from 2013, since earlier years fail the pre-trend test, to 2022, after which registered population data cease; estimates through 2024 appear as a robustness check. The final panel has 2,868 city-year observations.

Table 2. Descriptive statistics, 2013-2022

Variable	Treated mean	Treated SD	Control mean	Control SD
Carbon emission intensity (log)	-0.178	0.700	0.260	0.813
GDP per capita (log)	11.591	0.455	10.821	0.537
Government intervention	0.150	0.058	0.219	0.133

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Table 2. (continued)

Registered population (log)	5.358	1.217	5.853	0.705
Loan balance (log)	17.436	1.562	16.640	1.092
Number of industrial enterprises (log)	7.017	1.612	6.534	1.155
Financial development	3.237	0.814	2.650	1.248

Note: the treatment group covers 40 city-year observations (4 cities over 10 years), against 2,828 observations in the control group.

4. Empirical results

Table 3. Baseline regression results

Variable	(1)	(2)
Pilot policy (did)	-0.064	-0.128***
	(0.056)	(0.048)
GDP per capita (log)		-0.681***
		(0.038)
Government intervention		0.872***
		(0.161)
Registered population (log)		-0.826***
		(0.129)
City/year fixed effects	Yes	Yes
Observations	2868	2868
R-squared	0.961	0.982

Column (1) of Table 3, with fixed effects only, yields an insignificant did coefficient of -0.064. Adding controls in column (2) gives -0.128, significant at the 1% level: treated cities cut carbon emission intensity by roughly 12.0% relative to controls (e^-0.128 is about 0.880), supporting H1. Controls take the expected signs, with higher GDP per capita and larger populations associated with lower intensity. Event study estimates (Figure 1) show the effect growing annually, reaching -0.183 by the fifth post-treatment year.

4.1. Parallel trends test

DID requires that treated and control cities would have trended together absent the policy. The event study estimates in Figure 1 bear this out: coefficients for k = -4 to -2 are small, with confidence bands covering zero, so the groups did not diverge before treatment. Post-treatment coefficients decline year by year, a pattern consistent with a policy that works gradually through institution building, credit extension, and project implementation.

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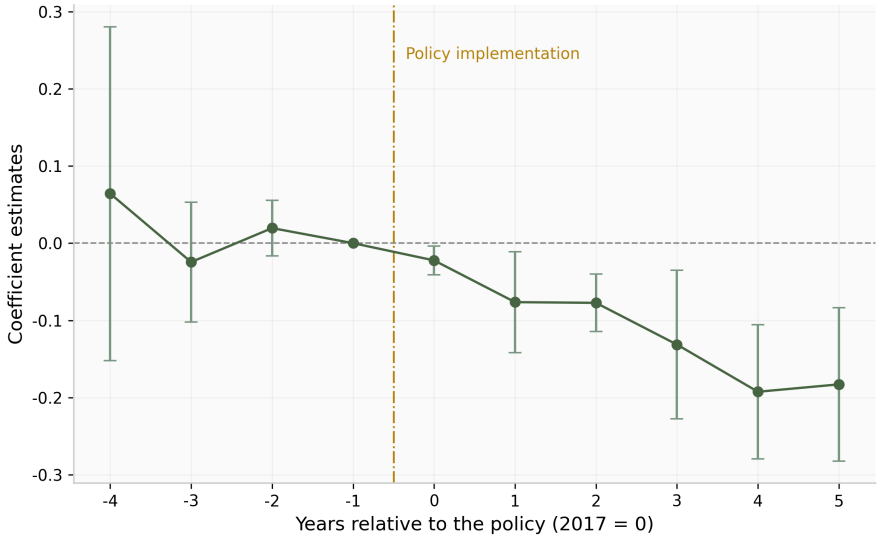


Figure 1. Parallel trends test based on the event study estimates

4.2. Placebo test

We also run a placebo test: four cities are randomly selected from the 283 controls, assigned a fictitious treatment, and the baseline model is re-estimated, with the whole procedure repeated 500 times. The pseudo estimates in Figure 2 center on zero, while the actual estimate of -0.128 lies far in the left tail; only about 8% of draws produce smaller values. Chance is therefore an unlikely explanation for the baseline finding.

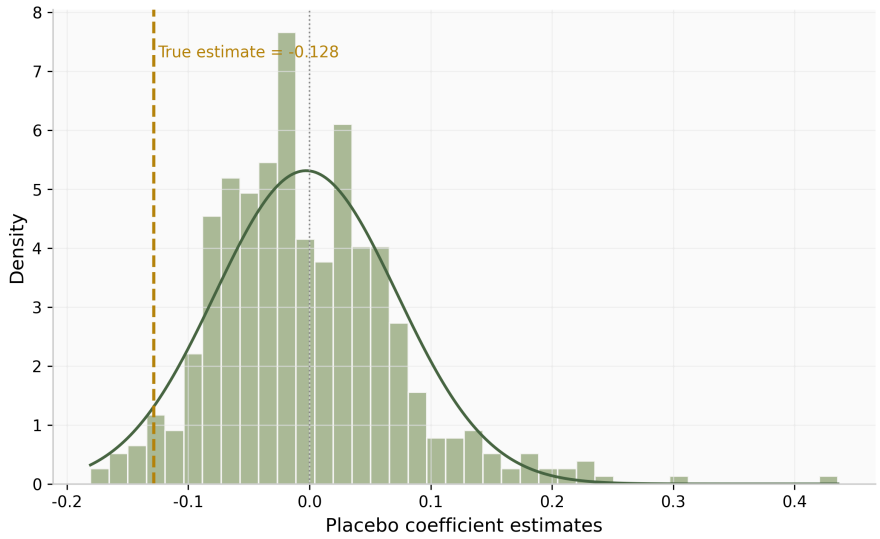


Figure 2. Placebo test based on 500 random draws

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4.3. Robustness checks

Table 4. Robustness checks

Variable	(1) Excluding Guangzhou	(2) Winsorized outcome	(3) Extended to 2024
Pilot policy (did)	-0.076***	-0.134**	-0.158**
	(0.021)	(0.052)	(0.078)
Control variables	Yes	Yes	Yes (excl. population)
City/year fixed effects	Yes	Yes	Yes
Observations	2858	2868	3447
R-squared	0.982	0.981	0.976

Note: column (3) controls for log GDP per capita and government intervention only, as registered population data are no longer available after 2022.

Table 4 reports three robustness checks. Excluding Guangzhou, by far the largest treated city, leaves a coefficient of -0.076, still significant at the 1% level. Winsorizing the outcome at the 1% level yields -0.134, and extending the sample to 2024 yields -0.158. All estimates lie between -0.08 and -0.16 and remain significant, so the baseline result is robust.

4.4. Mechanism tests

Table 5. Mechanism tests

Variable	(1) Credit allocation	(2) Within-industry greening	(3) SO2 intensity
Pilot policy (did)	0.084**	0.212***	-0.393*
	(0.035)	(0.073)	(0.223)
Control variables	Yes	Yes	Yes
City/year fixed effects	Yes	Yes	Yes
Observations	2868	2867	2859
R-squared	0.988	0.980	0.920

Note: the three outcomes are, respectively, the logarithms of the year-end loan balance of financial institutions, the number of above-designated-size industrial enterprises, and industrial SO2 emission intensity; column (3) provides corroborating evidence for channel (2). For each mechanism variable, event study estimates pass the pre-trend test.

In Table 5, column (1) shows that loan balances in treated cities grew by about 8.4% relative to controls, as green credit arrangements and relending eased financing for low-carbon projects; H2a holds. Column (2) shows a 21.2% rise in above-designated-size industrial firms, so abatement coincided with industrial expansion rather than contraction. Column (3) adds corroboration: industrial SO2 intensity fell by about 32.5% (e^-0.393 is about 0.675), confirming that industry became cleaner, so H2b holds as well. Two alternative channels fail the test: the coefficient on tertiarization (the ratio of tertiary to secondary value added) is significantly negative at -0.210, and

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green patenting is insignificant. Neither transmits the effect, which reinforces the within-industry greening account.

4.5. The moderating effect of financial development

Table 6. Moderating effect test

Variable	Carbon emission intensity
Pilot policy (did)	-0.002
	(0.050)
Policy x financial development (did x fin)	-0.186***
	(0.041)
Financial development (fin)	0.021
	(0.014)
Control variables	Yes
City/year fixed effects	Yes
Observations	2868
R-squared	0.983

In Table 6, the policy-financial development interaction equals -0.186, significant at the 1% level, while the did main effect is essentially zero. The emission cuts thus come almost entirely from financially developed cities, making a sound financial base a precondition for policy success. H3 is supported.

5. Conclusion and policy implications

Using the Green Finance Reform and Innovation Pilot Zones as a quasi-natural experiment and a 2013-2022 panel of 287 prefecture-level cities, we find that the policy cut urban carbon emission intensity by about 12%, with the effect growing over time. Credit expansion for low-carbon projects and green upgrading within industry drive the result, and financially developed cities account for nearly all of it. Three recommendations follow.

First, consolidate the existing zones and expand the reform gradually. The roughly 12% reduction in emission intensity kept growing over our sample, so current instruments, including green credit and relending support, should be maintained to avoid interrupting realized gains. Once the first batch's experience has been reviewed, qualified cities can join later batches to scale up the dividends.

Second, keep channeling finance toward greener industry. Emissions fell while the industrial firm count rose and pollutant intensity dropped, so abatement came from green entry and retrofitting rather than relocation. Green credit quotas should therefore fund industrial energy-saving retrofits and clean energy substitution; certification standards for green projects should be refined to lower financing thresholds; and land and energy allocation should accommodate incoming green capacity, so that cuts are not achieved by simply shrinking industry.

Third, tailor the reform to local financial conditions. Because the gains concentrate in financially advanced cities and effectiveness depends on institutions, products, and skilled staff, cities with

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strong financial bases should deepen product innovation, while those with weak bases should first build infrastructure, standards, statistical systems, and talent, scaling up only when ready. Substance should come before form.

Limitations remain. Having only four treated cities limits precision, which we mitigate with cluster-robust inference and the wild cluster bootstrap, and the sample ends in 2022 owing to data updates. As later pilot batches accumulate data, multi-period and finer-grained evaluations merit future work.

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