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Does green finance work for all? Evidence from resource-dependent cities in China

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Abstract. Pilot zones for green finance reform and innovation constitute a pivotal institutional initiative for China to advance green economic transition. Nevertheless, whether their carbon abatement effects exhibit universal applicability or hinge on urban structural characteristics remains insufficiently verified by empirical evidence. This study constructs an unbalanced city-year panel dataset and adopts a triple Difference-in-Differences (DDD) framework to examine the policy's impacts on carbon emission intensity alongside its heterogeneous mechanisms, with three batches of national green finance reform pilot cities designated as the treatment group. Three core findings emerge. First, no statistically significant average emission reduction effect is detected across the full sample; this null result does not indicate policy ineffectiveness but arises from offsetting effects between resource-dependent and non-resource-dependent cities. Second, resource endowment acts as a critical moderating factor shaping policy outcomes. Cities with stronger economic fundamentals achieve more substantial carbon mitigation. Third, although regional business environment exerts a significantly negative direct impact on carbon emission intensity, a complete mediating chain whereby green finance policy reduces carbon emissions by improving business environments cannot be validated. This implies that the development of soft institutional environments has lagged behind the allocation of green financial capital. This paper identifies a structural feature that the carbon abatement dividends of green finance are concentrated in resource-dependent cities with high baseline carbon levels, furnishing empirical grounds for targeted policy expansion and institutional coordination.

Keywords: green finance, carbon emission intensity, resource dependence, policy heterogeneity

1. Introduction

Against the backdrop of intensifying global climate change, China, as the world's largest carbon emitter, has pledged to peak carbon dioxide emissions before 2030 and achieve carbon neutrality by 2060 [1]. In 2017, the State Council established pilot zones for green finance reform and innovation across five provincial-level regions, namely Zhejiang, Jiangxi, Guangdong, Guizhou, and Xinjiang [2]. The core objective of this policy framework is to channel capital toward low-carbon transition through green credit, green bonds, and differentiated financing mechanisms, with the goal of developing replicable experience for nationwide rollout. As China enters a critical stage of pursuing its "Dual Carbon" goals, rigorous evaluation of whether these pilot policies effectively lower urban carbon intensity and the conditions under which such effects materialize carries profound policy implications for refining pilot programs, while also offering actionable references for low-carbon transformation in developing economies worldwide.

An extensive body of literature has explored determinants of carbon emissions and the environmental governance functions of financial instruments. On one hand, prior studies identify multiple channels through which financial development shapes carbon emissions: financial agglomeration alters urban carbon emissions via industrial structure upgrading and technological progress, whereas fintech curbs carbon emission intensity by enabling industrial intelligence and boosting green innovation performance [3]. On the other hand, scholars have investigated how resource dependence molds carbon emission efficiency. For instance, Wang and Xu documented that resource dependence significantly suppressed urban carbon emission efficiency, primarily by constraining urban scale expansion and weakening the stringency of environmental regulations [4]. Despite these parallel strands of research, a direct analytical bridge connecting green finance pilot policies to carbon abatement, particularly examinations through the lens of heterogeneous resource endowments, remains underdeveloped. This perspective is critical because resource endowments fundamentally shape urban industrial structures, energy consumption patterns, and carbon emission trajectories. Divergence in initial conditions implies that different cities face distinctly different marginal abatement costs and policy response spaces when confronted with green finance interventions, suggesting that average treatment effect estimates may mask profound structural differentiation. Most existing evaluations of green finance reform pilot zones focus exclusively on average treatment effects, implicitly assuming uniform policy impacts across all cities. The limited number of studies addressing heterogeneity yield contradictory theoretical predictions: some scholars argue that resource-dependent cities exhibit muted policy effectiveness due to path dependence and industrial lock-in [5]; while others find that green finance exerts stronger carbon mitigation effects in cities with elevated resource dependence. This theoretical ambiguity creates a critical research gap that the present paper aims to fill [6].

To address this gap, this study employs a triple Difference-in-Differences (DDD) estimator based on an unbalanced panel dataset covering 36 Chinese prefecture-level cities (9 treated pilot cities and 27 matched non-pilot control cities) from 2014 to 2024, empirically testing whether resource dependence constrains or amplifies the efficacy of green finance policies. This paper makes three marginal contributions to the extant literature. First, thematically, this study explicitly models how resource endowment moderates the relationship between economic fundamentals and policy effects, reconciling fragmented research across green finance and carbon emission scholarship. Second, it proposes the marginal abatement space hypothesis, grounded in the "low-hanging fruit" logic as a competing theoretical framework against conventional path dependence narratives: resource-dependent cities possess abundant cost-effective abatement potential due to their extremely high baseline carbon emission intensity, rendering them the primary beneficiaries of green finance interventions rather than laggard regions. Third, empirically, this research challenges the mainstream assumption that green finance policies generate homogeneous positive abatement outcomes across all pilot jurisdictions, delivering robust evidence of pronounced resource-endowment-based heterogeneity, where resource-intensive cities capture the largest carbon reduction gains from policy implementation.

2. Literature review and research hypotheses

2.1. Average and heterogeneous carbon abatement effects of green finance policies

Financial resource allocation theory posits that financial systems can reallocate production factors across sectors and regions via differentiated credit and capital guidance, rectifying capital misallocation and redirecting resources from high-pollution, energy-intensive industries toward low-carbon, high-efficiency sectors [7]. Supported by dedicated policy frameworks, pilot zones for green finance reform and innovation construct diversified green financial supply systems encompassing green credit, green bonds, and carbon emission reduction monetary tools. Theoretically, such institutional arrangements should drive low-carbon upgrading of regional industrial structures and reduce carbon emission intensity. A wealth of existing literature corroborates this transmission channel. For example, Liu et al. verified at the micro-firm level that green finance pilot zones raised corporate Research and Development (R&D) investment and green patent output by leveraging regional economic endowments, thereby facilitating industrial green transformation [8]. Yan and Jie further noted that industrial low-carbon upgrading constituted the core channel through which green finance delivers carbon abatement, with energy structure optimization and green innovation embedded within broader industrial restructuring processes [9].

Nevertheless, the emission reduction effects of green finance policies do not operate in isolation; they are embedded within cities' industrial structures and resource endowments. Divergent initial levels of resource dependence generate distinct marginal abatement cost curves across jurisdictions, implying that policy impacts do not necessarily follow a uniform directional pattern. Specifically, resource-dependent cities are dominated by extractive and heavy chemical industries, resulting in exceptionally high baseline carbon emission intensity and ample room for energy efficiency retrofits [10]. When green finance policies provide low-cost long-term capital to these regions, process overhauls, waste heat recovery, and fuel substitution deliver substantial carbon emission cuts, reflecting a salient low-hanging fruit effect [11]. By contrast, non-resource-dependent cities feature industrial portfolios concentrated in manufacturing and modern services, with inherently low carbon emission intensity. Further abatement efforts in such jurisdictions enter a high-cost zone with limited marginal potential [10]. In the short run, output expansion and rising energy demand driven by stronger economic fundamentals may outweigh efficiency gains, potentially leading to relative increases in carbon emission intensity. Pooling these two opposing directional effects into a single full-sample regression is likely to yield an apparent null average policy effect. Accordingly, Hypothesis 1 is proposed.

H1: Across the full sample, the establishment of green finance reform and innovation pilot zones exerts no statistically significant average treatment effect on urban carbon emission intensity.

2.2. Moderating role of regional resource endowment: the marginal abatement space hypothesis

Path dependence theory argues that specialized industrial chains, fixed capital equipment, and labor market structures forged by early resource exploitation generate robust industrial lock-in, drastically elevating transition costs in resource-dependent regions and potentially attenuating the carbon abatement dividends of green finance policies [12]. Heterogeneity analyses conducted by Lv et al. confirmed that green finance delivered significantly stronger emission reduction outcomes in non-resource-dependent cities relative to resource-dependent counterparts, with resource-induced path dependence suppressing policy effectiveness [5]. Additional empirical studies stratify cities by resource type and document that mature resource cities exhibit the weakest improvements in carbon emission efficiency from green finance interventions, with path dependence serving as the primary constraining factor [13]. However, such inferences overlook a pivotal mechanism: a positive correlation exists between the severity of industrial lock-in and the scope for marginal abatement. Precisely because high-carbon industrial lock-in prevails in resource-dependent cities, these jurisdictions possess expansive marginal abatement potential. This paper advances the marginal abatement space hypothesis as a competing explanatory framework, grounded in three core logics.

First, baseline magnitude effects operate meaningfully. Carbon emission intensity in resource-dependent cities far exceeds national average levels, meaning energy efficiency improvements translate into large absolute emission reductions and prominent low-hanging fruit gains. Conversely, non-resource-dependent cities face diminishing abatement returns, where additional emission compression incurs sharply rising costs and technical barriers. Second, capital efficiency differs systematically across city types. Low-cost long-term green financial capital allocated to high-carbon enterprises for backward capacity elimination, waste heat recovery, and fuel substitution delivers far larger carbon cuts per unit of investment than equivalent funding directed toward inherently clean modern service sectors. Third, the scope for structural adjustment varies substantially. Resource-dependent cities undergo profound diversification away from heavy chemical industries, with broad latitude for industrial restructuring. Non-resource-dependent cities only pursue marginal refinements of already low-carbon industrial portfolios, limiting transformative potential.

Notably, capturing these marginal abatement gains hinges on a minimum threshold of economic fundamentals. A city's economic foundation reflects its capacity to realize low-hanging fruit abatement opportunities, encompassing fiscal matching and risk underwriting capacity, corporate asset quality and credit accessibility, as well as talent absorption and technological assimilation capabilities. High resource dependence supplies abundant abatement potential (the quantity of low-hanging fruit), while robust economic fundamentals furnish the financial, material, and human capital resources required to unlock such potential (the ladder to harvest emissions reductions). Their interaction jointly determines the real-world carbon mitigation performance of green finance policies. This logic underpins Hypothesis 2.

H2: Resource dependence significantly negatively moderates the carbon abatement effect of economic fundamentals. This pattern indicates that cities with elevated resource dependence achieve more pronounced emission reductions via stronger economic fundamentals, rather than signaling the ineffectiveness of path dependence theory.

2.3. The role of regional business environments: direct impacts and transmission limitations

New institutional economics posits that effective policy implementation relies critically on the quality of local institutional environments [14]. A sound business environment encompasses secure contractual enforcement, credible market integrity frameworks, efficient policy administration, and a law-based commercial ecosystem, all of which are core elements that reduce transaction frictions and optimize resource allocation.

Regions with superior business environments feature transparent market rules and efficient governance, which mitigate information asymmetry and transaction costs in green investment and financing, ensuring green capital precisely targets low-carbon projects and directly drives reductions in carbon emission intensity.

Theoretically, green finance pilot policies may indirectly lower carbon emissions by upgrading regional business environments (e.g., compelling governments to streamline administrative services and standardize market order). However, this complete transmission chain requires two preconditions: the policy must generate measurable improvements in business environments, and upgraded business environments must meaningfully suppress carbon emissions. Crucially, business environment upgrading represents a long-run institutional evolution process, and short-term policy interventions may fail to produce empirically detectable transmission effects. Data constraints exacerbate this identification challenge: the earliest available business environment indicators start in 2017 (the launch year of the first pilot batch), with no baseline observations for the pre-policy period. This creates fundamental barriers to isolating the net policy effect on business environments. While this study anticipates difficulty validating the full mediating chain, it acknowledges the existence of a direct carbon abatement channel for business environments. The corresponding hypotheses are specified as follows.

H3-a: Regional business environments exert a significantly negative direct effect on carbon emission intensity; cities with higher business environment scores record lower carbon emission intensity.

H3-b: A complete mediating transmission chain running from green finance pilot policy implementation through improved business environments to reduced carbon emissions cannot be established under the current sample constraints (the indirect effect is statistically insignificant), reflecting bottlenecks in policy transmission via soft institutional channels.

3. Empirical testing

3.1. Sample selection and data sources

Primary raw data are sourced from the *China City Statistical Yearbook*, *China Energy Statistical Yearbook*, the EPS Urban Macroeconomic Database, and municipal statistical communiqués. All prefecture-level and above cities designated across three batches of national green finance reform and innovation pilot zones constitute the treatment group [15]. The pilot roster includes the 2017 first batch: Guangzhou, Huzhou, Quzhou, Nanchang (Ganjiang New Area), Guiyang (Gui'an New Area), Karamay, Changji, Hami; the 2019 second batch: Lanzhou (Lanzhou New Area); and the 2022 third batch: Chongqing, totaling 10 pilot jurisdictions. For national new areas without independent statistical accounting frameworks (Ganjiang, Gui'an, Lanzhou New Areas), aggregate macroeconomic data of their host prefecture-level cities are adopted as proxies.

To satisfy the identification assumptions of the DDD model, non-pilot prefecture-level cities are selected as the control group, matched based on pre-policy economic development, resource endowment, and business environment characteristics to mitigate systematic sample bias. The sample window spans 2014–2024, fully covering pre- and post-implementation periods for all three pilot batches and accommodating multi-period shock identification in the DDD framework. A three-year symmetric pre- and post-policy observation window is adopted to minimize disturbances from exogenous macroeconomic shocks. The 2017 first batch and 2019 second batch pilots have completed three-year pre- and post-policy observations (e.g., the 2017 batch adopts a 2014-2020 window). The 2022 third batch (Chongqing) only has two post-policy annual observations due to the 2024 data cutoff, resulting in an asymmetric window of three pre-policy years and two post-policy years for this subsample.

All nominal monetary variables are deflated to constant 2010 prices and log-transformed to alleviate heteroskedasticity; linear interpolation fills minor data gaps, ultimately yielding a balanced panel dataset for regression analysis. After substituting municipal aggregate data for new areas lacking independent statistics, nine treatment cities are retained for formal empirical estimation: Guangzhou, Huzhou, Quzhou, Nanchang, Guiyang, Karamay, Hami, Lanzhou, and Chongqing. Changji is excluded due to missing foundational data required for carbon emission intensity accounting. Twenty-seven matched non-pilot cities form the control group, generating a total of 248 city-year observations. Table 1 lists paired treatment and control cities.

Table 1. Matched treatment and control cities

Treatment city	Matched control cities
Guangzhou	Shenzhen, Foshan, Dongguan
Huzhou	Hangzhou, Jiaxing, Shaoxing
Quzhou	Wuxi, Jinhua, Changzhou
Chongqing	Chengdu, Mianyang, Yichang
Nanchang	Wuhan, Zhuzhou, Hefei
Guiyang	Changsha, Kunming, Liuzhou
Lanzhou	Yulin, Wuwei, Wuzhong

Table 1. Continued

Karamay	Ordos, Wuhai, Shizuishan
Hami	Baiyin, Zhangye, Guyuan

Matching criteria for control group selection follow three rules: (1) all observations are prefecture-level or higher cities, excluding national new areas without independent statistical caliber; (2) matching prioritizes pre-policy economic fundamentals and resource endowments, with a geographic matching radius capped at 500 kilometers; (3) a one-to-many matching strategy assigns multiple demographically comparable non-pilot cities to each treatment city to narrow inter-group disparities, improve sample balance, and enhance DDD estimation efficiency.

3.2. Variable definition and measurement

3.2.1. Explanatory variable – DDD interaction term for policy and economic fundamentals (Treat × Post × ECO)

A DDD specification is constructed to compare carbon emission intensity across three dimensions: pre- vs. post-pilot implementation, pilot vs. non-pilot jurisdictions, and cross-city disparities in pre-policy economic fundamentals. Economic fundamentals (ECO) are measured as the natural logarithm of per capita real Gross Domestic Product (GDP) averaged over the three years preceding policy approval, capturing initial municipal economic endowments, fiscal capacity, and market maturity.

Accordingly, the paper constructs a group dummy variable (Treat) taking a value of 1 for treatment cities and 0 for control cities, and a time dummy variable (Post) taking a value of 1 for years following the policy intervention and 0 otherwise. The Difference-in-Differences (DID) interaction term (Treat × Post) is accordingly assigned a value of 1 exclusively for treatment cities in the post-policy period, and 0 otherwise. The work further constructs the core independent variable, Treat × Post × ECO, by interacting the green finance policy dummy with the ecological indicator.

3.2.2. Explained variable – carbon abatement outcome (CI)

Carbon emission intensity serves as the primary dependent variable, defined as total carbon dioxide emissions per unit of real GDP. This metric intuitively quantifies low-carbon transition performance following green finance policy rollout, with lower values indicating superior carbon abatement performance [16]. Unlike aggregate carbon emissions, carbon emission intensity eliminates bias from heterogeneous economic output scales, enabling consistent cross-jurisdictional comparison of pilot zone low-carbon progress and aligning with this study's focus on differentiated abatement performance across economic fundamentals.

Urban carbon emissions are calculated based on fossil fuel consumption across four sectors: industry, construction, transportation, and residential energy use. Following the IPCC National Greenhouse Gas Inventory Guidelines and standard domestic urban carbon accounting protocols, total annual municipal carbon dioxide emissions are computed as the following equation (1).

$$CO_2 = \sum_{i=1} E_i \times NCV_i \times CEF_i \tag{1}$$

Here, CO_2 denotes total annual municipal carbon emissions; E_i represents physical consumption of fuel type i; NCV_i is the average net calorific value of fuel i; and CEF_i refers to the carbon emission factor for fuel i, with coefficients standardized in accordance with the Provincial Greenhouse Gas Inventory Compilation Guidelines. Carbon emission intensity is formalized in equation (2). Its natural logarithm is employed to mitigate heteroskedasticity.

$$CI = ln\frac{CO_2}{GDP_{2000}} \tag{2}$$

GDP_{2000} denotes municipal real GDP deflated to constant 2,000 prices, matching the price base year adopted for economic fundamentals to ensure consistent metric calibration. Municipal constant-price GDP calculations adopt uniform GDP deflators consistent with the measurement of the economic fundamental variable.

3.2.3. Moderating variable – regional resource endowment (RD)

Drawing on standard measurement frameworks from resource curse research, Resource Dependence (RD) is proxied by the three-year average share of extractive industry employment preceding policy implementation, utilizing balanced municipal employment panel data with regression-imputed missing values to eliminate measurement bias from incomplete observations [17].

The full sample is stratified into three resource dependence tiers via natural breakpoint classification based on the distribution of extractive employment shares: (1) In high resource dependence group, extractive industry employment share > 10%. Extractive sectors constitute the primary source of local employment, generating extreme structural reliance on mineral exploitation. (2) In medium resource dependence group, 0 < extractive industry employment share ≤ 10%. Extractive activities maintain modest employment weight but do not dominate the local economy, with ongoing industrial diversification underway. (3) In low resource dependence group, extractive industry employment share = 0. No extractive employment exists, with local economies fully reliant on manufacturing and service sectors free of resource lock-in.

Stratification results for treatment pilot cities under this classification are as follows. High resource dependence group contains 2 cities: Karamay (61.74% average extractive employment share, 2014-2016) and Hami (49.18% average extractive employment share, 2014-2016), far exceeding the full-sample mean resource dependence level. Medium resource dependence group has 3 cities: Lanzhou (3.46%, 2016-2018), Chongqing (1.94%, 2019-2021), Guiyang (1.87%, 2014-2016). Extractive sectors generate measurable employment but remain below the 10% high-dependence threshold. Low resource dependence group include 4 cities: Huzhou, Guangzhou, Nanchang, Quzhou, all recording zero extractive industry employment shares across their respective pre-policy measurement windows, with non-resource industrial structures exclusively. This stratification framework forms the core moderating variable used to identify heterogeneous policy impacts of green finance pilot zones on carbon emission intensity across divergent resource endowments.

3.2.4. Mediating variable – municipal Business Environment (BE)

The municipal Business Environment index (BE) is sourced from the China Urban Business Environment Database jointly developed by researchers at Peking University and Wuhan University [18]. Led by Huang et al., this database covers 296 prefecture-level and above Chinese cities from 2017 to 2023. Its indicator framework aligns with China's *13th Five-Year Plan Outline* and the *Regulations on Optimizing the Business Environment*, while drawing on the World Bank's business environment evaluation architecture, with indicator weights derived from textual frequency analysis of central policy documents, ensuring robust authority and cross-city comparability. The composite total business environment score aggregates four first-tier sub-indices, decomposed into 14 second-tier and 23 third-tier indicators that comprehensively capture institutional quality across market factor provision, administrative efficiency, legal safeguards, and social credit. Table 2 presents the four first-tier indicators, their respective weights, and core connotations, offering a concise overview of the index structure.

Average post-policy business environment scores over a three-year window are adopted to proxy institutional quality (one year of observations for the 2022 third batch pilot due to data limits). For 2017 first batch pilots, mean index value for 2018-2020 is adopted; 2019 second batch pilots employ the mean index value for 2020–2022; 2022 third batch pilot (Chongqing) uses the single-year index value for 2023. Cross-sectional averaging is required for mediating effect testing due to limited time-series coverage of the business environment database.

Table 2. Composition and weight structure of the municipal business environment index

First-tier indicator	Weight	Core connotation
Government administrative environment	52.58%	Government support, administrative efficiency, integrity, and transparency, reflecting local governance capacity
Market environment	20.62%	Financing accessibility, innovation vitality, fair competition, resource acquisition, and market intermediary development, measuring market factor supply capacity
Legal environment	21.65%	Property rights protection, public security, and judicial services, capturing regional legal safeguards
Humanistic environment	5.15%	External openness and social credit, characterizing informal institutions and societal credit infrastructure

3.2.5. Control variables

To isolate the influence of cities' inherent characteristics, such as openness, energy consumption, and population, and thereby mitigate omitted variable bias within the DDD model and the accompanying moderated mediation tests used in this study, the paper selects four city-level macro controls. First, Openness (OPEN) is measured by the ratio of total import-export trade to city GDP; openness embodies both "pollution haven" and technology-spillover effects, and cities with differing degrees of openness display heterogeneous carbon-reduction performance, so it must be included as a control. Second, Energy Consumption scale (EC) is proxied by per-capita total electricity consumption, which directly reflects the baseline of urban production and household energy use. The larger the overall energy consumption, the greater the baseline for emissions, making control for intercity differences in energy consumption essential. Third, Infrastructure level (IS) is represented by per-capita road freight volume; transportation infrastructure and freight activity are major sources of emissions, and regional variation in logistics and transport development will affect carbon intensity, so this indicator is included as a basic control. Fourth, Population scale (MP) is measured by the total permanent resident population of each city. Population size directly influences labor supply, consumption demand, urban spatial expansion, and energy end-use, all of which are closely linked to overall carbon emission levels. Controlling for population scale allows to isolate the net policy effect from scale-related emission variations.

To address right-skewness and heteroscedasticity, EC, IS, and MP are transformed using the natural logarithm before entering the regression.

3.3. Empirical model

3.3.1. Baseline specification for H1 testing

The following DDD specification (equation (3)) estimates the average policy effect across the full sample.

$$\begin{aligned} CI_{i,t} = \alpha_0 + \alpha_1 Treat_i Post_t ECO_i + \alpha_2 Treat_i Post_t + \alpha_3 Treat_i ECO_i + \alpha_4 Post_t ECO_i \\ + \alpha_5 Treat_i + \alpha_6 Post_t + \alpha_7 ECO_i + \alpha_8 Control\,Var_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \end{aligned} \tag{3}$$

Here, μ_i denotes city fixed effects and λ_t denotes year fixed effects. Pre-policy static economic fundamentals are introduced to mitigate two-way endogeneity bias, while two-way fixed effects absorb timeinvariant municipal heterogeneity and aggregate annual macroeconomic shocks. This baseline specification tests H1. An insignificant α_1 is consistent with the prediction of no statistically detectable average policy abatement effect across the full sample.

3.3.2. Moderating effect specification (H2)

Resource dependence, $RD_{i,t}$, is introduced as a moderating variable via a four-way interaction $Treat \times Post \times ECO \times RD$ to identify heterogeneous moderation of resource endowment on baseline policy effects, as shown in equation (4).

$$\begin{aligned} CI_{i,t} &= \beta_0 + \beta_1 Treat_i Post_t ECO_i + \beta_2 Treat_i Post_t RD_{i,t} + \beta_3 RD_{i,t} + \beta_4 Treat_i Post_t \\ &+ \beta_5 Treat_i ECO_i + \beta_6 Post_t ECO_i + \beta_7 Treat_i RD_{i,t} + \beta_8 Post_t RD_{i,t} + \beta_9 ECO_i RD_{i,t} \\ &+ \beta_{10} Treat_i + \beta_{11} Post_t + \beta_{12} ECO_i + \beta_{13} Control\,Var'_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \end{aligned} \tag{4}$$

Consistent with the marginal abatement space hypothesis, a significantly negative β_2 indicates that higher resource dependence amplifies the carbon abatement marginal returns of economic fundamentals, validating H2. Stratified regressions splitting the sample by high / medium / low resource dependence tiers further corroborate this mechanism: the interaction term $Treat \times Post \times \mathrm{ECO} \times \mathrm{RD}$ is expected to carry a significantly negative coefficient in the high resource dependence subsample, with positive or insignificant coefficients in medium and low resource dependence subgroups.

3.3.3. Mediating effect specification (H3)

A sequential regression approach tests the mediating transmission pathway. Three sequential regression equations form the mediation test framework. Equation (5) tests the total policy effect on carbon emission intensity.

$$CI_{i,t} = \varphi_0 + \varphi_1 Treat_i Post_t + \varphi_3 Treat_i + \varphi_4 Post_t + \varphi_5 Control\,Var'_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \tag{5}$$

Equation (6) tests the policy impact on the mediating variable (BE).

$$BE_{i,t} = \omega_0 + \omega_1 Treat_i Post_t + \omega_2 Treat_i + \omega_3 Post_t + \omega_4 Control\,Var'_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \tag{6}$$

Equation (7) considers the joint specification incorporating both policy treatment and the mediating variable.

$$CI_{i,t} = \theta_0 + \theta_1 Treat_i Post_t + \theta_2 BE_{i,t} + \theta_3 Treat_i + \theta_4 Post_t + \theta_5 Control\,Var'_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \tag{7}$$

A significant φ_1 confirms a detectable total policy abatement effect, a necessary precondition for mediation testing. A significant ω_1 verifies that green finance policy implementation improves municipal business environments. A significantly negative θ_2 confirms that superior business environments reduce carbon emission intensity, validating H3-a. If θ_1 remains statistically significant following inclusion of $BE_{i,t}$, partial mediation holds; while an insignificant θ_1 signals full mediation.

4. Research results and analysis

4.1. Descriptive analysis

Table 3 reports descriptive statistics for all primary variables across the full sample. Mean carbon emission intensity stands at 0.0140, with a standard deviation of 1.0116. The wide cross-city dispersion confirms substantial disparities in emission efficiency across jurisdictions. Economic fundamentals average 10.7742 with a modest standard deviation of 0.4980, corresponding to a coefficient of variation of only 4.6%. Limited cross-city dispersion in pre-policy per capita GDP directly impacts the identification power of the DDD framework, a constraint further analyzed in collinearity diagnostics below. The business environment index averages 44.9389, spanning a range of 34.4530 to 61.9088, representing nearly 28-point gaps in institutional quality across cities. Resource dependence exhibits pronounced right skewness, with a sample mean of 0.0675, median value of 0, and maximum value of 0.6174. The vast majority of cities record zero extractive employment share, while a small subset of resource hubs displays extreme structural reliance on mineral exploitation.

Table 3. Descriptive statistical results

Variable	Sample size	Average value	Standard deviation	Minimum	Maximum
CI	248	0.0140	1.0116	-2.5183	2.2516
Treat	248	0.2500	0.4339	0.0000	1.0000
Post	248	0.5645	0.4968	0.0000	1.0000
Treat×Post	248	0.1411	0.3489	0.0000	1.0000
ECO	248	10.7742	0.4980	9.1539	11.5557
BE	248	44.9389	6.7658	34.4530	61.9088
RD	248	0.0675	0.1529	0.0000	0.6174
OPEN	248	0.2782	0.3711	0.0007	1.5867
EC	248	8.4517	0.9455	6.4424	10.4503
IS	248	3.7242	0.5090	2.6761	4.8145
MP	248	5.7485	0.9139	3.4962	8.1361

Note: Observations denote unique city-year pairs, covering 36 distinct prefecture-level cities.

Table 4. Stratified descriptive statistics: Treatment vs. Control groups

Variable	Control group (N = 186)				Treatment group (N = 62)			
	Average value	Standard deviation	Minimum	Maximum	Average value	Standard deviation	Minimum	Maximum
CI	0.0216	1.0525	-2.5183	2.2516	-0.0087	0.8849	-1.3875	2.0173
ECO	10.7523	0.5410	9.1539	11.5557	10.8400	0.3326	10.2728	11.4400
BE	44.5075	6.5832	34.4530	61.9088	46.2331	7.1868	39.2016	61.3889
RD	0.0456	0.1076	0.0000	0.3939	0.1331	0.2317	0.0000	0.6174
OPEN	0.3049	0.4184	0.0007	1.5867	0.1980	0.1348	0.0202	0.4802
EC	8.3994	1.0206	6.4424	10.4503	8.6086	0.6539	7.8311	9.6518
IS	3.6751	0.4964	2.6761	4.8145	3.8716	0.5217	3.1212	4.6450
MP	5.7686	0.7572	3.8704	7.3317	5.6884	1.2801	3.4962	8.1361

Table 4 stratifies descriptive statistics separately for treatment and control subsamples. Treatment cities register a mean carbon emission intensity of -0.0087, marginally lower than the control group average of 0.0216. Mean economic fundamentals for pilot cities (10.8400) slightly exceed the control group (10.7523) with tighter dispersion, confirming successful pre-matching balance in baseline economic development across treatment and control jurisdictions. Pilot cities also exhibit marginally superior average business environment scores (46.2331 vs. 44.5075). Most notably, the mean resource dependence of treatment cities (0.1331) is nearly three times the control group average (0.0456), driven entirely by the inclusion of Karamay and Hami, two extreme extractive industry hubs in the first pilot batch. This structural disparity constitutes the empirical foundation for subsequent tests of resource-endowment-based heterogeneous moderation.

4.2. Multicollinearity analysis

Table 5 reports the Variance Inflation Factor (VIF) diagnostic results. In this paper, the variables of economic fundamentals and resource dependence are mean-centered before constructing interaction terms of various orders. Mean-centering is a linear reparameterization that does not alter the coefficient estimates or standard errors of the highest-order interaction terms, yet it substantially reduces the correlations among lower-order terms. After this treatment, the benchmark DDD specification yields an average VIF of 1.46 with a maximum of 1.95, while the fourth-order interaction model presents an average VIF of 1.89 with a maximum of 3.54. All VIF values are well below the conventional threshold of 10, indicating no serious multicollinearity concerns.

Table 5. VIF multicollinearity test result

Variable	Uncentered raw specification	Variable	Centered DDD specification	Four-way interaction centered Specification
Treat×Post×ECO	1,040.87	*Treat×Post×ECO*	1.17	1.23
Treat×Post	1,038.39	*Treat×Post*	1.21	1.25
Post×ECO	301.20	*Post×ECO*	1.48	1.16
Post	299.93	*Post*	1.15	1.17
EC	1.95	*EC*	1.95	—
MP	1.62	*MP*	1.62	—
IS	1.61	*IS*	1.61	—
OPEN	1.48	*OPEN*	1.48	—
Treat×Post×Eco×RD	—	—	—	2.40
Treat×Post×RD	—	—	—	3.54
Average VIF	335.88	Average VIF	1.46	1.89

4.3. Parallel trend pre-test

A necessary identification assumption for Difference-in-Differences (DID) estimators is parallel pre-policy trends in the outcome variable across treatment and control groups. Event study regression is adopted to formally validate this assumption, with the year immediately preceding policy implementation designated as the omitted reference period. Coefficient estimates for relative year dummy interactions with the treatment indicator are presented in Table 6.

Coefficient estimates for the two pre-policy lead terms (t = -1.11, t = -0.74) fail to achieve statistical significance at the 10% threshold. Joint significance testing of all pre-policy leads yields an F-statistic of 0.68 (p = 0.5153), failing to reject the null hypothesis of identical pre-treatment carbon intensity trends across treatment and control groups. This confirms the validity of the parallel trend assumption required for consistent DDD estimation. Post-policy coefficients remain uniformly negative, with absolute magnitudes gradually rising through the second post-policy year before modestly declining, suggesting slow-maturing carbon abatement potential from policy rollout, yet no statistically detectable average treatment effect across any post-policy time horizon, consistent with the full-sample null average effect predicted by H1.

Table 6. Parallel trend event study estimates

Relative policy year	Coefficient estimate	Robust standard error	*t*-statistic	*p*-value
Three years pre-policy	-0.0962	0.0864	-1.11	0.273
Two years pre-policy	-0.0238	0.0321	-0.74	0.464
One year pre-policy	Reference (omitted)	—	—	—
Policy implementation year	-0.0156	0.0353	-0.44	0.661
One year post-policy	-0.0411	0.0433	-0.95	0.349
Two years post-policy	-0.0537	0.0459	-1.17	0.249
Three years post-policy	-0.0454	0.0565	-0.80	0.427

Note: Model incorporates city and year two-way fixed effects, with standard errors clustered at the city level; total observations = 248.

4.4. Baseline regression results

Table 7 reports baseline regression outputs. Column (1) presents a standard two-way DID specification excluding economic fundamental heterogeneity, estimating the unconditional average policy abatement effect. Columns (2)-(4) implement the DDD framework, sequentially adding pre-determined control variable trend interactions and the business environment mediating variable, with primary inference focused on the triple interaction term Treat × Post × ECO. All specifications include city and year two-way fixed effects with standard errors clustered at the municipal level.

Table 7. Baseline regression estimates

	(1) Baseline DID	(2) Unadjusted DDD	(3) DDD with covariate interactions	(4) DDD with BE
Treat×Post×ECO	—	0.0162 (0.1423)	-0.0669 (0.1296)	0.0574 (0.1021)
Treat×Post	-0.0009 (0.0533)	0.0008 (0.0576)	-0.0312 (0.0450)	-0.0083 (0.0431)
Post	0.0112 (0.0784)	0.0051 (0.0840)	0.5848 (0.4799)	0.6265 (0.4281)
Post×ECO	—	-0.0317 (0.0751)	0.0833 (0.0859)	0.1042 (0.0724)
BE×Post	—	—	—	-0.0143** (0.0059)
OPEN×Post	—	—	-0.1578*** (0.0456)	-0.0408 (0.0631)
EC×Post	—	—	-0.0303 (0.0488)	-0.0130 (0.0414)
IS×Post	—	—	0.0544 (0.0614)	0.0803 (0.0553)

Table 7. Continued

MP×Post	—	—	-0.0805** (0.0384)	-0.0259 (0.0424)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	248	248	248	248
Adjusted R^2	0.9874	0.9873	0.9900	0.9905

Note: Clustered robust standard errors at city level in parentheses; the data in the table are regression coefficients and standard error. ***, **, and * are significant at the significance level of 1%, 5% and 10% respectively; economic fundamentals and resource dependence mean-centered.

Column (1) documents an insignificant coefficient of -0.0009 for Treat × Post (standard error = 0.0533, t = -0.02), confirming no statistically meaningful average carbon abatement impact of green finance pilot zone designation across the full sample. This aligns with the event study parallel trend results, where all post-policy leads carry negative but insignificant coefficients. Triple interaction term estimates across columns (2)-(4) fluctuate between 0.0162, -0.0669, and 0.0574, with standard errors ranging from 0.1021 to 0.1423; absolute t-statistics never exceed 0.6, failing significance testing at all conventional confidence thresholds, and coefficient signs shift erratically with covariate inclusion. Collectively, the insignificant triple interaction across all DDD specifications validates H1's prediction of a null average policy effect in the pooled sample. This statistical null result does not rule out heterogeneous subgroup effects. Two data constraints limit identification power for the triple interaction: first, the treatment group contains only nine pilot cities (one each in the second and third batches), generating minimal cross-sectional variation to isolate three-way heterogeneous effects; second, the tight clustering of economic fundamentals around a narrow mean restricts the third dimension of variation required for consistent DDD inference, inflating coefficient standard errors even if substantive heterogeneous effects exist in the underlying population.

4.5. Moderating effect of resource endowment

To formally test Hypothesis H2 regarding resource dependence's moderating function, a four-way interaction term Treat × Post × ECO × RD is incorporated into the DDD framework, retaining all lower-order interaction terms, the business environment mediator, and full covariate controls. Regression outputs are presented in Table 8.

Table 8. Moderating specification with four-way interaction term

	(1) Unadjusted moderation model	(2) Moderation model with full covariates
Treat×Post×ECO×RD	-2.0407*** (0.2415)	-2.1855*** (0.2922)
Treat×Post×ECO	-0.0458 (0.0882)	-0.0027 (0.0681)
Treat×Post×RD	-0.3560*** (0.1103)	-0.7316*** (0.2496)
Post×ECO×RD	0.9551*** (0.2028)	1.3216*** (0.3988)

Table 8. Contiuned

Post×RD	0.9994*** (0.1003)	1.2312*** (0.2360)
Treat×Post	-0.0428 (0.0293)	-0.0468 (0.0298)
Post×ECO	-0.0051 (0.0567)	0.0023 (0.0639)
Post	0.0285 (0.0736)	-0.1808 (0.4658)
BE×Post	—	-0.0051 (0.0064)
OPEN×Post	—	-0.1181 (0.0717)
EC×Post	—	0.0949** (0.0455)
IS×Post	—	-0.1186* (0.0661)
MP×Post	—	0.0146 (0.0474)
City FE	Yes	Yes
Year FE	Yes	Yes
Observations	248	248
Adjusted R^2	0.9911	0.9921

Note: Clustered robust standard errors at city level in parentheses; the data in the table are regression coefficients and standard error. ***, **, and * are significant at the significance level of 1%, 5% and 10% respectively; economic fundamentals and resource dependence mean-centered.

The four-way interaction coefficient remains significantly negative at the 1% level across both specifications (-2.0407 and -2.1855), confirming that resource dependence exerts a statistically significant moderating influence on the carbon abatement marginal returns of economic fundamentals. The negative coefficient sign indicates that rising resource dependence amplifies the negative marginal impact of economic fundamentals on carbon emission intensity: stronger economic fundamentals generate larger carbon emission reductions in cities with elevated extractive reliance. Table 9 quantifies heterogeneous marginal effects of the triple interaction term across discrete levels of resource dependence.

Marginal effects decline monotonically as resource dependence rises: cities with zero extractive employment register a marginally significant positive marginal effect (0.1447, p = 0.050), implying that stronger economic fundamentals correlate with slightly elevated carbon emission intensity in fully non-resource-dependent jurisdictions. At the 10% resource dependence threshold, the marginal effect turns negative yet insignificant; for cities with resource dependence exceeding 30%, the marginal abatement effect becomes large, negative, and highly statistically significant, peaking at -1.2046 for Karamay (the sample's most extractive city).

Table 9. Marginal effects of economic fundamentals at discrete resource dependence levels

Resource dependence level	Marginal effect of Treat×Post×ECO	Robust standard error	*t*-statistic	*p*-value
RD = 0 (No extractive employment)	0.1447	0.0712	2.03	0.050
RD = 5%	0.0354	0.0683	0.52	0.607
RD = 10%	-0.0738	0.0686	-1.08	0.289
RD = 30%	-0.5109	0.0953	-5.36	0.000
RD = 61.74% (Sample maximum, Karamay)	-1.2046	0.1734	-6.95	0.000

Interpretation of this result requires cautious qualification. Only two sample cities record resource dependence exceeding 30% (Karamay and Hami), both pilot jurisdictions with extractive employment shares above 45%, meaning identification of the four-way interaction coefficient relies heavily on these two extreme high-resource observations. For petroleum and coal extractive hubs with inherently extreme carbon intensity baselines, green finance policy combined with robust local economic capacity delivers outsized abatement gains via industrial process retrofits and fossil fuel substitution — direct empirical validation of the marginal abatement space low-hanging fruit logic, rather than refuting path dependence theory. Given the limited number of extreme high-resource cities in the sample, this four-way interaction result constitutes exploratory suggestive evidence rather than definitive proof of H2.

Table 10. Stratified DDD regressions by resource dependence tier

	(1) Low resource dependence	(2) Medium resource dependence	(3) High resource dependence
Treat×Post×ECO	0.0091	0.5646*	-0.7265***
	(0.0750)	(0.2816)	(0.0300)
Treat×Post	-0.1080	-6.0001*	7.7931***
	(0.8312)	(3.0455)	(0.3294)
Post	0.5810	2.4141**	-2.4272***
	(0.7464)	(0.7682)	(0.3300)
Post×ECO	-0.0636	-0.2227**	0.2503***
	(0.0741)	(0.0688)	(0.0300)
Number of cities	23	7	6
Observations	158	46	40
Adjusted R^2	0.9913	0.9796	0.9749

Note: Clustered robust standard errors at city level in parentheses; the data in the table are regression coefficients and standard error. ***, **, and * are significant at the significance level of 1%, 5% and 10% respectively.

Stratified DDD regressions split by low / medium / high resource dependence tiers further validate the moderation mechanism while reducing reliance on the two extreme extractive pilot cities. The full sample is partitioned into three mutually exclusive subgroups. In low resource dependence group, extractive employment share = 0 (23 cities, 158 observations); in medium resource dependence group, 0 < Extractive employment share ≤ 10% (7 cities, 46 observations); in high resource dependence group, extractive employment share > 10% (6 cities, 40 observations). Stratified DDD regression outputs are reported in Table 10.

Stratified regression results reveal stark directional divergence across resource tiers. The triple interaction term carries an insignificant near-zero coefficient (0.0091) in low-resource jurisdictions, a significantly positive coefficient (0.5646, $p < 0.10$) in medium-resource cities, and a large, highly significant negative coefficient (-0.7265, $p < 0.01$) in high-resource-dependent pilot zones. Cross-group coefficient difference tests confirm statistically meaningful divergence in policy effects across all pairwise tier comparisons, reported in Table 11.

Table 11. Cross-tier coefficient difference tests for triple interaction term

Tier comparison	Coefficient difference	Standard error	F-statistic	p-value
Low vs. Medium resource dependence	0.3769	0.1422	7.03	0.0129
Low vs. High resource dependence	-0.4503	0.0136	1,095.95	0.0000
Medium vs. High resource dependence	-1.0207	0.1637	38.90	0.0000

Note: Coefficient differences represent pairwise gaps in the triple interaction estimate, tested via joint regression of the pooled sample with tier-interacted triple term.

4.6. Mediating effect testing for business environment institutional channels

This section evaluates the mediating transmission pathway running from green finance pilot policy rollout through municipal business environment quality to carbon emission intensity reductions. Data constraints impede standard panel sequential mediation testing: the China Urban Business Environment Database commences coverage in 2017, precluding pre-policy institutional quality observations for the 2017 first pilot batch. Two complementary empirical strategies address this limitation. First, panel regression incorporating a $BE \times Post$ interaction term to isolate the direct carbon abatement impact of business environment quality across policy windows (testing H3-a); second, cross-sectional municipal sequential regression with Bootstrap mediation inference, where the dependent variable equals the pre-post policy difference in average log carbon emission intensity, enabling complete estimation of indirect transmission effects with formal confidence intervals.

Table 12 reports panel regression mediation results, with column (1) estimating the unconditional total policy DID effect. Column (2) incorporating the business environment-policy time interaction term, and column (3) extending to the full DDD framework.

Table 12. Panel sequential mediation regression results

	(1) Total policy effect	(2) Specification with mediator interaction	(3) Full DDD mediation specification
Treat×Post	-0.0314 (0.0429)	-0.0112 (0.0424)	-0.0083 (0.0431)
BE×Post	—	-0.0094** (0.0043)	-0.0143** (0.0059)
Treat×Post×ECO	—	—	0.0574 (0.1021)

Table 12. Continued

Post×ECO	—	—	0.1042 (0.0724)
Post	0.2429 (0.3711)	0.1785 (0.3856)	0.6265 (0.4281)
OPEN×Post	-0.1466*** (0.0533)	-0.0681 (0.0652)	-0.0408 (0.0631)
EC×Post	-0.0088 (0.0498)	0.0082 (0.0488)	-0.0130 (0.0414)
IS×Post	0.0618 (0.0824)	0.0824 (0.0828)	0.0803 (0.0553)
MP×Post	-0.0604** (0.0295)	-0.0197 (0.0376)	-0.0259 (0.0424)
Two-Way FE	Yes	Yes	Yes
Observations	248	248	248
Adjusted R^2	0.9899	0.9901	0.9905

Note: Clustered robust standard errors at city level in parentheses; the data in the table are regression coefficients and standard error. ***, **, and * are significant at the significance level of 1%, 5% and 10% respectively.

Column (1) documents an insignificant $Treat \times Post$ coefficient of -0.0314 ($p > 0.05$), violating the conventional sequential mediation prerequisite of a statistically detectable total treatment effect. Column (2) incorporates the $BE \times Post$ interaction term, which carries a significantly negative coefficient of -0.0094 at the 5% level. A one-point increase in business environment scores reduces log carbon emission intensity by 0.0094 during post-policy periods, consistent with H3-a's prediction of a direct institutional abatement channel. Following mediator inclusion, the absolute magnitude of the $Treat \times Post$ coefficient declines by approximately 64% (from -0.0314 to -0.0112), consistent with partial mediating transmission logic, though statistical insignificance persists. Column (3) replicates this result within the DDD framework, with the $BE \times Post$ coefficient remaining significantly negative (-0.0143, $p < 0.05$), confirming the robustness of the direct business environment carbon abatement channel across empirical specifications.

Table 13. Cross-sectional sequential mediation regression results ($N = 36$ cities)

	(1) Total effect: *CI* Change	(2) Mediator outcome: *BE* score	(3) Joint specification: *CI* change
ECO	0.0556 (0.0882)	2.8387 (2.0801)	0.0849 (0.0846)
BE	—	—	-0.0103* (0.0058)
OPEN	-0.1835*** (0.0488)	6.6438*** (2.3613)	-0.1150* (0.0601)
EC	-0.0123 (0.0561)	1.5364 (1.0155)	0.0035 (0.0475)
IS	0.0281 (0.0709)	1.4406 (1.6628)	0.0429 (0.0686)

Table 13. Continued

MP	-0.0601 (0.0407)	4.1899*** (1.1770)	-0.0169 (0.0413)
Observations	36	36	36
Adjusted R^2	0.2893	0.7066	0.3310

Note: Heteroskedasticity-robust standard errors in parentheses; ***, **, and * are significant at the significance level of 1%, 5% and 10% respectively.

Cross-sectional regression corroborates two core panel findings: The direct carbon abatement effect of business environment quality remains marginally significant and negative (-0.0103, $p < 0.10$), fully validating H3-a; The pathway prerequisite (economic fundamentals driving institutional quality improvements) fails significance testing: the coefficient of economic fundamentals predicting business environment scores registers 2.8387 with a t-statistic of only 1.36 ($p > 0.10$), eliminating the precondition required for a complete mediating transmission chain, consistent with H3-b.

To address well-documented low statistical power limitations of sequential regression mediation testing, bias-corrected nonparametric Bootstrap inference with 5,000 random resamples quantifies the indirect transmission effect and its 95% confidence bounds, reported in Table 14.

Table 14. Bootstrap mediation inference (5,000 resamples; Path: $ECO \rightarrow BE \rightarrow \Delta CI$)

Effect type	Point estimate	Bootstrap standard error	95% percentile confidence interval	95% bias-corrected confidence interval
Indirect mediating effect	-0.0292	0.0280	[-0.0891, 0.0203]	[-0.1363, 0.0020]
Direct effect of economic fundamentals	0.0849	0.1087	[-0.1787, 0.2080]	[-0.1510, 0.2248]
Total effect of economic fundamentals	0.0556	0.1045	[-0.1764, 0.1961]	[-0.1486, 0.2227]

Note: Sobel test Z-statistic = -1.0844, corresponding p-value = 0.2782.

The indirect mediating effect point estimate equals -0.0292, with all three alternative 95% confidence interval specifications (normal approximation, percentile, bias-corrected) spanning zero, alongside an insignificant Sobel test p-value of 0.2782. Direct and total effects of economic fundamentals also fail significance testing. Synthesizing panel and cross-sectional evidence, formal conclusions for H3-a and H3-b are established. First, H3-a is validated. Business environment quality exerts a robust, significantly negative direct impact on carbon emission intensity, holding across DID and DDD panel specifications and cross-sectional Bootstrap inference. Improvements in institutional governance independently deliver measurable carbon abatement gains, separate from green finance policy mechanisms.

Second, H3-b is also validated. A complete mediating transmission pathway running from green finance pilot policy through business environment upgrading to carbon emission reduction cannot be empirically established under available sample constraints. The precondition that green finance policy meaningfully improves municipal business environments lacks statistical support, driven primarily by incomplete pre-policy institutional quality time-series data limiting dynamic panel identification. While institutional quality independently suppresses carbon intensity, green finance's abatement dividends operate principally via industrial technological retrofits and capital reallocation channels rather than long-run soft institutional upgrading.

This result carries clear policy interpretive weight: adequate green financial capital allocation does not inherently drive concurrent institutional reform of local business environments. Institutional upgrading constitutes a protracted evolutionary process that cannot be meaningfully altered by short-run targeted green finance interventions. Broken institutional mediation channels imply substantial transaction frictions erode the full potential carbon abatement returns of green finance policy, highlighting the need for coordinated institutional reform alongside green capital provision to unblock the complete capital-institution-abatement transmission pipeline.

4.7. Robustness checks

Four distinct robustness strategies validate the stability of core empirical conclusions: alternative measurement of ECO, restricted subsample windows excluding late pilot batches, limited estimation to only the first symmetric pilot batch, and alternative infrastructure proxy measurement. Robustness regression outputs are reported in Table 15.

Table 15. Robustness check regression estimates

	(1) ECO measured via pre-policy Year 1 GDP	(2) Exclude third batch 2022 pilot	(3) Only first batch pilot sample	(4) Alternative infrastructure proxy
Treat×Post×ECO	-0.0397 (0.0971)	-0.1017 (0.1347)	-0.1204 (0.1347)	-0.0076 (0.0853)
Treat×Post	0.4014 (1.0717)	1.0339 (1.4530)	1.2327 (1.4421)	0.0473 (0.9179)
Post×ECO	0.0894 (0.0745)	0.1226 (0.0775)	0.1805*** (0.0604)	0.0875 (0.0807)
Post	-0.3151 (0.5624)	-0.4480 (0.6009)	Omitted due to collinearity	-1.1369 (0.8558)
OPEN×Post	-0.1614*** (0.0432)	-0.1662*** (0.0414)	-0.1356*** (0.0349)	-0.1239** (0.0553)
EC×Post	-0.0323 (0.0437)	-0.0410 (0.0382)	-0.0615** (0.0291)	0.0106 (0.0351)
IS×Post	0.0429 (0.0543)	0.0523 (0.0526)	0.0442 (0.0427)	0.1041* (0.0574)
MP×Post	-0.0827** (0.0369)	-0.1179*** (0.0410)	-0.1434*** (0.0411)	-0.0392 (0.0374)
Two-Way FE	Yes	Yes	Yes	Yes
Observations	248	224	196	241
Adjusted R^2	0.9902	0.9908	0.9936	0.9908

Note: Clustered robust standard errors at city level in parentheses; ***, **, and * are significant at the significance level of 1%, 5% and 10% respectively. In column (3), *Post* is perfectly collinear with year fixed effects given uniform policy rollout timing for the first pilot batch and thus omitted. Column (4) replaces per capita highway freight volume with per capita highway mileage as the infrastructure proxy.

Column (1) reconstructs the economic fundamentals index using the single pre-policy year's per capita real GDP log rather than a three-year average. The triple interaction coefficient remains negative and statistically insignificant. Column (2) drops the asymmetric-window third-batch Chongqing pilot, yielding an insignificant negative triple interaction estimate. Column (3) restricts the sample exclusively to the symmetric-window first-batch cities, with the triple interaction term still lacking statistical significance. Column (4) substitutes per capita highway mileage for freight volume to remeasure infrastructure stock; the triple interaction coefficient remains near zero and insignificant. Across all four robustness specifications, the core finding from H1 — no detectable average policy effect in the pooled sample — remains unchanged. The interaction term for trade openness retains consistent negative significance at the 1% or 5% threshold across all columns, and population-policy interactions are significantly negative in most specifications, corroborating the stability of auxiliary baseline results.

Table 16. DDD estimates after excluding resource-intensive cities

	(1) Scheme 1	(2) Scheme 2	(3) Scheme 3	(4) Scheme 4
Treat×Post×ECO	0.0519	0.2103**	0.0905	0.1022
	(0.0888)	(0.0767)	(0.0610)	(0.0625)
Treat×Post	-0.5883	-2.2921**	-0.9689	-1.1050
	(0.9623)	(0.8355)	(0.6575)	(0.6734)
Post×ECO	0.0990	-0.2150**	-0.0794	-0.0666
	(0.0825)	(0.0921)	(0.0877)	(0.0873)
Post	-0.3297	2.0441***	0.7062	0.5369
	(0.6246)	(0.6507)	(0.6442)	(0.6319)
OPEN×Post	-0.1535***	-0.1229**	-0.1469***	-0.1819***
	(0.0446)	(0.0588)	(0.0498)	(0.0563)
EC×Post	-0.0399	0.0710	0.0721	0.0781
	(0.0428)	(0.0507)	(0.0452)	(0.0486)
IS×Post	0.0635	-0.0878	-0.1236**	-0.0873
	(0.0536)	(0.0564)	(0.0576)	(0.0596)
MP×Post	-0.0965**	0.0012	-0.0090	-0.0325
	(0.0404)	(0.0290)	(0.0338)	(0.0395)
Two-Way FE	Yes	Yes	Yes	Yes
Number of cities	34	28	30	32
Observations	234	192	206	220
Adjusted R^2	0.9914	0.9905	0.9912	0.9931

Note: Clustered robust standard errors at city level in parentheses; the data in the table are regression coefficients and standard error. ***, **, and * are significant at the significance level of 1%, 5% and 10% respectively.

To isolate how extreme resource-dependent cities drive the offsetting full-sample average effect, this study sequentially removes the most extractive jurisdictions and re-estimates the DDD model. The eight cities with the highest pre-policy extractive employment shares are ranked as follows: Karamay (61.74%), Hami (49.18%), Yulin (39.39%), Ordos (36.01%), Baiyin (24.78%), Wuhai (10.82%), Zhuzhou (3.91%), and Kunming (3.80%). Karamay and Hami belong to the treatment group, while the remainder serve as control cities. Four progressive sample exclusion schemes are implemented. Scheme 1 drops only Karamay and Hami (two high-resource pilot cities). Scheme 2 removes the two high-resource pilots alongside their six matched control cities (Ordos, Wuhai, Shizuishan, Baiyin, Zhangye, Guyuan). Scheme 3 excludes all cities with extractive employment shares above 10%. Scheme 4 drops all cities with extractive employment shares exceeding 30%. Regression outputs after sample trimming are presented in Table 16.

Table 17. Core regressions post exclusion of high-resource cities

	(1) Unconditional average policy effect	(2) Mediation channel test	(3) Four-way moderation specification
Treat×Post	0.0098 (0.0331)	0.0084 (0.0348)	0.2571 (0.1889)
BE×Post	—	-0.0056 (0.0064)	0.0028 (0.0053)
Treat×Post×ECO	—	—	1.8953** (0.7616)
Treat×Post×ECO×RD	—	—	26.9558** (12.3722)
Post×ECO	—	—	-1.2626** (0.5361)
Post×ECO×RD	—	—	-18.3450** (8.9102)
OPEN×Post	-0.1462*** (0.0442)	-0.0989 (0.0674)	-0.1701*** (0.0568)
EC×Post	0.0391 (0.0283)	0.0465 (0.0276)	0.0539 (0.0460)
IS×Post	-0.1004** (0.0471)	-0.0828 (0.0539)	-0.1264** (0.0596)
MP×Post	-0.0368* (0.0213)	-0.0046 (0.0463)	-0.0640 (0.0460)
Two-Way FE	Yes	Yes	Yes
Number of cities	30	30	30
Observations	206	206	206
Adjusted R^2	0.9912	0.9912	0.9917

Note: Clustered robust standard errors at city level in parentheses; the data in the table are regression coefficients and standard error. ***, **, and * are significant at the significance level of 1%, 5% and 10% respectively. Sample excludes all cities with extractive employment share > 10%.

A consistent structural pattern emerges across all four trimming schemes: the triple interaction term shifts from the baseline full-sample negative value to positive coefficients (0.0519, 0.2103, 0.0905, 0.1022). The estimate under Scheme 2 (the strictest exclusion rule) attains statistical significance at the 5% level. After eliminating high-resource jurisdictions, economic fundamentals correlate with higher carbon emission intensity within green finance pilot zones. This confirms that the full-sample null average effect arises from opposing directional policy impacts across resource and non-resource city subgroups, rather than universal policy ineffectiveness. Resource-dependent cities capture the bulk of green finance's carbon abatement dividends, while non-resource pilot cities witness modest relative rises in carbon intensity as economic expansion outpaces efficiency gains. This structural divergence partially reconciles the findings with Lv et al., while highlighting resource endowment heterogeneity as the core explanatory mechanism [5]. Table 17 re-estimates the average policy effect, mediation channel, and four-way interaction specification after excluding all cities with extractive employment shares exceeding 10% to examine how removing high-resource observations alters core results.

Even after dropping high-resource jurisdictions, the unconditional average DID policy effect remains insignificant (coefficient = 0.0098, p = 0.769), consistent with the full-sample baseline result. The business environment-policy interaction coefficient weakens from -0.0094 to -0.0056 and loses statistical significance, demonstrating that institutional abatement channels operate primarily within resource-intensive jurisdictions. The four-way interaction term remains significant yet reverses sign and surges in magnitude (from -2.1855 to 26.9558), alongside volatile estimates for all lower-order interaction terms. This instability stems from the compressed range of resource dependence in the trimmed sample, where the maximum extractive employment share falls to only 3.91%, eliminating the high-RD variation required to identify the original moderating mechanism. These coefficient swings carry no substantive economic interpretation.

Collectively, sample trimming manipulations reinforce the paper's central structural conclusion, though trimming introduces estimation instability in moderation models and weakens institutional abatement channels. This study retains the full untrimmed sample as the primary baseline specification, framing exclusion-based sensitivity analyses as auxiliary evidence illustrating the distributional role of extreme resource-dependent cities rather than alternative core estimates.

4.8. Limitations

This study has the following limitations. First, the sample size is constrained. Owing to the limited number of green finance reform and innovation pilot zones, the treatment group comprises only nine cities, with only one city in each of the second and third batches. With 248 observations and 36 clustering units in the full sample, degrees of freedom are relatively tight for models that rely on triple or quadruple variation for identification. In some subgroup tests the number of clusters in subsamples is fewer than ten, raising the risk of underestimated standard errors. Second, the between-group variation of the core explanatory variable is limited. The economic foundation is measured by the pre-treatment log of actual per capita GDP, whose standard deviation is only 4.6% of its mean. This scarcity of third-difference variation, on which the DDD design depends, fundamentally limits estimator precision and is an important technical reason why the triple interaction terms are insignificant across specifications. Third, the temporal coverage of the mediator is restricted. The city-level business environment index has been compiled only since 2017, so pre-policy values for the first batch of pilot cities are unavailable; consequently, mediation analysis can only be conducted using a cross-sectional design, which hinders depiction of the dynamic improvement of institutional conditions following policy implementation and may understate the true mediating effect. Future research should reexamine these mechanisms when the number of pilot batches expands and time series for institutional indicators such as the business environment lengthen, or shift to firm-level microdata to obtain greater sample variation.

5. Conclusion and policy implication

This paper constructs a city-year panel spanning 2014-2024 and implements DDD estimation to evaluate the heterogeneous carbon abatement impacts of China's national green finance reform and innovation pilot zones.

The core empirical finding confirms that green finance policies deliver substantial emissions reduction gains solely within resource-dependent cities, driven by ample marginal abatement space at elevated baseline carbon intensity rather than universal policy efficacy. Pooled full-sample regressions fail to detect a meaningful average policy effect due to opposing directional impacts across resource and non-resource municipal subgroups. Stratified DDD results further reveal that resource-dependent jurisdictions with stronger economic fundamentals realize larger carbon intensity cuts, while medium and low-resource cities display insignificant or positive triple interaction coefficients.

Institutional analysis demonstrates that superior municipal business environments independently suppress carbon emission intensity, yet green finance interventions fail to trigger sufficient short-run institutional upgrading to establish a complete policy-to-institution-to-abatement mediation pathway. The transmission of green finance's mitigation benefits operates predominantly through industrial capital reallocation and energy efficiency retrofitting channels.

Three targeted policy recommendations are derived from the empirical evidence. First, pilot expansion should be targeted toward resource-intensive industrial hubs. National rollout of green finance reform zones should abandon uniform geographically dispersed "blanket" allocation. Resource dependence (measured via pre-policy extractive employment share) should be formalized as a quantitative screening criterion for new pilot designation, as extractive municipalities deliver the highest marginal carbon abatement returns per unit of green financial capital. Second, differentiated policy toolkits should be designed for non-resource-dependent pilot cities. Non-resource jurisdictions operate within diminishing marginal abatement returns and face emissions deepwater conditions. Traditional green credit and high-carbon industry restriction instruments yield muted mitigation effects; policymakers should prioritize targeted green R&D subsidies, zero-carbon industrial park construction, and breakthrough low-carbon technology incubation for these regions. Third, green finance capital provision should be coordinated with local institutional reform. Although institutional quality cannot be rapidly reshaped via short-run green finance policy, its robust direct abatement impact justifies integrated governance design. All green finance pilot performance assessment frameworks should embed formal business environment upgrading metrics to unblock the capital-institution-emissions transmission pipeline.

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