



Article

Low-Carbon City Pilot Policy and Corporate Green Innovation: A Quasi-Natural Experiment Based on Chinese A-Share Listed Companies

Shiyu Wang

School of Economics, Hefei University of Technology, Hefei 230601, China; 15962330114@163.com

How To Cite: Wang, S. Low-Carbon City Pilot Policy and Corporate Green Innovation: A Quasi-Natural Experiment Based on Chinese A-Share Listed Companies. *Ecological Economics and Management* 2026, 2(4), 16. <https://doi.org/10.53941/eem.2026.100016>

Received: 14 August 2026
 Revised: 23 September 2026
 Accepted: 8 October 2026
 Published: 14 October 2026

Abstract: Whether environmental regulation “forces” or “crowds out” corporate green innovation is both the core proposition of the four-decade Porter hypothesis debate and a practical concern for policy design under China’s “dual-carbon” goals. Taking the low-carbon city pilots established in three waves in 2010, 2012, and 2017 as a quasi-natural experiment, this paper uses panel data of 4957 Chinese A-share listed companies over 2008–2023 and a staggered difference-in-differences (DID) model to systematically examine the effect of the pilot policy on corporate green invention patent applications. The findings are as follows. (1) The low-carbon city pilot significantly promotes corporate green invention patent applications, raising them by about 5.6% on average for firms in pilot cities, which supports the “forcing” mechanism of the weak Porter hypothesis. (2) Financing constraints are the key threshold determining whether firms benefit: the policy effect is significantly positive for firms with lower financing constraints, whereas resource-constrained firms obtain no significant improvement. This study provides direct micro-level evidence for the “conditional validity” of the Porter hypothesis in the Chinese context: the policy produces a “forcing” effect on average, but the distribution of its benefits is moderated by firm resource endowments, and an innovation-offset threshold exists—firms that cross it benefit, while those that do not show no significant effect. The paper provides new evidence from Chinese firms on the relationship between environmental regulation and corporate green innovation and offers implications for differentiated environmental policy design.

Keywords: low-carbon city pilot; green innovation; Porter hypothesis; staggered difference-in-differences; firm heterogeneity

1. Introduction

In July 2010, the National Development and Reform Commission (NDRC) issued a notice launching the first wave of low-carbon pilots in five provinces and eight cities, including Guangdong and Liaoning. For firms in pilot cities, the policy made clear that total carbon emissions and emission intensity would gradually be constrained, so that a green transition had become inevitable. Faced with new compliance costs and policy signals, firms must choose: to continue carbon-intensive production and concentrate resources on expanding output, or to invest early in green technologies and patents and turn environmental constraints into innovation opportunities. Behind this choice lies precisely the proposition that environmental economics has debated for four decades: does environmental regulation “force” or “crowd out” corporate innovation?

The claim that “strict but well-designed environmental regulation can improve both the environment and competitiveness by stimulating innovation” has triggered a lasting debate [1–4]. The traditional view holds that pollution-reducing regulation inevitably raises compliance costs and crowds out R&D resources, thereby



suppressing innovation [3]; the Porter hypothesis instead emphasizes that the “innovation offsets” released by regulation can partly or even fully offset compliance costs [2]. Which mechanism dominates depends on the regulatory design and on whether firms can cross the innovation-offset threshold.

China is undergoing an environmental governance experiment of unprecedented scale. To implement carbon emission control targets and promote green development, the NDRC approved low-carbon city pilots in three waves, in 2010, 2012, and 2017, covering hundreds of cities. Oriented toward the dual control of total carbon emissions and emission intensity, this policy created clear compliance expectations for firms and constitutes an ideal quasi-natural experiment for testing the Porter hypothesis: firms in pilot and non-pilot cities faced a similar institutional environment before the policy, while the staggered, gradual rollout provides time-dimension variation for identification.

However, existing evidence is still insufficient to answer how the pilot affects firm-level green innovation. Most low-carbon pilot studies remain at the city or provincial macro level [5–8], and firm-level evidence remains relatively scarce; the few studies that touch on corporate green innovation [9–11] also rarely distinguish, within a unified framework, how the “forcing” and “crowding-out” mechanisms are distributed across firm types; moreover, staggered DID may be biased under heterogeneous treatment effects [12–15], a challenge that some earlier studies have not fully addressed. In this respect, the impact of the low-carbon pilot on corporate green innovation is far from settled.

Using panel data of Chinese A-share listed companies over 2008–2023 and a staggered difference-in-differences method, this paper systematically examines the effect of the low-carbon city pilot on corporate green innovation and its heterogeneity. The paper’s marginal contributions are threefold. First, in identification, it constructs a staggered DID based on the three-wave quasi-natural experiment, accompanied by parallel trends, PSM, and dynamic effect tests, and directly addresses the estimation bias of staggered DID. Second, in evidence, the paper distinguishes green patent types and finds that the policy effect is concentrated in green invention patent applications and grants, whereas green utility-model applications and total green patent applications are both insignificant, indicating that the observed increase more likely reflects a reallocation of innovation resources toward substantive green inventions rather than a net expansion of total green innovation; Third, in mechanism, the paper identifies financing constraints as the resource threshold: firms with low constraints exhibit significantly positive effects while firms with high constraints show no significant gains, providing direct micro-level evidence for the “conditional validity” of the Porter hypothesis.

2. Theoretical Analysis and Research Hypotheses

The core of the four-decade Porter hypothesis debate is not whether environmental regulation affects innovation, but the direction and conditions of that effect. In empirical studies, evidence supporting “forcing” and “crowding out” coexists: rising energy prices induce energy-saving patents [16], and carbon taxes drive clean technology transitions [17,18]; at the same time, stringent regulation has also depressed total factor productivity in manufacturing [19–23]. Since both forces can genuinely exist, under the same policy shock, what types of firms are “forced” and what types are “crowded out”? Where is the boundary between the two?

2.1. Microfoundations of the Forcing Effect

The core claim of the weak Porter hypothesis is that environmental regulation can stimulate innovation [24–26]. Its premise is that regulation conveys new and credible information to firms, prompting them to discover innovation opportunities obscured by existing routines [2]; when managerial inertia and incomplete information are present, the “no free lunch” assumption no longer holds [3], and compliance pressure can be converted into innovation incentives. Evidence from the following two decades broadly supports this logic: pollution abatement expenditure raised R&D and patent applications in US manufacturing [24,27]; evidence from Japanese manufacturing likewise shows that regulatory investment raises productivity [28]; renewable energy policies significantly promote green technology patents [29,30]; and OECD studies distinguishing the weak and strong Porter hypotheses also find that environmental policy has a robust stimulating effect on innovation [31].

China’s low-carbon city pilot provides an ideal setting for testing the weak Porter hypothesis. The pilot takes the dual control of total carbon emissions and emission intensity as its core task and is supported by measures such as low-carbon industry cultivation and green finance innovation, creating compliance expectations for firms. Existing studies find green growth promotion effects at the city level [5], improvements in carbon emission efficiency [6,32], and increases in corporate low-carbon innovation [10,33], while firm-level evidence shows that the policy effect is significant and concentrated among firms with lower financing constraints [34], with public environmental concern serving as an important mediating channel [35]. This paper argues that the pilot policy

“forces” corporate green innovation through two channels. First, the compliance signal. The dual control of total emissions and intensity constitutes a predictable compliance constraint that requires firms to plan technological responses in advance; green patents are both the technological proof of compliance and the credentials for applying for policy subsidies and green finance support. Second, the policy expectation. Supportive measures in pilot cities lower firms’ expected costs of developing green innovation. Accordingly, we propose:

H1: *The low-carbon city pilot policy significantly promotes the green invention patent applications of firms in pilot cities.*

2.2. The Boundary between Forcing and Crowding Out

If the forcing effect held universally, there would be no room for the Porter hypothesis debate. The disagreement arises precisely from the “threshold” nature of innovation offsets. Review studies have long pointed out that the weak Porter hypothesis holds conditional on firm resources, industry characteristics, and the institutional environment [4], and financing constraints are among the most central resource constraints [36–38]. Specifically, this resource threshold consists mainly of two conditions: financing constraints and the sunk costs of innovation investment. When compliance costs exceed the innovation investment that firms can bear and transform, compliance pressure does not “force” but “crowds out” innovation [3,19].

In mechanism, “crowding out” is not a simple contraction at the aggregate level, but the result of two superimposed channels. First, the substitution effect. For resource-constrained firms, compliance costs (pollution abatement investment, emission-standard retrofits) directly crowd out the cash flow available for R&D, forcing R&D expenditure to shrink. Second, the irreversibility effect. Fixed investment in green patent portfolios is sunk in nature: once the funding chain tightens or the policy environment becomes uncertain, early investment is difficult to recover, and firms are more likely to suspend long-term innovation plans. Together, these two channels turn the effect of compliance pressure on corporate green innovation from “forcing” into “crowding out”.

The threshold logic implies a clear stratification of firms under the pilot policy. State-owned firms respond consistently with policy objectives and face small financing constraints, enabling them to bear the upfront investment of green innovation; large firms have abundant resources, can absorb the sunk costs of green patent portfolios, and capture scale-based innovation offsets; heavily polluting firms face the deepest emission constraints and the greatest compliance pressure, giving them the strongest green innovation incentives. Firms with these characteristics are all more likely to cross the innovation-offset threshold. However, type differences are not the essence of the threshold: characteristics such as firm size, ownership, and pollution intensity are highly correlated with resource endowments, so type stratification may merely approximate the resource threshold. A more explanatory stratification should fall directly on firm resource endowments—especially financing constraints. Accordingly, we propose:

H2: *The effect of the policy on corporate green innovation is subject to a resource threshold. Firms with lower financing constraints can cross the threshold and obtain a positive “forcing” effect; firms with higher financing constraints, limited by resources, exhibit no significant policy effect.*

3. Research Design

3.1. Policy Background and Identification Strategy

In 2010, the NDRC issued a notice launching low-carbon province and city pilots, the first wave comprising five provinces and eight cities including Guangdong and Liaoning, with the second and third waves added in 2012 and 2017. Pilot cities undertake tasks such as controlling total carbon emissions and intensity, cultivating low-carbon industries, and innovating green finance. Three features of the policy constitute the basis for identification: the staggered, gradual rollout provides time-dimension variation; the announcement of the pilot list is exogenous, as firms can hardly anticipate the timing of inclusion; and the policy is consistent with the subsequent “dual-carbon” strategy, forming a sustained and credible commitment.

This paper adopts a staggered difference-in-differences design. Firms in pilot cities are the treated group and firms in non-pilot cities the control group, and we compare the relative change in green innovation between the two groups before and after the policy. Identification relies on the parallel trends assumption: absent the policy intervention, green innovation in the treated and control groups would evolve along parallel paths.

3.2. Sample and Data

The sample comprises Chinese A-share listed companies over 2008–2023. After excluding ST and ST* firms, firms in the financial industry, and observations with missing key variables, we obtain 4957 firms and 47,146 firm-year observations. Green patent data come from the patent database of the China National Intellectual Property Administration, with green invention patent applications measuring corporate green innovation and total green patent applications and grants serving as robustness replacements; corporate financial and governance data come from the CSMAR database; and the list of low-carbon pilot cities comes from the NDRC's successive approval documents. We winsorize green invention patent applications, total green patent applications, total green patents granted, firm size, leverage, and return on assets at the 1st and 99th percentiles, and set missing values of environmental regulation intensity to zero; Table 1 reports the descriptive statistics after these treatments.

Table 1. Descriptive statistics of the main variables.

Variable	Definition	Mean	Sd	Min	Max
lngrpat_app_inv	Green invention patent applications, $\ln(1 + x)$	0.567	0.966	0	4.331
lngrpat_app_total	Total green patent applications, $\ln(1 + x)$	0.835	1.171	0	4.796
lngrpat_grant_total	Total green patents granted, $\ln(1 + x)$	0.649	1.000	0	4.159
size	Firm size	22.110	1.315	19.511	26.190
lev	Leverage ratio	0.423	0.213	0.051	0.965
roa	Return on assets	0.036	0.070	−0.304	0.212
age	Firm age (years)	18.183	6.321	0	65
top1	Largest shareholder ownership (%)	34.313	15.258	0.290	100
boardsize	Board size	8.498	1.696	0	18
indratio	Independent director ratio (%)	37.608	5.590	0	100
dual	CEO duality	0.300	0.458	0	1
soe	State-owned enterprise (SOE)	0.343	0.475	0	1
env_reg	Environmental regulation intensity	0.911	0.328	0	11

3.3. Variable Definitions

Dependent variable: Green invention patent applications, taken as $\ln(1 + \text{applications})$, denoted *lngrpat_app_inv*. Green invention patent applications serve as the core indicator because they represent firms' substantive green technological innovation capability; green patent grants are used as a robustness check because they are closer to patent quality.

Core explanatory variable: The policy variable *did* = *treat* × *post*. It equals 1 if the firm's city was included in the low-carbon pilot and the year is at or after the policy implementation year, and 0 otherwise; the three waves take 2010, 2012, and 2017 as the policy start years, respectively.

Control variables: firm size (*size*), leverage (*lev*), profitability (*roa*), firm age (*age*), ownership concentration (*top1*), board size (*boardsize*), proportion of independent directors (*indratio*), CEO duality (*dual*), and environmental regulation intensity (*env_reg*, a city-year measure constructed as the ratio of completed investment in industrial pollution control to the value added of the secondary sector, merged into the firm panel by the firm's city). Among these, corporate governance characteristics are closely related to green innovation decisions [39] and are also controlled for.

3.4. Model Specification

The baseline staggered DID model is specified as:

$$\ln \text{GreenInnovation}_{it} = \alpha_i + \lambda_t + \beta \text{DID}_{it} + \gamma X_{it} + \varepsilon_{it} \quad (1)$$

where α_i is the firm fixed effect, λ_t is the year fixed effect, X_{it} are the control variables, ε_{it} is the random error term. Standard errors are clustered at the firm level. β measures the average treatment effect of the pilot policy on firms' green invention patent applications.

To test parallel trends and dynamic effects, we construct an event-study model:

$$\ln \text{GreenInnovation}_{it} = \alpha_i + \lambda_t + \sum_{k=-3}^5 \beta_k D_{it}^k + \gamma X_{it} + \varepsilon_{it} \quad (2)$$

where D_{it}^k are year dummies relative to the policy timing (with the year before the policy, $k = -1$, as the base period). The pre-policy coefficients β_{-2} and β_{-1} should be insignificant to ensure parallel trends, while the post-policy coefficients characterize the dynamic effects.

4. Baseline Empirical Results

4.1. Baseline Regression

Table 2 reports the baseline regression results. Column (1) controls only firm and year fixed effects, with a did coefficient of 0.0622, significant at the 1% level; column (3) adds firm-level control variables, with a did coefficient of 0.0548 (standard error 0.0211, $t = 2.60$), still significant at the 1% level.

Based on the column (3) estimate, the policy raises the green invention patent applications of firms in pilot cities relative to the control group by about $\exp(0.0548) - 1 \approx 5.6\%$. To verify whether the effect is concentrated in substantive innovation, we re-estimate with total green patent applications as the dependent variable: in column (4), with control variables added, the did coefficient is 0.0374 (standard error 0.0244), not significant. The significance of invention applications combined with the insignificance of total applications indicates that the pilot policy stimulates substantive innovation responses concentrated in green inventions, rather than a generalized expansion of total patent applications. The baseline results support H1.

Table 2. Baseline regression: low-carbon city pilot and corporate green invention patent applications.

	(1) Green Invention Patent Applications	(2) Total Green Patent Applications	(3) Green Invention Patent Applications	(4) Total Green Patent Applications
did	0.0622 *** (0.0224)	0.0440 * (0.0262)	0.0548 *** (0.0211)	0.0374 (0.0244)
size			0.2620 *** (0.0150)	0.3408 *** (0.0170)
lev			−0.0158 (0.0436)	0.0035 (0.0534)
roa			0.0846 (0.0630)	0.1340* (0.0752)
age			0.0271 *** (0.0022)	0.0392 *** (0.0026)
top1			−0.0012 (0.0009)	−0.0023 ** (0.0011)
boardsize			0.0002 (0.0064)	0.0008 (0.0072)
indratio			0.0015 (0.0015)	0.0021 (0.0017)
dual			0.0454 *** (0.0148)	0.0290* (0.0170)
env_reg			0.0002 (0.0124)	0.0012 (0.0139)
Constant	0.0193 (0.0187)	0.0967 *** (0.0217)	−5.7428 *** (0.3265)	−7.4208 *** (0.3698)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	47146	47146	45950	45950
R ²	0.1709	0.2182	0.2092	0.2638

Note: Robust standard errors clustered at the firm level are in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

4.2. Parallel Trends and Dynamic Effects

The premise for the identification strategy to hold is the parallel trends assumption. Figure 1 presents the coefficient path of the event-study estimates.

The coefficients in the periods immediately before the policy (D_{-3} , D_{-2} , and the base period D_{-1}) are all insignificant and very small in absolute value; D_{-5} the coefficient is −0.0925 (standard error 0.0415), significant at the 5% level, but this period pools all years from five years before the pilot and earlier. Testing the four pre-policy coefficients D_{-5} through D_{-2} jointly, the F-statistic is 1.96 ($p = 0.098$), so the null hypothesis that the four coefficients are jointly zero cannot be rejected at the 10% level; the coefficients in the policy year and the early post-policy periods (D_0 , D_1 , D_2) are positive but not yet significant, and from three periods after the policy onward (D_3 , D_4 , D_5) they become significantly positive. This indicates that the treated and control groups exhibit no

systematic trend deviation in the periods immediately before the policy and that anticipatory effects are not a major confounding factor, so that the parallel trends assumption generally holds; the policy effect emerges gradually.

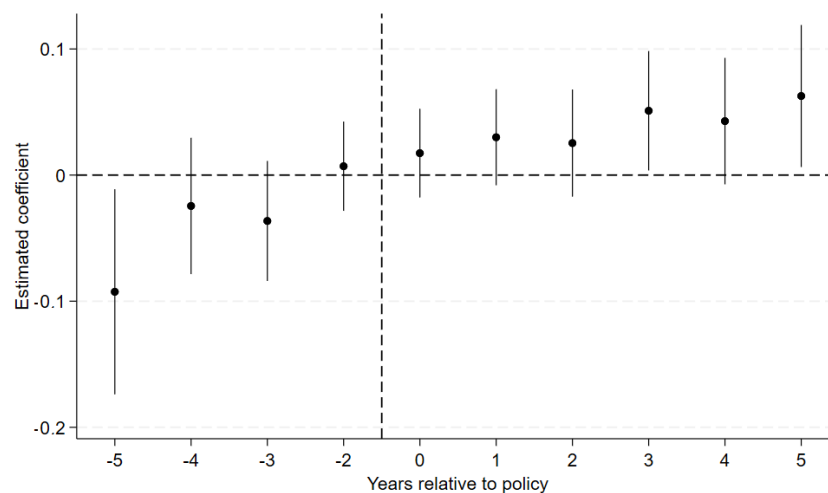


Figure 1. Parallel trends.

To further characterize the time structure of the policy effect, Table 3 combines the post-policy periods into the first year (post1y), the second year (post2y), and the third year and above (post3y), with coefficients of 0.0414, 0.0456, and 0.0623, all significant at least at the 5% level and increasing year by year; Figure 2 visually presents this upward trajectory. This indicates that the innovation effect of the pilot policy is persistent and cumulative.

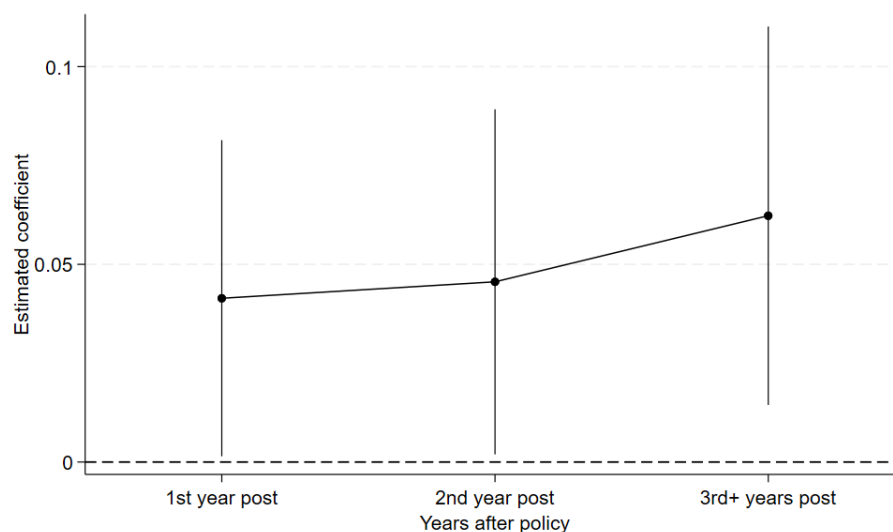


Figure 2. Dynamic effects.

Table 3. Dynamic effects.

	1st Year Post	2nd Year Post	3rd Year and above
Coefficient	0.0414 **	0.0456 **	0.0623 **
	(0.0204)	(0.0223)	(0.0244)

Note: N = 45,950; control variables and fixed effects are the same as in Table 2.

4.3. Robustness Checks

Table 4 reports six robustness checks, each modifying the baseline in one dimension to assess the robustness of the conclusions. (1) Keeping only the first two waves of pilots, to avoid interference from the different policy environment faced by the third wave of 2017; the coefficient is 0.0722, significant at the 1% level. (2) Replacing the dependent variable with granted green invention patents, to mitigate strategic patent applications; the

coefficient is 0.0514, still significant at the 1% level; (3) Province $\times$ year fixed effects, absorbing province-level time-varying policy shocks; the coefficient is 0.0690, significant at the 5% level. (4) Excluding the post-pandemic window, to remove the impact of pandemic shocks; the coefficient is 0.0554, significant at the 1% level. (5) Poisson pseudo-maximum-likelihood (PPML) estimation with the raw count of green invention patent applications as the dependent variable, controlling for firm and year fixed effects and clustering standard errors at the firm level, to address the concern that the $\ln(1 + x)$ transformation is sensitive to the treatment of zeros. The PPML estimate is positive but imprecise, with a did coefficient of 0.0334 and a standard error of 0.0822 ($z = 0.41$), not significant at conventional levels. (6) Clustering standard errors at the city level, to examine the correlation of the policy shock within cities; the coefficient is 0.0538, still significant at the 5% level.

Across the six completed checks, apart from PPML, the TWFE estimates of the DID coefficient in the remaining five specifications remain stable in the 0.051–0.072 range and are significantly positive, indicating that the baseline conclusion is not affected by the sample range, the innovation measure, the form of fixed effects, the level of clustering, or the shock of a special period.

As for the PPML estimate, its point estimate is positive and of the same order of magnitude as the baseline, with a consistent direction, but it is not statistically significant. This loss of precision comes mainly from the dependent variable itself: 65.2% of firm-year observations in the sample record zero green invention patent applications, and once firm and year fixed effects are absorbed the model can only be identified from firms switching between zero and positive values, so the available information is limited. This row takes the raw patent count as the dependent variable, which is not the same measure as the 1%/99% winsorized $\ln(1 + y)$ used in the baseline regression and in the other rows of Table 4; to rule out the influence of this difference in measurement, we re-estimate using the winsorized count, obtaining a point estimate of 0.0343 (standard error 0.0994, $z = 0.35$), again insignificant, which shows that the loss of precision is not caused by the winsorization. In addition, the parallel trends test in Figure 1 is conducted on the logarithmic scale, whereas PPML models the level of patent counts; the two parallel trends assumptions are not equivalent, so the test result of the former cannot be carried over to this row, and this row therefore serves only as a directional reference. The control variable age equals calendar year minus founding year and is collinear with the absorbed firm and year fixed effects, so it is automatically dropped in the PPML specification. In sum, this row is consistent with the baseline in direction and magnitude, but it does not constitute independent confirmation.

Table 4. Robustness checks.

Specification	β	SE	t-Stat	N	Within-R ²
(0) Baseline regression	0.0548 ***	(0.0211)	2.60	45950	0.2092
(1) First two waves only	0.0722 ***	(0.0273)	2.64	37980	0.2057
(2) Granted green invention patents as the outcome	0.0514 ***	(0.0151)	3.40	45950	0.1427
(3) Province $\times$ year fixed effects	0.0690 **	(0.0288)	2.40	45548	0.2244
(4) Excluding the post-pandemic window (2020–2023)	0.0554 ***	(0.0179)	3.10	28782	0.1681
(5) PPML, raw patent count	0.0334	(0.0822)	0.41	45749	0.8553
(6) City-level clustering	0.0538 **	(0.0237)	2.28	45572	0.2094

Note: Row (0) is the baseline regression, rows (1)–(6) are robustness specifications. The dependent variable is green invention patent applications in all rows except row (2), which uses granted green invention patents. All specifications control for firm and year two-way fixed effects; robust standard errors clustered at the firm level are in parentheses. *** and ** indicate significance at the 1% and 5%. Row (5) is estimated by Poisson pseudo-maximum-likelihood (PPML) on the raw patent count; the entry in the t-statistic column is its z-statistic, and the entry in the final column is the pseudo-R², since a within-R² is not defined for this estimator.

4.4. Placebo Test

To further rule out the possibility that the policy effect is driven by unobserved factors or sampling chance, we conduct a randomized placebo test: keeping the size of the treated group and the wave structure of policy timing unchanged, we randomly draw as many firms as the real treated group from the full sample as pseudo-treated firms, randomly shuffle the policy timing, and re-estimate with the same specification as the baseline, repeating 500 times. The results show that the mean of the 500 pseudo-treatment coefficients is -0.0002 and the standard deviation is 0.0215, approximately symmetrically distributed around zero, with a 95th percentile of 0.0353; the

real coefficient 0.0548 exceeds about 99.2% of the pseudo-treatment coefficients, giving a simulated p-value of about 0.008 (Figure 3). The baseline causal effect cannot be an artifact of random sampling. Further, when the policy timing is uniformly advanced by three years and only the pre-policy window is examined, the pseudo-advance treatment coefficient is -0.0126 (standard error 0.0150), insignificant, which rules out the possibility that the treated group already exhibited differentiated green innovation trends before the policy. Treatment timing is dated by the year of the NDRC notice announcing each pilot list, namely 2010, 2012, and 2017 for the three waves. This three-year-advance placebo test also rules out the possibility that firms made anticipatory adjustments upon inclusion in the pilot list. The randomized placebo test and the event study (Figure 1) corroborate each other and jointly support that the baseline results reflect a genuine policy causal effect.

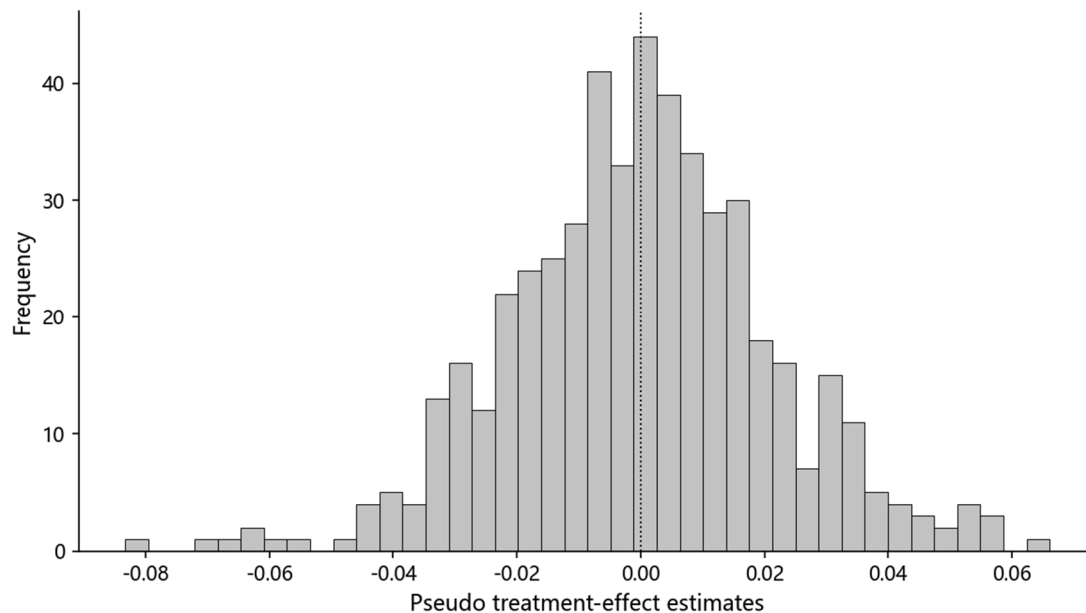


Figure 3. Placebo test.

4.5. Staggered DID Estimation under Heterogeneous Treatment Effects

Recent methodological literature shows that, when treatment effects are heterogeneous across cohorts and periods, the traditional two-way fixed effects (TWFE) estimator may assign negative weights to earlier-treated groups and thereby produce bias [12–14,40,41]. This paper therefore re-estimates the average effect using the Callaway–Sant’Anna [42] group-period estimator, which is robust to heterogeneous treatment effects.

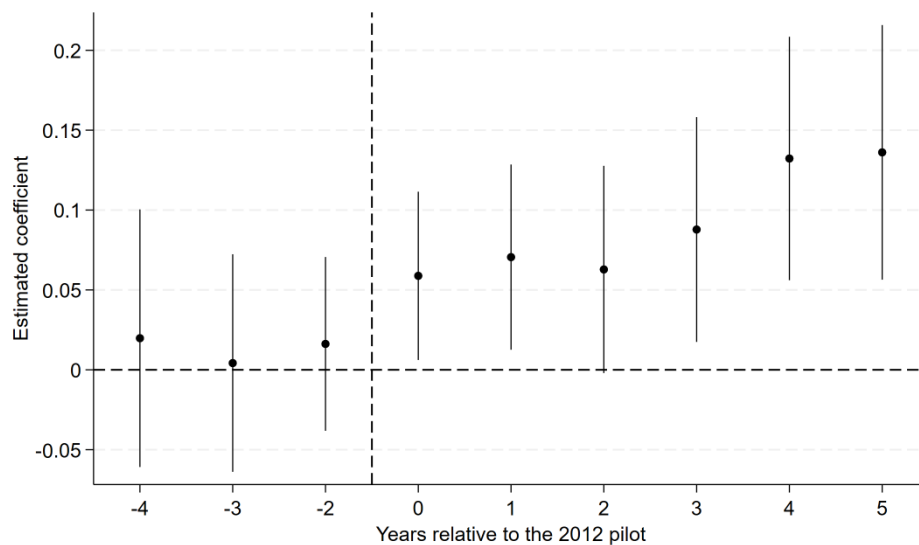
Table 5 reports the results of the Callaway–Sant’Anna [42] group-period estimator. The weighted average ATT with control variables is 0.0684, significant at the 5% level, consistent with the baseline. Table 5 reports the cohort effects of each treated group under unconditional estimation: the 2012 cohort effect is 0.1427, significant at the 1% level, constituting the main source of the policy effect, while the 2010 and 2017 cohort effects are insignificant. In addition, the Sun–Abraham [40] event study shows insignificant coefficients in the periods immediately before the policy and significantly positive coefficients from relative time 3 onward after the policy (0.0548, 0.0520, and 0.0903, significant at the 5%, 5%, and 1% levels, respectively), corroborating the event study in the main text (Figure 1).

Figure 4 reports the dynamic effects of the 2012 cohort, which accounts for the largest share of the average effect and complements the pooled event study in Figure 1. Two points should be noted. First, the post-treatment effects of the 2012 cohort increase year by year and only become significant in the third relative year, indicating that the policy effect involves a time lag. Second, the coefficient of the 2017 cohort is small because this cohort has the shortest post-treatment window, more than half of which falls in the pandemic period, and its dynamic effects likewise only appear in the third year; the small coefficient reflects the observation window rather than the effectiveness of the policy.

Table 5. Callaway–Sant’Anna group-period estimates.

Cohort	Coefficient
G 2010	0.0719 (0.0486)
G 2012	0.1427 *** (0.0335)
G 2017	0.0207 (0.0380)
G Average	0.0858 *** (0.0248)

Note: The dependent variable is green invention patent applications; the method is unconditional DRIPW estimation [42]; groups are defined by the pilot policy start year (gvar), with the never-treated group as control. GAverage is the simple average of the cohort effects. Robust standard errors are in parentheses. *** indicate significance at the 1% level.

**Figure 4.** Dynamic effects for the 2012 cohort.

4.6. Patent-Type Decomposition

To clarify the structure of the policy effect across patent types, Table 6 reports the baseline specification separately for four dependent variables: green invention patent applications, granted green invention patents, green utility-model patent applications, and total green patent applications. The purpose is to establish whether the increase is concentrated in substantive, invention-type green patents or instead reflects a general expansion of green patenting activity.

In Table 6 the effect is concentrated in invention-type green patents. The did coefficient is 0.0548 for green invention patent applications and 0.0514 for granted green invention patents, both significant at the 1% level. For green utility-model applications it is 0.0184 and for total green patent applications 0.0374; neither is statistically significant. The pilot therefore raises green invention patent applications and green invention patent grants without producing a detectable increase in the total count of green patents. As Table 6 shows, the policy effect is concentrated in substantive invention-type green patents, while no significant increase is detected in total green patents. This structure indicates that the observed increase is not a general expansion of green patenting activity, but more likely a concentration of innovation resources in compliance-relevant green inventions.

Table 6. Policy effects across green patent types.

Specification	β	SE	t-Stat	N	Within-R ²
(1) Green invention patent applications	0.0548 ***	(0.0211)	2.60	45950	0.2092
(2) Granted green invention patents	0.0514 ***	(0.0151)	3.40	45950	0.1427
(3) Green utility-model patent applications	0.0184	(0.0208)	0.88	45950	0.1906
(4) Total green patent applications	0.0374	(0.0244)	1.53	45950	0.2638

Note: *** indicate significance at the 1% level.

5. Heterogeneity Analysis

The baseline regression estimates the average treatment effect, and H2 further asks whether, beneath this average effect, a resource threshold determines the distribution of policy benefits. Testing the heterogeneity of the policy effect bears not only on the external validity of the core conclusions but also directly tests the theoretical mechanism of the innovation-offset threshold. If a staggered DID uses common year fixed effects while different firm types have different time trends, cross-group differences may be misattributed to differences in treatment effects; this section therefore adopts a group-specific year effects specification (year $\times$ firm-type dummy interactions), allowing different firm types to follow their own time trends. Table 7 separately tests the three dimensions through the interactions of did with ownership, size, and pollution intensity; the baseline group effect in each dimension corresponds to the policy effect for the group of firms not flagged in that dimension.

5.1. Firm Ownership

Table 7 Column (1) interacts a state-owned enterprise dummy with did under the group-specific year effects specification. The did coefficient (the baseline group effect for non-SOEs) is 0.0408, significant at the 10% level; did $\times$ soe is 0.0133, insignificant; the total within-group effect for SOEs is 0.0541 ($t = 1.83$), significant at the 10% level. State-owned firms respond consistently with policy objectives and face small financing constraints, enabling them to bear green innovation investment and exhibiting a positive green innovation response; however, did $\times$ soe is insignificant, and the data provide no evidence that the two groups differ.

5.2. Firm Size

Table 7 Column (2) interacts a large-firm dummy with did. Since the model controls for the continuous variable size, the baseline group of the interaction corresponds to firms with size below the median. The did coefficient is 0.0195, insignificant; did $\times$ large is 0.0463, insignificant; the total within-group effect for large firms is 0.0658 ($t = 2.11$), significantly positive at the 5% level. No significant between-group difference appears along the size dimension, and the point estimates do not suggest that the policy systematically favors large firms.

5.3. Pollution Intensity

The pollution intensity dimension in this section is delineated by a heavy-polluting industry dummy (heavy). Following the CSRC industry classification, firms in mining (category B), manufacturing (category C), and the production and supply of electricity, heat, gas, and water (category D) take the value 1, and all others 0. This is a broad industry-level proxy for pollution intensity rather than an exact identification of heavy polluters, since category C covers all manufacturing; the heterogeneity results along this dimension should therefore be read as descriptive evidence on the contrast between secondary-sector firms and other firms, not as a precise estimate for heavy-polluting industries.

Table 7 Column (3) interacts a heavy-polluting industry dummy with did. The did coefficient (the baseline group effect for non-heavy-polluting firms) is 0.0885, significant at the 5% level; did $\times$ heavy is -0.0272 , insignificant; the total within-group effect for heavily polluting firms is 0.0613 ($t = 2.41$), significantly positive at the 5% level. The interaction did $\times$ heavy is insignificant, that is, no statistically significant difference is detected between the two types of firms.

Table 7. Heterogeneity analysis.

	(1) Ownership: SOE	(2) Size: Large	(3) Pollution: Heavy-Polluting
did (baseline group effect)	0.0408 * (0.0241)	0.0195 (0.0223)	0.0885 ** (0.0357)
did $\times$ soe	0.0133 (0.0337)		
did $\times$ large		0.0463 (0.0383)	
did $\times$ heavy			-0.0272 (0.0432)
Total within-group effect (did + did $\times$ group)	0.0541 * (0.0295)	0.0658 ** (0.0311)	0.0613 ** (0.0254)
N	45950	45950	45950
R ²	0.2176	0.2292	0.2126

Note: The group-specific year effects specification (year $\times$ firm-type dummy interactions) is adopted, allowing different firm types to follow their own time trends. The total within-group effects (did + did $\times$ group) are obtained from a joint lincom test, with significance corresponding to their p-values (SOE 0.067, large 0.035, heavy-polluting 0.016). Robust standard errors clustered at the firm level are in parentheses. * and ** indicate significance at the 10% and 5% levels.

Taking the three dimensions of heterogeneity analysis together. First, the within-group effects are positive. Under the specification allowing different firm types to follow their own time trends, the total within-group effects for state-owned firms, large firms, and heavily polluting firms are all positive, significant at the 10%, 5%, and 5% levels respectively, whereas the baseline group estimate for smaller firms (0.0195) is positive but insignificant. Second, none of the between-group differences is statistically significant. In the group-specific specification for each of the three dimensions, the interactions of did with the ownership, size, and pollution intensity dummies are all insignificant, indicating that no difference in the policy effect across firm types is detected.

6. Mechanism Analysis

6.1. Financing Constraint Threshold and Measurement Caveats

Financing constraints are measured by the absolute value of the SA index [36]:

$$SA = -0.737 \times \text{Size} + 0.043 \times \text{Size}^2 - 0.040 \times \text{Age}$$

where Size is the natural logarithm of total assets (in RMB 10,000) and Age is the number of years since incorporation; a larger |SA| indicates stronger financing constraints. To address the concern that firm size may change with the policy and thereby contaminate the moderating variable, we construct this index from firm size and founding age at the firm's first year in the sample (the baseline period), so that it is time-invariant and determined entirely before the policy; Section 6.2 repeats the tests using the year-by-year panel SA index as a robustness check. It enters the regression in standardized form zsa, so the main effect did represents the policy effect for a firm at the average constraint level (zsa = 0). Table 8 reports the mechanism test results.

Table 8. Mechanism tests.

	(1) Threshold	(2) Threshold + Size	(3) Low-Constraint Group	(4) High-Constraint Group	(5) R&D Investment
did	0.0208 (0.0201)	−0.0073 (0.0275)	0.0844 *** (0.0296)	0.0124 (0.0278)	−0.5789 *** (0.1884)
did $\times$ zsa	−0.2223 *** (0.0196)	−0.2077 *** (0.0236)			−0.9784 *** (0.1473)
did $\times$ large		0.0567 (0.0415)			
N	45,950	45,950	22,848	23,102	45,950
R ²	0.2217	0.2218	0.2546	0.1738	0.3423

Note: Financing constraints are measured by the absolute value of the SA index (|SA|), with zsa its standardized form; the SA index is constructed from firm size and founding age at the firm's first year in the sample (baseline period) [36] and is time-invariant. A significantly negative did $\times$ zsa indicates that the policy effect declines as constraints rise. In columns (3) and (4) the sample is split into low- and high-constraint groups by the median of baseline |SA|. Dependent variable: ln(green invention patent applications) lngprpat_app_inv in columns (1)–(4), and ln(1 + R&D expenditure) in column (5). Robust standard errors clustered at the firm level are in parentheses. *** indicates significance at the 1% level.

Columns (1) and (2) of Table 8 show that the did $\times$ zsa coefficients are −0.2223 and −0.2077, both significant at the 1% level; after controlling for did $\times$ large in column (2) the coefficient changes only slightly, indicating that the constraint threshold is not a disguise for the size channel and that financing constraints independently moderate the policy effect. The subsample results in columns (3) and (4) are consistent with this: the policy effect in the low-constraint group is 0.0844, significant at the 1% level, while in the high-constraint group it is 0.0124, insignificant. Resource-constrained firms fail to obtain significant policy benefits, providing direct micro-level evidence for H2. Column (5) takes R&D investment as the dependent variable to examine the substitution effect, but this column is highly sensitive to the treatment of missing values. The conclusion regarding the financing constraint threshold (columns (1)–(4)) does not depend on the R&D expenditure variable.

As for the R&D investment variable, 19.25% of firm-year observations in the sample do not disclose R&D expenditure, and the probability of non-disclosure is significantly higher for pilot firms. Column (5) of Table 8 follows the same zero-filling treatment as the other columns, treating non-disclosure as zero R&D investment,

under which the did coefficient is -0.5789 and $\text{did} \times \text{zsa}$ is -0.9784 , both significant at the 1% level. To determine whether this negative result is driven by the zero-filling itself, we run two comparisons: when only the disclosing sample is retained, the did coefficient is -0.0279 ($t = -0.89$) and is no longer significant; when firms are required to disclose in every year of the sample period, the coefficient is 0.0278 ($t = 0.78$), likewise insignificant. It is therefore evident that the negative estimate in column (5) of Table 8 mainly comes from the zero-filling of missing values rather than from an actual contraction of R&D investment, and on this basis we do not treat the decline in R&D expenditure as mechanism evidence, retaining this column only to report the sensitivity to measurement.

The core conclusion regarding the financing constraint threshold does not depend on the R&D expenditure variable: $\text{did} \times \text{zsa}$ is significantly negative, the policy effect in the low-constraint group is 0.0844 and significant at the 1% level, and in the high-constraint group it is 0.0124 and insignificant, which still supports H2.

6.2. Measurement Robustness

As a measurement robustness check, we repeat the tests using the year-by-year panel SA index (varying annually with firm size and age) as an alternative measure; the results are reported in Table 9, the threshold effect is consistent with columns (1)–(4) of Table 8, and column (5) is likewise sensitive to the treatment of missing values, so the conclusion does not depend on the construction of the SA index.

Table 9. Robustness of mechanism tests.

	(1) Threshold	(2) Threshold + Size	(3) Low-Constraint Group	(4) High-Constraint Group	(5) R&D Investment
did	0.0288 (0.0187)	0.0210 (0.0274)	0.0816 ** (0.0318)	0.0225 (0.0220)	-0.5557 *** (0.1857)
$\text{did} \times \text{zsa}$	-0.1160 *** (0.0203)	-0.1130 *** (0.0239)			-0.6917 *** (0.1484)
$\text{did} \times \text{large}$		0.0140 (0.0395)			
N	45950	45,950	22,902	23,048	45,950
R ²	0.2389	0.2389	0.2679	0.0970	0.3451

Note: The SA index is constructed from year-by-year panel size and founding age [36] and varies over years. A significantly negative $\text{did} \times \text{zsa}$ indicates that the policy effect declines as constraints rise. In columns (3) and (4) the sample is split into low- and high-constraint groups by the annual median of |SA|. Dependent variable: $\ln(\text{green invention patent applications})$ $\ln \text{grpat_app_inv}$ in columns (1)–(4), and $\ln(1 + \text{R\&D expenditure})$ in column (5). Robust standard errors clustered at the firm level are in parentheses. ** and *** indicate significance at the 5% and 1% levels.

7. Further Analysis

7.1. PSM-DID to Mitigate Sample Selection Bias

The selection of pilot cities is not random, and firms in pilot cities may systematically differ from firms in non-pilot cities in observable characteristics. To mitigate this sample selection bias, we perform propensity score matching (PSM) based on 2008 (pre-policy) cross-sectional characteristics, using firm size, leverage, profitability, age, governance structure, and environmental regulation intensity as covariates, and re-estimate the staggered DID on the matched comparable subsample. Because 2008 precedes the establishment of the first pilot wave, only pre-policy information enters the match, and no covariate realized after the first treatment wave is introduced. Propensity scores are estimated by a logit model, treated firms are matched to control firms by 1:1 nearest-neighbor matching with a caliper of 0.05, and only observations within the common support region are retained, yielding 21,025 firm-year observations after matching.

Table 10 shows that, after matching, the did coefficient is 0.0749 with a standard error of 0.0281 , significant at the 1% level and larger than in the baseline regression. This difference stems from the change that matching makes to the composition of the control group: the caliper and common support requirements exclude treated firms that have no close control counterpart and remove control firms whose pre-policy characteristics lie far from those of the treated firms, so the estimate is obtained from firms that are more similar at baseline. The larger coefficient therefore indicates that the unmatched baseline estimate, even if biased, is biased in a conservative direction. The balance test in Figure 5 shows that, after matching, the standardized differences between the treated and control groups in the main characteristics converge substantially, indicating that matching effectively constructs a comparable sample. On this sample, the forcing effect of the pilot policy on corporate green innovation remains significantly positive, further supporting H1.

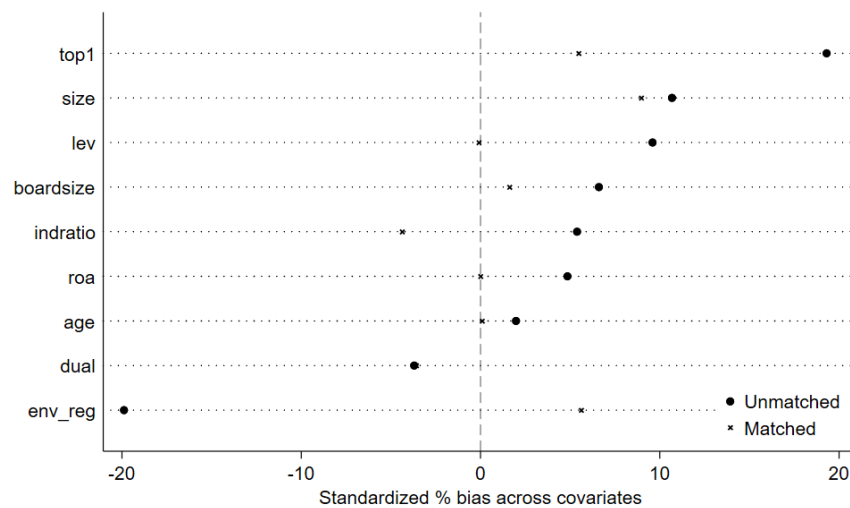


Figure 5. PSM balance test.

Table 10. PSM-DID: matched subsample.

	(1)
did	0.0749 *** (0.0281)
N	21,025
R ²	0.2477

Note: Subsample after PSM matching based on 2008 cross-sectional characteristics (size/leverage/profitability/age/governance/environmental regulation); control variables and fixed effects are the same as in Table 2. Robust standard errors clustered at the firm level are in parentheses. *** indicates significance at the 1% level.

7.2. Goodman–Bacon Decomposition

This section uses the Goodman-Bacon [13] decomposition as a complement to directly diagnose the weight structure of the TWFE estimator; the results are consistent with the baseline and with Section 4.5.

The Goodman-Bacon [13] decomposition expresses the TWFE estimator as a weighted average of all 2×2 group-period comparisons and is used to diagnose the potential negative-weight problem of “using already-treated units as controls” in staggered DID. Owing to the strongly balanced panel requirement of the *bacondecomp* procedure, we construct a balanced subsample of firms continuously present in the sample over 2008–2023 (21,216 observations) and re-run the decomposition. Table 11 shows that the TWFE coefficient on the balanced subsample is 0.0893 (standard error 0.0162, significant at the 1% level); the weights of the individual β terms sum to 1, and the weighted sum exactly reproduces the TWFE coefficient (0.0893), indicating that the decomposition is internally consistent. Figure 6 plots each 2×2 estimate against its weight, with the horizontal dashed line marking the overall DD estimate (0.0893). The timing-group comparisons receive small weights and scatter on both sides of the overall estimate, and no single comparison dominates the estimate, indicating that the baseline result is not an artefact of negative weighting. It should be noted that the balanced subsample contains only firms continuously listed during the sample period and may be skewed toward large and stable firms; its decomposition results are mainly used to diagnose the weight structure of TWFE and do not represent the full sample or replace the conclusions from the main sample.

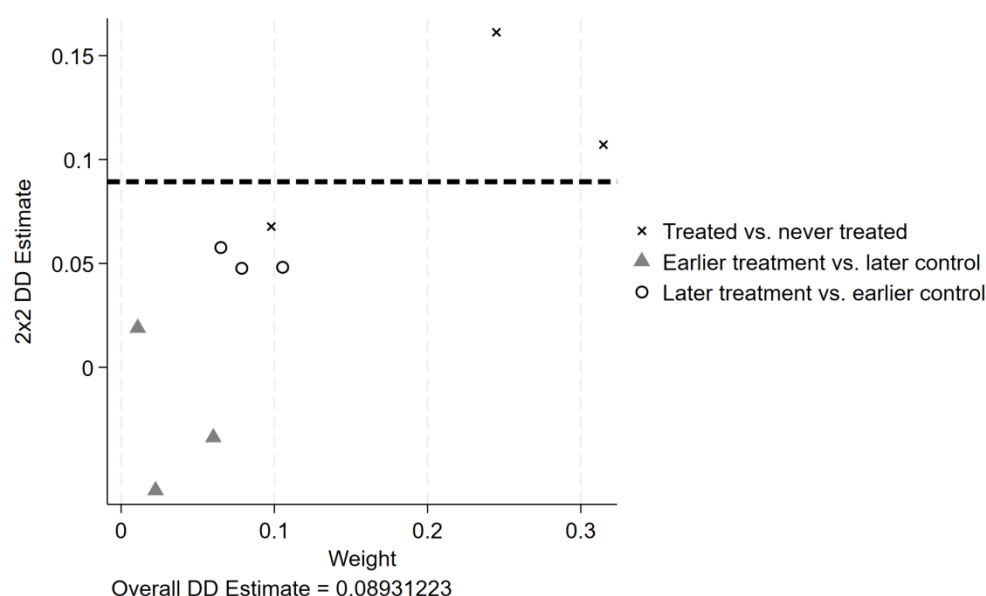


Figure 6. Goodman–Bacon decomposition.

Table 11. Goodman–Bacon decomposition.

Comparison Type	β	Weight
Treated vs. control (never-treated)	0.1215	0.6575
Treated vs. treated: Wave 1 vs. Wave 2 (earlier treated)	0.0189	0.0108
Treated vs. treated: Wave 2 vs. Wave 1 (later treated)	0.0577	0.0650
Treated vs. treated: Wave 1 vs. Wave 3 (earlier treated)	−0.0593	0.0225
Treated vs. treated: Wave 3 vs. Wave 1 (later treated)	0.0477	0.0787
Treated vs. treated: Wave 2 vs. Wave 3 (earlier treated)	−0.0338	0.0602
Treated vs. treated: Wave 3 vs. Wave 2 (later treated)	0.0482	0.1053
Total	0.0893	1.0000

Note: Waves 1, 2, and 3 are the 2010, 2012, and 2017 pilot cohorts, respectively; the control group comprises firms never included in the pilots. “Earlier treated” refers to the 2×2 comparison in which the earlier-treated group is treated and the later-treated group serves as control during periods before its treatment; “later treated” refers to the later-treated group as treated, with the earlier-treated (already-treated) group as control. The balanced subsample consists of firms continuously present in the sample over 2008–2023 (21,216 observations). The weights of the individual comparisons sum to 1, and the weighted average of β equals the TWFE coefficient 0.0893.

8. Conclusions and Policy Implications

8.1. Main Conclusions

Using panel data of Chinese A-share listed companies over 2008–2023 and a staggered difference-in-differences method, this paper addresses the question of whether the low-carbon city pilot “forces” or “crowds out” corporate green innovation, forming judgments at two levels.

First, at the level of the average effect, the low-carbon city pilot significantly “forces” corporate green innovation, and the weak Porter hypothesis holds in the Chinese context. The policy raises the green invention patent applications of firms in pilot cities by about 5.6% on average. The effect increases year by year.

Second, at the level of theoretical implications, the Porter hypothesis debate converges to a question of conditionality. “Forcing” and “crowding out” are not mutually exclusive opposing hypotheses. The policy produces a robust “forcing” effect on average, but the distribution of its benefits is moderated by firm resource endowments: firms with lower financing constraints can cross the innovation-offset threshold and obtain significant gains, whereas resource-constrained firms can hardly obtain policy benefits of comparable magnitude. Whether the innovation effect of environmental regulation holds, and which firms it benefits, depends on whether the regulatory design is credible and whether firms can cross the resource threshold.

8.2. Policy Implications

First, adhere to a green innovation orientation. The pilot policy effectively stimulates corporate green innovation through clear and sustained signals, and policymakers should maintain the credibility and continuity of emission reduction and green development commitments. Second, establish differentiated support for resource-constrained firms. Provide targeted support such as green credit, green patent subsidies, and R&D tax incentives to resource-constrained firms, reducing their cost of crossing the innovation threshold and avoiding a widening divergence in green innovation capability across firms. Third, guard against the bubble risk of strategic patenting. The low marginal cost of green patent applications may turn them into strategic behavior by firms catering to the policy.

8.3. Limitations

First, the selection of pilot cities is not random; although this paper mitigates the sample selection problem through PSM and parallel trends tests, the role of unobserved factors cannot be fully excluded. Second, green patent applications emphasize the quantity dimension; although grants are used as a robustness check, deeper measures such as patent citation quality and green total factor productivity await support from future data. Third, the mechanism test of the financing constraint threshold relies more on model specification, and this paper treats it as suggestive evidence, which could be strengthened in the future through instrumental strategies such as exogenous credit shocks. Fourth, there is systematic missingness in the disclosure of corporate R&D expenditure, and the disclosure probability of pilot firms is significantly lower; this paper does not claim an R&D crowding-out effect on this basis. Fifth, green patent data can distinguish green inventions from green utility models but cannot distinguish green from non-green patents; because the non-green patents of firms cannot be observed within the same framework, this paper cannot examine how the policy affects the green share of firms' total innovation. Sixth, a log-count outcome dominated by zeros cannot separate an increase in the share of firms that begin patenting from an increase in application intensity among firms that already patent, so the results should not be read as evidence on the extensive margin of green innovation as distinct from the intensive margin.

Author Contributions

S.W.: conceptualization, methodology, software, validation, formal analysis and resources, data curation, visualization, writing—original draft preparation, writing—review & editing, project administration and funding acquisition. The author has read and agreed to the published version of the manuscript.

Funding

This research was funded by the National Innovation and Entrepreneurship Training Program for College Students, through the project *Research on the Prediction and Governance of Pollution Violations by National Training Program of Innovation for Undergraduates* (Grant No. 202610359057).

Institutional Review Board Statement

Not applicable.

Informed Consent Statement

Not applicable.

Data Availability Statement

This study draws on multiple publicly available datasets covering Chinese A-share listed companies, green patent applications, corporate financial and governance characteristics, and the low-carbon city pilot program. The firm-level observations are limited to Chinese A-share listed companies over the period 2008–2023; unlisted firms and firms with missing key variables are not directly included. Green innovation is measured by patent-based indicators, which capture innovation counts but not the full quality of innovation or firms' actual environmental performance. The resulting dataset therefore supports comparative and heterogeneity-based analysis but should not be interpreted as a complete representation of the green innovation behavior of all Chinese firms. The processed data and analysis code may be provided by the corresponding author upon reasonable request.

Conflicts of Interest

The author declares no conflict of interest.

Use of AI and AI-Assisted Technologies

Artificial intelligence tools were used only for language translation and linguistic polishing of the manuscript. All research design, data analysis, interpretation of results and manuscript writing were completed by the authors.

References

- Porter, M.E. America's Green Strategy. *Bus. Environ.* **1996**, *33*, 1072.
- Porter, M.E.; van der Linde, C. Toward a New Conception of the Environment-Competitiveness Relationship. *J. Econ. Perspect.* **1995**, *9*, 97–118. <https://doi.org/10.1257/jep.9.4.97>.
- Palmer, K.; Oates, W.E.; Portney, P.R. Tightening Environmental Standards: The Benefit-Cost or the No-Cost Paradigm? *J. Econ. Perspect.* **1995**, *9*, 119–132. <https://doi.org/10.1257/jep.9.4.119>.
- Ambec, S.; Cohen, M.A.; Elgie, S.; et al. The Porter Hypothesis at 20: Can Environmental Regulation Enhance Innovation and Competitiveness? *Rev. Environ. Econ. Policy* **2013**, *7*, 2–22. <https://doi.org/10.1093/reep/res016>.
- Cheng, J.; Yi, J.; Dai, S.; et al. Can Low-Carbon City Construction Facilitate Green Growth? Evidence from China's Pilot Low-Carbon City Initiative. *J. Clean. Prod.* **2019**, *231*, 1158–1170. <https://doi.org/10.1016/j.jclepro.2019.05.327>.
- Yu, Y.; Zhang, N. Low-Carbon City Pilot and Carbon Emission Efficiency: Quasi-Experimental Evidence from China. *Energy Econ.* **2021**, *96*, 105125. <https://doi.org/10.1016/j.eneco.2021.105125>.
- Chen, C.; Lin, Y.; Lv, N.; et al. Can Government Low-Carbon Regulation Stimulate Urban Green Innovation? Quasi-Experimental Evidence from China's Low-Carbon City Pilot Policy. *Appl. Econ.* **2022**, *54*, 6559–6579. <https://doi.org/10.1080/00036846.2022.2072466>.
- Zhang, H.; Di Maria, C.; Ghezelayagh, B.; et al. Climate Policy in Emerging Economies: Evidence from China's Low-Carbon City Pilot. *J. Environ. Econ. Manag.* **2024**, *124*, 102943. <https://doi.org/10.1016/j.jeem.2024.102943>.
- Zou, C.; Huang, Y.; Wu, S.; et al. Does “Low-Carbon City” Accelerate Urban Innovation? Evidence from China. *Sustain. Cities Soc.* **2022**, *83*, 103954. <https://doi.org/10.1016/j.scs.2022.103954>.
- Luo, Y.; Liu, Y.; Wang, D.; et al. Low-Carbon City Pilot Policy and Enterprise Low-Carbon Innovation—A Quasi-Natural Experiment from China. *Econ. Anal. Policy* **2024**, *83*, 204–222. <https://doi.org/10.1016/j.eap.2024.06.014>.
- Qu, F.; Xu, L.; He, C. Leverage Effect or Crowding out Effect? Evidence from Low-Carbon City Pilot and Energy Technology Innovation in China. *Sustain. Cities Soc.* **2023**, *91*, 104423. <https://doi.org/10.1016/j.scs.2023.104423>.
- de Chaisemartin, C.; D'Haultfœuille, X. Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *Am. Econ. Rev.* **2020**, *110*, 2964–2996. <https://doi.org/10.1257/aer.20181169>.
- Goodman-Bacon, A. Difference-in-Differences with Variation in Treatment Timing. *J. Econom.* **2021**, *225*, 254–277. <https://doi.org/10.1016/j.jeconom.2021.03.014>.
- Baker, A.C.; Larcker, D.F.; Wang, C.C.Y. How Much Should We Trust Staggered Difference-in-Differences Estimates? *J. Financ. Econ.* **2022**, *144*, 370–395. <https://doi.org/10.1016/j.jfineco.2022.01.004>.
- Roth, J.; Sant'Anna, P.H.C.; Bilinski, A.; et al. What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature. *J. Econom.* **2023**, *235*, 2218–2244. <https://doi.org/10.1016/j.jeconom.2023.03.008>.
- Popp, D. Induced Innovation and Energy Prices. *Am. Econ. Rev.* **2002**, *92*, 160–180. <https://doi.org/10.1257/000282802760015658>.
- Aghion, P.; Dechezleprêtre, A.; Hémous, D.; et al. Carbon Taxes, Path Dependency, and Directed Technical Change: Evidence from the Auto Industry. *J. Political Econ.* **2016**, *124*, 1–51. <https://doi.org/10.1086/684581>.
- Calel, R.; Dechezleprêtre, A. Environmental Policy and Directed Technological Change: Evidence from the European Carbon Market. *Rev. Econ. Stat.* **2016**, *98*, 173–191. https://doi.org/10.1162/rest_a_00470.
- Albrizio, S.; Kozluk, T.; Zipperer, V. Environmental Policies and Productivity Growth: Evidence across Industries and Firms. *J. Environ. Econ. Manag.* **2017**, *81*, 209–226. <https://doi.org/10.1016/j.jeem.2016.06.002>.
- Dechezleprêtre, A.; Sato, M. The Impacts of Environmental Regulations on Competitiveness. *Rev. Environ. Econ. Policy* **2017**, *11*, 183–206. <https://doi.org/10.1093/reep/rex013>.
- Chen, Q.; Chen, Z.; Liu, Z.; et al. Regulating Conglomerates: Evidence from an Energy Conservation Program in China. *Am. Econ. Rev.* **2025**, *115*, 408–447. <https://doi.org/10.1257/aer.20211455>.
- Xie, T.; Yuan, Y. Go with the Wind: Spatial Impacts of Environmental Regulations on Economic Activities in China. *J. Dev. Econ.* **2023**, *164*, 103139. <https://doi.org/10.1016/j.jdevco.2023.103139>.
- Cheng, Y.; Du, K.; Yao, X. Stringent Environmental Regulation and Inconsistent Green Innovation Behavior: Evidence from Air Pollution Prevention and Control Action Plan in China. *Energy Econ.* **2023**, *120*, 106571. <https://doi.org/10.1016/j.eneco.2023.106571>.
- Jaffe, A.B.; Palmer, K. Environmental Regulation and Innovation: A Panel Data Study. *Rev. Econ. Stat.* **1997**, *79*, 610–619. <https://doi.org/10.1162/003465397557196>.

25. Jaffe, A.B.; Newell, R.G.; Stavins, R.N. Environmental Policy and Technological Change. *Environ. Resour. Econ.* **2002**, *22*, 41–70. <https://doi.org/10.1023/a:1015519401088>.
26. Jaffe, A.B.; Newell, R.G.; Stavins, R.N. A Tale of Two Market Failures: Technology and Environmental Policy. *Ecol. Econ.* **2005**, *54*, 164–174. <https://doi.org/10.1016/j.ecolecon.2004.12.027>.
27. Brunnermeier, S.B.; Cohen, M.A. Determinants of Environmental Innovation in US Manufacturing Industries. *J. Environ. Econ. Manag.* **2003**, *45*, 278–293. [https://doi.org/10.1016/s0095-0696\(02\)00058-x](https://doi.org/10.1016/s0095-0696(02)00058-x).
28. Hamamoto, M. Environmental Regulation and the Productivity of Japanese Manufacturing Industries. *Resour. Energy Econ.* **2006**, *28*, 299–312. <https://doi.org/10.1016/j.reseneeco.2005.11.001>.
29. Johnstone, N.; Haščič, I.; Popp, D. Renewable Energy Policies and Technological Innovation: Evidence Based on Patent Counts. *Environ. Resour. Econ.* **2010**, *45*, 133–155. <https://doi.org/10.1007/s10640-009-9309-1>.
30. Horbach, J. Determinants of Environmental Innovation—New Evidence from German Panel Data Sources. *Res. Policy* **2008**, *37*, 163–173. <https://doi.org/10.1016/j.respol.2007.08.006>.
31. Lanoie, P.; Laurent-Lucchetti, J.; Johnstone, N.; et al. Environmental Policy, Innovation and Performance: New Insights on the Porter Hypothesis. *J. Econ. Manag. Strategy* **2011**, *20*, 803–842. <https://doi.org/10.1111/j.1530-9134.2011.00301.x>.
32. Lee, C.C.; Feng, Y.; Peng, D. A Green Path towards Sustainable Development: The Impact of Low-Carbon City Pilot on Energy Transition. *Energy Econ.* **2022**, *115*, 106343. <https://doi.org/10.1016/j.eneco.2022.106343>.
33. Pan, A.; Zhang, W.; Shi, X.; et al. Climate Policy and Low-Carbon Innovation: Evidence from Low-Carbon City Pilots in China. *Energy Econ.* **2022**, *112*, 106129. <https://doi.org/10.1016/j.eneco.2022.106129>.
34. Song, Y.; Bian, Z.; Ma, N.; et al. How Does the Low-Carbon City Pilot Policy Affect Enterprises' Green Innovation? Empirical Evidence from the Context of China's Digital Economy Development. *Sustainability* **2024**, *16*, 1760. <https://doi.org/10.3390/su16051760>.
35. Li, Y.; Yaacob, M.H.; Xie, T. Effects of China's Low Carbon Pilot City Policy on Corporate Green Innovation: Considering the Mediating Role of Public Environmental Concern. *Financ. Res. Lett.* **2024**, *65*, 105641. <https://doi.org/10.1016/j.frl.2024.105641>.
36. Hadlock, C.J.; Pierce, J.R. New Evidence on Measuring Financial Constraints: Moving Beyond the KZ Index. *Rev. Financ. Stud.* **2010**, *23*, 1909–1940. <https://doi.org/10.1093/rfs/hhq009>.
37. Ivanov, I.T.; Kruttli, M.S.; Watugala, S.W. Banking on Carbon: Corporate Lending and Cap-and-Trade Policy. *Rev. Financ. Stud.* **2024**, *37*, 1640–1684. <https://doi.org/10.1093/rfs/hhad085>.
38. De Haas, R.; Martin, R.; Muûls, M.; et al. Managerial and Financial Barriers to the Green Transition. *Manag. Sci.* **2025**, *71*, 2890–2921. <https://doi.org/10.1287/mnsc.2023.00772>.
39. Amore, M.D.; Bennesen, M. Corporate Governance and Green Innovation. *J. Environ. Econ. Manag.* **2016**, *75*, 54–72. <https://doi.org/10.1016/j.jeem.2015.11.003>.
40. Sun, L.; Abraham, S. Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects. *J. Econom.* **2021**, *225*, 175–199. <https://doi.org/10.1016/j.jeconom.2020.09.006>.
41. Borusyak, K.; Jaravel, X.; Spiess, J. Revisiting Event-Study Designs: Robust and Efficient Estimation. *Rev. Econ. Stud.* **2024**, *91*, 3253–3285. <https://doi.org/10.1093/restud/rdae007>.
42. Callaway, B.; Sant'Anna, P.H.C. Difference-in-Differences with Multiple Time Periods. *J. Econom.* **2021**, *225*, 200–230. <https://doi.org/10.1016/j.jeconom.2020.12.001>.