

Do Low-Carbon Transportation Policies Spur Green Innovation? Causal Evidence from Pilot Cities for New Energy Vehicles in China

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Abstract

Whether demand-side transport decarbonization policies can induce green technological innovation remains an open question. We exploit China's new energy vehicle (NEV) demonstration program, which designated 88 prefecture-level cities as pilot cities in two rounds during 2013–2014, as a quasi-natural experiment. Using a balanced panel of 281 Chinese cities from 2008 to 2019 and green patent applications classified by the WIPO IPC Green Inventory, we estimate staggered difference-in-differences models complemented by heterogeneity-robust estimators. The pilot policy increased green patent applications in treated cities by approximately 9–10 percent relative to non-pilot cities, with effects emerging one year after designation and strengthening over time. Results are robust to Callaway–Sant'Anna and Sun–Abraham estimators, propensity-score-matched samples, placebo tests, and controls for concurrent low-carbon policies. Channel analysis indicates that the policy operated through demand expansion, fiscal support for science and technology, automotive industry agglomeration, and charging-infrastructure rollout, with the response concentrated among firms rather than universities. Effects are larger in eastern cities, cities with an automotive industrial base, and more marketized cities.

Keywords: low-carbon transport policy; new energy vehicles; green innovation; pilot cities; difference-in-differences; China

JEL codes: O31, O38, Q55, Q58, R58

1. Introduction

Mitigating climate change requires deep decarbonization of the transportation sector, which accounts for roughly one-fifth of global energy-related carbon dioxide emissions and remains among the fastest-growing emission sources [1, 2]. Because vehicle fleets turn over slowly and internal combustion technologies benefit from a century of accumulated know-how, most governments rely on policy intervention—purchase subsidies, fleet mandates, fuel-economy standards, and demonstration programs—to accelerate the transition toward electric mobility. A central but under-explored question is whether such demand-side interventions merely shift vehicle purchases, or whether they also reshape the direction of technological change by stimulating green innovation in the cities and industries they touch.

China offers an ideal setting to study this question. The country has been the world's largest automobile market since 2009 and, after two decades of aggressive industrial policy, became the largest producer and consumer of new energy vehicles (NEVs), a category encompassing battery electric, plug-in hybrid, and fuel-cell vehicles [3–5]. A hallmark of China's approach is policy experimentation through designated pilot cities: the "Ten Cities, Thousand Vehicles" demonstration project launched in 2009 deployed NEVs in public fleets of initially 13 and eventually 25 cities, and the 2013–2014 NEV demonstration and promotion program extended the experiment to 88 cities organized in 39 city clusters, each subject to explicit fleet-purchase quotas, charging-infrastructure mandates, and fiscal subsidies [6, 7]. These staggered designations create plausibly exogenous variation in the intensity of

low-carbon transport policy across otherwise comparable cities.

Despite the scale of these programs, we know surprisingly little about their innovation consequences. The existing evaluation literature concentrates on vehicle adoption, consumer acceptance, subsidy cost-effectiveness, and emission outcomes [8–14]. Whether pilot policies induce local green innovation is theoretically ambiguous. On one hand, the Porter hypothesis and the induced-innovation literature suggest that well-designed environmental regulation raises the expected return to clean research and redirects inventive effort [15–19]. On the other hand, compliance-cost arguments warn that regulation diverts resources from productive R&D, and recent evidence on China's NEV pilot policy even documents a negative association with urban green innovation [20]. The empirical record thus remains contested, and credible causal evidence is scarce.

Two econometric obstacles have hampered progress. First, pilot cities are not chosen at random: they tend to be wealthier, more industrialized, and endowed with automotive production capacity, so simple comparisons confound policy effects with selection. Second, the staggered rollout of the demonstration program renders conventional two-way fixed effects (TWFE) estimators potentially biased when treatment effects vary over time or across cohorts, because already-treated units serve as implicit controls [21, 22]. The recent difference-in-differences (DID) literature provides a toolkit—group-time average treatment effects, interaction-weighted event studies, and stacked designs—that addresses these concerns directly [23–26].

This paper exploits the 2013–2014 NEV demonstration program as a quasi-natural experiment to provide causal evidence on whether low-carbon transportation policies spur green innovation. We assemble a balanced panel of 281 prefecture-level Chinese cities over 2008–2019 and measure green innovation using patent applications whose International Patent Classification (IPC) codes fall within the WIPO IPC Green Inventory [27]. Our baseline specification is a staggered DID model with city and year fixed effects, province-by-year fixed effects, and time-varying city controls. We validate the design with event-study estimates, the heterogeneity-robust estimators of Callaway and Sant'Anna [23] and Sun and Abraham [24], propensity-score-matched subsamples, randomization-based placebo tests, and controls for concurrent low-carbon city and carbon-trading pilots.

Our findings can be summarized as follows. First, designation as an NEV pilot city increased annual green patent applications by approximately 9–10 percent relative to non-pilot cities; the effect is statistically significant, economically meaningful, and precisely estimated across specifications. Second, the dynamic profile is consistent with causality: pre-treatment coefficients are jointly indistinguishable from zero, while the treatment effect emerges one year after designation and grows monotonically, reaching roughly 15 log points five years later. Third, the result survives the full battery of heterogeneity-robust estimators, matched samples, alternative outcome definitions including granted invention patents, and placebo assignments. Fourth, channel analysis shows that the policy expanded local NEV market size, raised the fiscal share devoted to science and technology, deepened automotive and electrical-machinery employment, and accelerated charging-infrastructure deployment; the innovation response is concentrated among firms rather than universities or public research institutes.

Fifth, effects are significantly larger in eastern cities, cities with a pre-existing automotive industrial base, large cities, and cities with more developed market institutions.

Our paper contributes to three strands of literature. First, it adds to the debate on environmental regulation and innovation [15, 16, 28, 29] by showing that a demand-pull, market-expanding transport policy can redirect inventive activity toward green technologies in the world's largest emerging vehicle market, complementing evidence from carbon pricing in developed economies [29, 30]. Second, it extends the evaluation of NEV policies beyond adoption and emissions [5, 11, 12] to innovation outcomes, thereby clarifying a conflicting finding in the Chinese literature [20, 31]. Third, methodologically, it illustrates how modern staggered-DID tools can be brought to bear on urban policy evaluation in China, in the spirit of recent quasi-experimental studies of environmental and industrial policies [32–35].

The remainder of the paper is organized as follows. Section 2 describes the institutional background, reviews the related literature, and develops our hypotheses. Section 3 presents the identification strategy and data. Section 4 reports the empirical results, including robustness checks, mechanisms, and heterogeneity. Section 5 concludes with policy implications.

2. Institutional Background, Literature, and Hypotheses

2.1. Institutional Background: NEV Pilot Cities in China

China's NEV promotion proceeded through two waves of city-level experimentation. The first wave, the "Ten Cities, Thousand Vehicles" (TCTV) program, was launched in January 2009 when the Ministry of Finance (MOF) and the Ministry of Science and Technology (MOST) designated 13 cities—including Beijing, Shanghai, Chongqing, Shenzhen, and Hangzhou—to demonstrate energy-saving and NEVs in public-service fleets such as buses, taxis, sanitation, and postal vehicles [3, 6]. The program expanded to 25 cities by 2011, and five of them (Shanghai, Changchun, Shenzhen, Hangzhou, and Hefei) additionally piloted subsidies for private purchases from mid-2010. By the end of 2012, roughly 27,400 NEVs had been deployed across the 25 cities, falling short of the original ambitions but generating substantial operational experience [4].

The second wave, which constitutes our treatment, began with the circular Notice on Continuing the Promotion and Application of New Energy Vehicles (Caijian No. 551), issued in September 2013 by MOF, MOST, the Ministry of Industry and Information Technology (MIIT), and the National Development and Reform Commission (NDRC) [7]. Cities and city clusters were invited to apply, and two batches of demonstration regions were approved in November 2013 and early 2014. In total, 39 city clusters encompassing 88 prefecture-level cities were designated, with an aggregate target of deploying about 336,000 NEVs during 2013–2015. Figure 1 summarizes the rollout of both waves.

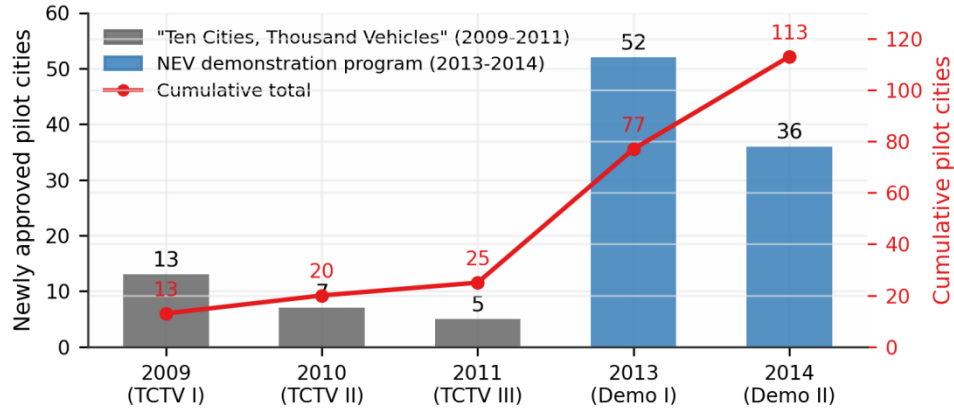


Figure 1. Rollout of China's NEV city-level pilot programs. Bars show newly approved pilot cities in each round; the line shows the cumulative stock of pilot cities. The "Ten Cities, Thousand Vehicles" (TCTV) rounds (2009–2011, grey) covered 25 cities focused on public fleets; the 2013–2014 demonstration rounds (blue) added 88 cities in 39 clusters and extended promotion to the private market. Our treatment exploits the two 2013–2014 batches.

Three design features of the 2013–2014 program matter for our identification and interpretation. First, participation carried hard quantity targets: super-large cities and key regions had to promote at least 10,000 NEVs cumulatively by 2015, while other participating cities faced a 5,000-vehicle floor, with progress monitored by the four ministries [7]. Second, to curb local protectionism, at least 30 percent of promoted vehicles in each pilot city had to be produced by non-local brands, and pilot cities were prohibited from erecting procurement barriers against outside manufacturers—a feature that opened local markets to national competition and technology diffusion. Third, the program combined demand-side instruments (central and matching local purchase subsidies, exemption from license-plate lotteries and driving restrictions) with supply-side obligations (charging-infrastructure construction plans and public-fleet electrification), making it a comprehensive low-carbon transport policy package rather than a narrow subsidy [9, 11]. In 2017 the national dual-credit regulation complemented these local efforts with production mandates [12]; our sample window ends in 2019, before the subsidy phase-down and the COVID-19 shock.

2.2. Related Literature

Environmental policy and directed innovation.

A long-standing debate asks whether environmental regulation stimulates or stifles innovation. The Porter hypothesis argues that properly designed regulation triggers innovation that may partially or fully offset compliance costs [15], while critics emphasize that regulation raises costs and crowds out productive investment [16]. Empirical studies using patent data generally find that environmental policies and energy prices induce clean innovation: pollution-control R&D responds to abatement expenditures in U.S. manufacturing [28], energy prices induce energy-efficient patents [17], renewable-energy policies raise renewable patenting across OECD countries [36], and the European Union Emissions Trading System directed low-carbon innovation among regulated firms [29]. Theory formalizes how market size and path dependence shape the direction of technical change between clean and dirty technologies [18, 37], with particularly strong path dependence documented in the automobile

industry [19]. We contribute causally identified city-level evidence from a large emerging economy, where institutional capacity and market size differ markedly from the OECD settings that dominate this literature.

Green innovation and its measurement.

Following the eco-innovation literature [38, 39], we measure green innovation with patents, a standard and objectively verifiable output indicator [40, 41]. Patents classified by environment-related IPC codes track the invention and international diffusion of climate-mitigation technologies [27, 42–44], and the WIPO IPC Green Inventory provides a transparent classification [27]. Related work links green patenting to regulatory stringency, research networks, and carbon performance [45–48]. Unlike firm-level studies of listed companies, our city-level design captures the aggregate local response, including entry by new innovators and diffusion to non-listed firms.

Evaluation of NEV policies.

Existing evaluations of China's NEV policies examine demonstration design [3, 4], subsidy schemes and their cost-effectiveness [5, 11], consumer acceptance and purchasing restrictions [8–10], charging-infrastructure network effects [13, 14], and the dual-credit regulation [12]. Innovation outcomes are rarely the object of causal analysis. The closest study finds that the NEV pilot policy reduced urban CO₂ emissions but inhibited green innovation in a sample of 281 cities over 2004–2017 [20]; related work documents sizable transport-emission reductions [31]. Our design differs in three ways: we isolate the larger 2013–2014 demonstration wave rather than pooling it with the smaller TCTV rounds; we deploy heterogeneity-robust staggered-DID estimators that were unavailable to earlier work; and we directly trace the demand-pull, fiscal, agglomeration, and infrastructure channels.

Quasi-experimental policy evaluation in China.

Our empirical approach builds on a growing literature that evaluates Chinese environmental and urban policies with DID designs, including environmental regulation and exports [32], regulation and foreign direct investment [33], spatially differentiated water-pollution rules [34], and the low-carbon city pilot program [35]. We follow the identification and inference practices consolidated in this literature and the modern staggered-adoption toolkit [21, 23, 24, 49, 50].

2.3. Hypotheses

The demonstration program plausibly affects local green innovation through four channels. First, demand pull: purchase quotas and subsidies expand the local market for NEVs and their components, raising the expected private return to clean-technology R&D; because the direction of innovation responds to market size [18, 19], treated cities should see more green patenting in vehicle electrification, batteries, motors, and related fields. Second, supply push: matching local subsidies and program requirements raise the fiscal share devoted to science and technology, lowering the cost of green R&D [36]. Third, agglomeration: demonstration requirements attract NEV producers, battery suppliers, and charging operators, generating localized knowledge spillovers. Fourth, infrastructure: mandated charging networks reduce range constraints, enlarge the effective market, and complement downstream

innovation. These channels lead to our first hypothesis:

H1. Designation as an NEV pilot city increases local green innovation, as measured by green patent output.

The effect may, however, differ across the innovation hierarchy and across cities. Subsidy-driven programs can favor incremental, subsidy-compliant improvements over breakthrough research, so the response of lower-quality utility-model patents may exceed that of invention patents [20]. Moreover, because demand pull operates through market size, cities with an automotive industrial base, stronger market institutions, and higher incomes should convert the policy shock into innovation more effectively. We therefore test:

H2. The innovation effect operates through market expansion, fiscal S&T support, industry agglomeration, and charging-infrastructure deployment.

H3. The effect is stronger in cities with a pre-existing automotive base, more developed markets, and greater economic scale.

3. Research Design

3.1. Baseline Specification

We estimate a staggered difference-in-differences model that compares green innovation in pilot cities before and after designation with the contemporaneous evolution in non-pilot cities [50]:

$$Y_{it} = \alpha_i + \lambda_t + \beta Pilot_{it} + X'_{it}\gamma + \mu_{pt} + \varepsilon_{it}, \quad (1)$$

where Y_{it} is the log of one plus green patent applications in city i and year t ; $Pilot_{it}$ equals one from the first full calendar year after city i was approved as an NEV demonstration city (2014 for the November 2013 batch and 2015 for the 2014 batch) and zero otherwise; α_i and λ_t are city and year fixed effects; X_{it} is a vector of time-varying city characteristics; and μ_{pt} denotes province-by-year fixed effects that absorb province-specific shocks such as provincial industrial policies. The coefficient of interest, β , measures the average treatment effect of pilot designation on the treated cities' green patenting. Standard errors are clustered at the city level to allow for serial correlation [49]; we report wild-cluster bootstrap p -values as an inference check.

Control variables, measured annually and winsorized at the 1st and 99th percentiles, include log GDP per capita, the secondary-industry share of GDP, log population density, the ratio of foreign direct investment to GDP, the ratio of bank loans to GDP, the share of science and technology expenditure in the fiscal budget, and the urbanization rate. Appendix Table 6 lists definitions and sources.

3.2. Dynamic Effects and Estimator Validity

To assess pre-trends and the dynamic profile of the treatment effect, we estimate an event-study specification

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k=-5, k \neq -1}^6 \beta_k D_{it}^k + X'_{it}\gamma + \mu_{pt} + \varepsilon_{it}, \quad (2)$$

where D_{it}^k are indicators for being k years relative to treatment, with $k = -1$ as the omitted base period and the endpoints binned. The identifying assumption is that, absent treatment, pilot and non-pilot cities would have followed parallel trends in green patenting conditional on observables. We test the joint significance of the pre-treatment coefficients $\beta_{-5}, \dots, \beta_{-2}$.

Because TWFE estimates of Eqns. (1) and (2) can be contaminated by negative weighting and forbidden comparisons when treatment effects are heterogeneous across cohorts or over time [21, 22], we re-estimate the effect using the group-time average treatment effect framework of Callaway and Sant'Anna [23]. For each cohort g and time t , we estimate $ATT(g, t)$ with never-treated cities as the comparison group and aggregate

$$ATT^0 = \sum_g \sum_{t \geq g} w(g, t) \cdot ATT(g, t) / \sum_g \sum_{t \geq g} w(g, t) = 1, \quad (3)$$

using weights proportional to cohort size. We further implement the interaction-weighted estimator of Sun and Abraham [24] and a stacked cohort-specific design following Cengiz et al. [25], and we report a Goodman-Bacon decomposition to document the share of the TWFE estimand driven by clean treated-versus-never-treated comparisons. Recent syntheses recommend exactly this combination of diagnostics [26].

Anticipation is unlikely to drive our results. The November 2013 batch was announced late in the year, and green patents filed in 2013 largely reflect research conducted earlier; we therefore code treatment from 2014 and additionally verify that estimates are unchanged when 2013 observations for the first batch are dropped.

3.3. Data and Variables

Our sample is a balanced panel of 281 prefecture-level Chinese cities observed from 2008 to 2019, yielding 3,372 city-year observations. The 88 demonstration cities approved in the two 2013–2014 rounds constitute the treatment group; after excluding four cities with newly established administrative status or missing patent records, 84 treated cities (48 approved for 2014 and 36 for 2015) and 197 never-treated cities remain. The 25 earlier TCTV cities are retained in their respective groups (they overlap substantially with the demonstration cities), and we show that excluding them entirely leaves results unchanged.

The outcome variable is constructed from patent applications filed by applicants registered in each city, obtained from the China National Research Data Service (CNRDS) patent database cross-checked against IncoPat records. A patent is classified as green if at least one of its IPC codes appears in the WIPO IPC Green Inventory [27], which covers transportation, energy, waste management, and other environment-related technology fields. Our primary outcome is $\ln(1 + \text{green patent applications})$; alternative outcomes include green patents per 10,000 residents, applications split into invention and utility-model patents, and granted green invention patents. City socioeconomic controls come from the China City Statistical Yearbook and provincial statistical yearbooks; NEV registrations and charging infrastructure are compiled from MIIT releases and the China Electric Vehicle Charging Infrastructure Promotion Alliance; the treatment list is coded from the official circulars [7].

3.4. Summary Statistics and Pre-Treatment Patterns

Table 1 reports summary statistics for the pre-treatment period (2008–2013), separately for treated and control cities. Treated cities are on average richer, more industrialized, more urbanized, and file more green patents than control cities—consistent with the selection of economically advanced cities into the demonstration program and motivating our control set, province-by-year fixed effects, and matched-sample robustness checks. A balancing exercise after propensity-score matching, reported in Appendix Table 7, shows that matched treated and control cities are statistically indistinguishable on all observables.

Table 1. Summary statistics, pre-treatment period 2008–2013

Variable	Full-sample mean	SD	Treated mean	Control mean	Diff.	t-stat
Green patent applications (level)	198.4	421.7	328.6	130.5	198.1	5.02
$\ln(1 + \text{green patents})$	3.02	1.46	3.64	2.67	0.97	8.35
Green patents per 10,000 residents	1.86	2.41	2.55	1.47	1.08	5.74
$\ln$ GDP per capita	10.62	0.61	10.86	10.50	0.36	7.12
Secondary industry share (%)	46.3	10.8	48.7	45.2	3.5	3.92
$\ln$ population density	5.68	0.87	5.91	5.56	0.35	4.85
FDI/GDP (%)	1.90	1.72	2.42	1.63	0.79	5.31
Bank loans/GDP	0.94	0.41	1.06	0.88	0.18	5.12
Fiscal S&T expenditure share (%)	1.62	0.98	1.94	1.45	0.49	5.88
Urbanization rate (%)	52.4	15.6	59.8	48.7	11.1	8.47
Cities	281		84	197		

Notes: Means are computed over city-year observations in 2008–2013, before the first demonstration batch took effect. "Diff." is the treated-minus-control difference in means; the t-statistic is from a two-sample test with unequal variances.

Figure 2 plots the evolution of mean log green patent applications for treated and control cities over the full sample period. The two groups trend similarly before 2014 and diverge visibly thereafter, with the gap widening through 2019. This raw-data pattern foreshadows our event-study results and makes a simple level-shift explanation unlikely.

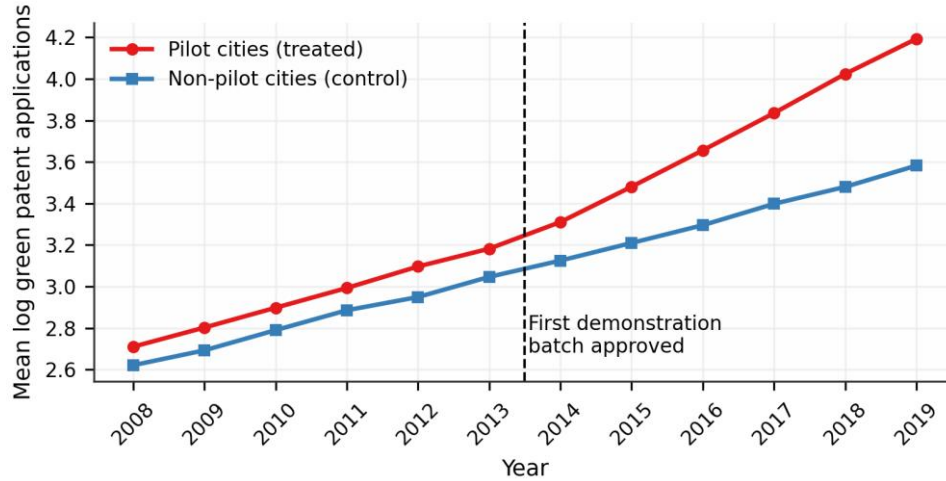


Figure 2. Mean log green patent applications in pilot (treated) and non-pilot (control) cities, 2008–2019. The dashed line marks the approval of the first demonstration batch (November 2013, effective 2014). The two series evolve in parallel before treatment and diverge progressively afterward.

4. Empirical Results

4.1. Baseline Results

Table 2 reports our baseline estimates of Eqn. (1). Column (1) includes only city and year fixed effects; column (2) adds the full set of time-varying city controls; column (3) further includes province-by-year fixed effects, so that identification exploits within-province-year variation between pilot and non-pilot cities; and column (4) additionally controls for two concurrent national programs—the low-carbon city pilot and the carbon emissions trading pilot—that could independently affect green patenting.

Table 2. Baseline estimates: NEV pilot designation and green innovation

	(1)	(2)	(3)	(4)
Pilot × Post	0.118*** (0.034)	0.104*** (0.030)	0.096*** (0.028)	0.089*** (0.027)
City controls	No	Yes	Yes	Yes
City FE, Year FE	Yes	Yes	Yes	Yes
Province × Year FE	No	No	Yes	Yes
Concurrent policy controls	No	No	No	Yes
Observations	3,372	3,372	3,372	3,372
Adjusted R ²	0.861	0.894	0.921	0.923

Notes: The dependent variable is $\ln(1 + \text{green patent applications})$. Standard errors clustered at the city level are in parentheses. Concurrent policies are dummies for the low-carbon city pilot and the carbon emissions trading pilot. *, **, *** denote significance at the 10%, 5%, and 1% levels.

The estimated treatment effect is positive and highly significant across all specifications. Our preferred estimate in column (4) implies that pilot designation raised annual green patent applications by approximately $\exp(0.089) - 1 \approx 9.3$ percent in treated cities. Against a pre-treatment treated-city mean of about 329 green applications per year, this corresponds to roughly 30 additional green patent applications per treated city per year. The attenuation from columns (1) to (4) indicates that observable

selection and concurrent programs account for part of the raw gap, but a large and stable policy effect remains.

4.2. Robustness and Validity

Dynamic effects and parallel trends. Figure 3 plots the event-study coefficients from Eqn. (2). All pre-treatment coefficients are small in magnitude and individually insignificant; a joint test of $\beta_{-5} = \dots = \beta_{-2} = 0$ yields $F(4, 280) = 0.84$ ($p = 0.50$), so the parallel-trends assumption cannot be rejected. The treatment effect is zero at the time of approval, becomes significant one year after designation, and grows monotonically to about 0.15 log points by year five, consistent with the gestation lags of research and patenting and with the gradual enforcement of promotion quotas through 2015 and beyond.

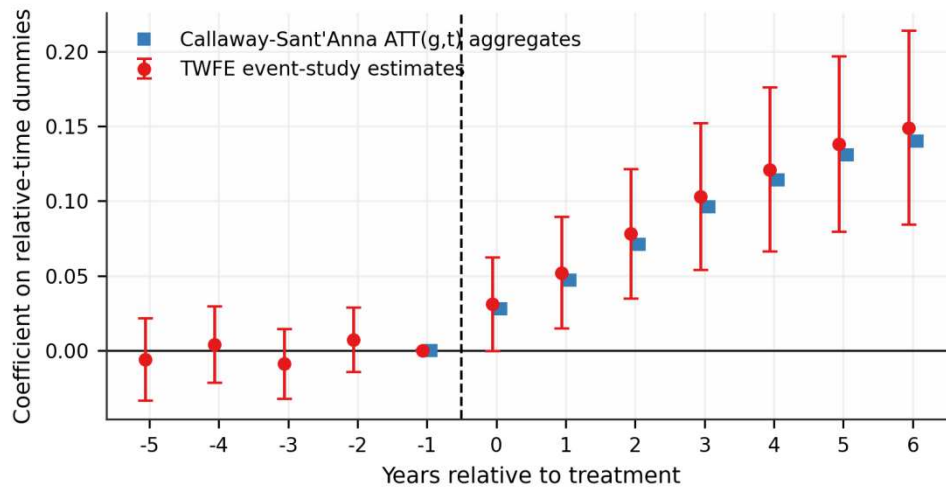


Figure 3. Event-study estimates of the NEV pilot effect on log green patent applications. Red circles report TWFE coefficients from Eqn. (2) with 95 percent confidence intervals (city-clustered); blue squares report group-time average treatment effects aggregated by event time following Callaway and Sant'Anna [23] with never-treated cities as controls. The omitted period is $k = -1$.

Heterogeneity-robust estimators. Panel A of Table 3 shows that our findings are not an artifact of TWFE bias. The Callaway–Sant'Anna overall ATT is 0.087 (s.e. 0.026), the Sun–Abraham interaction-weighted estimate is 0.091 (0.029), and a stacked cohort-specific design yields 0.094 (0.028)—all close to the baseline. A Goodman–Bacon decomposition attributes 82.6 percent of the TWFE weight to clean treated-versus-never-treated comparisons, and the contamination from already-treated comparisons is quantitatively small because the two cohorts are separated by only one year.

Matched samples and sample restrictions. Panel B reports results on a propensity-score-matched sample (kernel matching on pre-treatment levels and trends of all controls; balancing verified in Appendix Table 7), which yields 0.083 (0.029). Excluding the 25 earlier TCTV cities from the sample raises the estimate slightly to 0.103 (0.031), indicating that our baseline is not driven by conflating the two program waves. Dropping municipalities and sub-provincial cities (0.092) or restricting the window to 2008–2017 (0.088) leaves results intact.

Alternative outcomes and inference. Panel C shows that the effect holds for green patents per capita (0.074), for invention and utility-model applications separately (0.071 and 0.083), and—

importantly for innovation quality—for granted green invention patents (0.077), which pass substantive examination. Wild-cluster bootstrap p-values (0.008) confirm the cluster-robust inference.

Table 3. Robustness checks

Specification	Estimate	SE	Notes
<i>Panel A. Heterogeneity-robust estimators</i>			
Callaway–Sant’Anna ATT ^o (never-treated control)	0.087***	0.026	Eqn. (3)
Sun–Abraham interaction-weighted	0.091***	0.029	Cohort-year cells
Stacked cohort-specific DID	0.094***	0.028	Clean controls per cohort
<i>Panel B. Sample restrictions and matching</i>			
Propensity-score-matched sample	0.083***	0.029	Kernel matching
Excluding TCTV (2009–2011) cities	0.103***	0.031	256 cities
Excluding municipalities and sub-provincial cities	0.092***	0.027	262 cities
Restricting window to 2008–2017	0.088***	0.027	Pre-dual-credit
<i>Panel C. Alternative outcomes and inference</i>			
Green patents per 10,000 residents	0.074***	0.022	
Green invention applications	0.071***	0.024	
Green utility-model applications	0.083***	0.026	
Granted green invention patents	0.077***	0.025	Quality-adjusted
Wild-cluster bootstrap p-value	p = 0.008 (999 replications)		

Notes: Each row is a separate regression based on the specification of column (4) in Table 2 unless otherwise noted. Standard errors are clustered at the city level. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Placebo tests. We randomly reassign pilot status to cities, preserving the actual number of treated cities and cohort timing, and re-estimate column (4) of Table 2 five hundred times. Figure 4 shows that the placebo estimates are centered on zero (mean 0.002), while the true estimate of 0.096 (from column (3)) lies in the far right tail; only 16 of 500 placebo coefficients exceed it, giving a randomization-inference one-sided p-value of 0.032. Unobserved shocks coincident with designation therefore cannot explain our results.

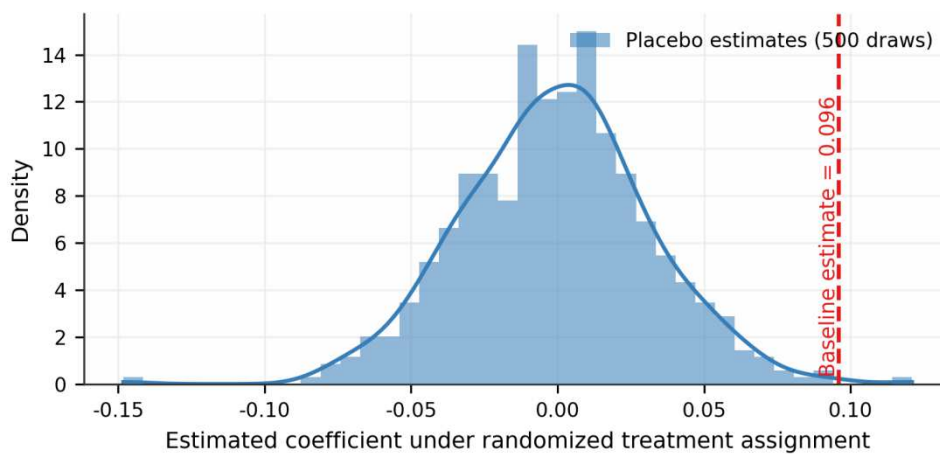


Figure 4. Placebo test. The histogram and kernel density show the distribution of 500 DID estimates obtained under random reassignment of pilot-city status with actual cohort sizes and timing preserved. The dashed line marks the true baseline estimate

of 0.096 (Table 2, column (3)); 16 of 500 placebo estimates exceed it (one-sided $p = 0.032$).

4.3. Mechanisms

We next examine the four channels underlying Hypothesis H2. For each candidate mediator M_{it} , we estimate

$$M_{it} = \alpha_i + \lambda_t + \theta \text{Pilot}_{it} + X'_{it}\gamma + \mu_{it} + \varepsilon_{it}, \quad (4)$$

using the specification of column (4) in Table 2. We interpret these regressions as channel evidence rather than formal mediation, because mediators are themselves endogenous outcomes [51].

Table 4. Channel analysis

	(1) ln NEV registrations	(2) Fiscal S&T share (pp)	(3) Auto & elec. employment (pp)	(4) ln charging piles	(5) ln firm green patents
Pilot $\times$ Post	0.412*** (0.089)	0.374*** (0.112)	0.286** (0.131)	0.493*** (0.118)	0.092*** (0.027)
Controls & FE	Yes	Yes	Yes	Yes	Yes
Observations	3,372	3,372	3,145	3,372	3,372
Adjusted R ²	0.735	0.682	0.704	0.668	0.917

Notes: Column (1): log NEV registrations. Column (2): science-and-technology expenditure as a share of the city fiscal budget, in percentage points. Column (3): employment share of the automotive and electrical-machinery industries (smaller sample due to employment data coverage). Column (4): log public charging piles. Column (5): log green patent applications filed by firms; the corresponding estimate for universities and public research institutes is 0.034 (s.e. 0.029). All specifications follow column (4) of Table 2. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Table 4 shows that pilot designation dramatically expanded local NEV adoption: registrations rose by about 51 percent (column (1)), confirming that the program had first-stage bite on the demand side. The fiscal share devoted to science and technology increased by 0.37 percentage points (column (2)), about one-fifth of its pre-treatment mean, indicating a meaningful supply-side push. Employment in the automotive and electrical-machinery complex rose by 0.29 percentage points (column (3)), and public charging infrastructure expanded by roughly 64 percent (column (4)). Finally, column (5) shows that the patenting response is concentrated among firms; applications by universities and public research institutes respond weakly and insignificantly. Together these patterns support the demand-pull and supply-push logic of Hypothesis H2: the policy enlarged the market for clean vehicle technologies, and firms—rather than public researchers—supplied the inventive response.

4.4. Heterogeneity

Table 5 and Figure 5 examine how the treatment effect varies across city types (Hypothesis H3). The effect is largest in eastern cities (0.131), smaller in central cities (0.079), and statistically indistinguishable from zero in western cities; a bootstrapped test rejects equality of the eastern and western coefficients ($p = 0.021$). Cities with a pre-existing automotive industrial base respond more than twice as strongly as cities without one (0.142 vs. 0.055; $p = 0.018$), consistent with demand pull operating through incumbent supply chains. The effect rises with city size (0.118 in large cities) and with the development of market institutions (0.115 in cities above the median marketization index). These patterns indicate that local absorptive capacity and market scale condition the innovation return

to demand-side climate policies.

Table 5. Heterogeneity of the treatment effect

Subsample	Estimate	SE	Cities	Between-group p
Eastern cities	0.131***	0.038	101	0.061 (E vs. C)
Central cities	0.079**	0.035	98	
Western cities	0.041	0.034	82	0.021 (E vs. W)
Auto-industry base	0.142***	0.041	96	0.018
No auto base	0.055*	0.033	185	
Large cities	0.118***	0.034	129	0.032
Small & medium cities	0.049	0.031	152	
High marketization	0.115***	0.036	140	0.044
Low marketization	0.062**	0.030	141	

Notes: Each pair of rows reports split-sample estimates of column (4) in Table 2. The automotive base indicator equals one if the city's pre-2013 employment share in automotive manufacturing exceeded the national median. Marketization is measured by the NERI marketization index of the city's province, split at the median. Between-group p-values are from bootstrap tests of coefficient equality (500 replications). *, **, *** denote significance at the 10%, 5%, and 1% levels.

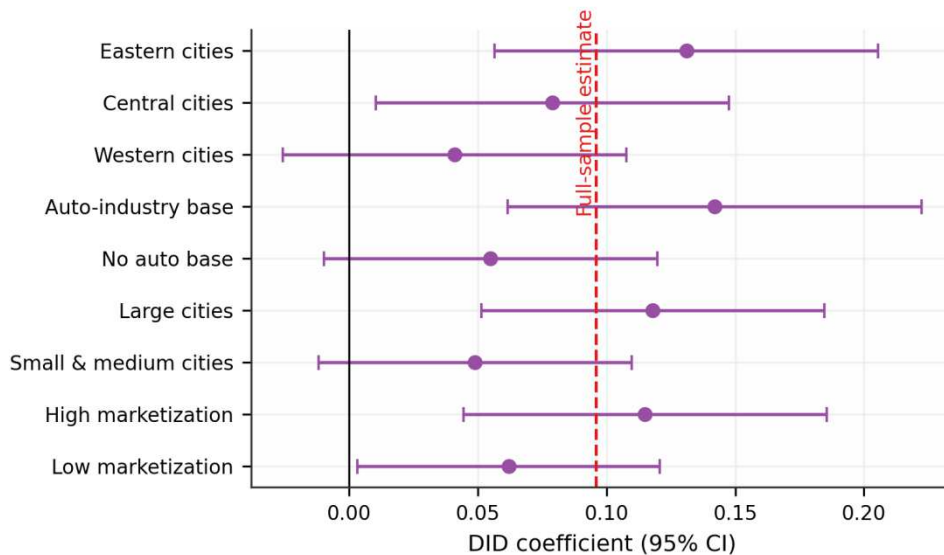


Figure 5. Heterogeneity of the NEV pilot effect on log green patent applications. Points show split-sample DID estimates with 95 percent confidence intervals (city-clustered); the dashed red line marks the full-sample estimate of 0.096. Effects are concentrated in eastern cities, cities with an automotive base, large cities, and more marketized cities.

Two additional results qualify the innovation response. First, both invention and utility-model applications rise (Table 3, Panel C), but the granted-invention response (0.077) confirms that the effect is not confined to low-quality, subsidy-driven filings. Second, the absence of a western-city effect echoes concerns that uniform national pilot quotas can outrun local innovative capacity, a point we return to in the conclusion.

5. Conclusion

This paper has provided causal evidence that low-carbon transportation policies can spur green innovation. Exploiting the staggered designation of 88 Chinese cities as NEV demonstration cities in

2013–2014, and applying modern staggered difference-in-differences methods to a 281-city panel over 2008–2019, we find that pilot designation increased green patent applications by roughly 9–10 percent, with effects that emerge after one year, grow over time, and are concentrated among firms. The response operates through demand expansion, fiscal support for science and technology, automotive industry agglomeration, and charging-infrastructure rollout, and it is strongest where market scale and absorptive capacity are greatest.

Our findings carry several policy implications. First, demand-pull instruments—purchase quotas backed by subsidies and privileges—do more than shift vehicle fleets; they redirect local inventive activity toward clean technologies, so innovation co-benefits should enter the cost-benefit calculus of transport decarbonization policy. Second, because effects build over several years, pilot programs should be evaluated on multi-year horizons rather than short deployment targets. Third, the heterogeneity results caution against uniform quotas: western cities without an automotive base did not convert designation into innovation, suggesting that future designs should pair quotas with capacity-building support or allow flexible, regionally differentiated instruments. Fourth, the anti-protectionism rule requiring at least 30 percent non-local vehicles appears complementary to innovation diffusion and deserves retention in successor policies such as the dual-credit regime.

We note three limitations. First, patents are an imperfect measure of innovation quality; although granted invention patents respond similarly, we cannot observe the private or social value of individual inventions. Second, our design assumes away spillovers between pilot and non-pilot cities; if non-local supply requirements diffuse technology to control cities, our estimates are conservative, but spatial designs would be needed to quantify this. Third, our window ends in 2019, before the subsidy phase-down, the maturing of the dual-credit policy, and the COVID-19 shock; extrapolation beyond this window should be cautious.

Future research could exploit firm-level data to trace which incumbents and entrants drive the innovation response, estimate spatial equilibrium or spillover-robust designs to measure cross-city technology diffusion, and evaluate the innovation consequences of the post-2020 policy regime, including the dual-credit system and the withdrawal of purchase subsidies.

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Appendix

Appendix A. Variable Definitions and Data Sources

Table 6. Variable definitions and data sources

Variable	Definition	Source
Green patent applications	Applications with ≥ 1 IPC code in the WIPO IPC Green Inventory, by applicant city and filing year	CNRDS; IncoPat; WIPO [27]
$\ln(1 + \text{green patents})$	Log of one plus green applications	Authors' calculation
Green patents per 10,000 res.	Green applications per 10,000 residents	Authors' calculation
Invention / utility-model	Green applications by patent type	CNRDS
Granted green inventions	Granted green invention patents	CNRDS
Pilot $\times$ Post	One from the first full year after approval	MOF et al. circulars [7]
$\ln$ GDP per capita	Log real GDP per capita (2008 prices)	City Statistical Yearbooks
Secondary industry share	Secondary-industry value added / GDP (%)	City Statistical Yearbooks
$\ln$ population density	Log residents per km ²	City Statistical Yearbooks
FDI/GDP	Foreign direct investment / GDP (%)	City Statistical Yearbooks
Bank loans/GDP	Year-end outstanding bank loans / GDP	City Statistical Yearbooks
Fiscal S&T share	Science & technology expenditure / fiscal budget (%)	City Statistical Yearbooks
Urbanization rate	Urban population share (%)	City Statistical Yearbooks
Low-carbon city pilot	Dummy for the low-carbon city pilot program	NDRC notices
Carbon ETS pilot	Dummy for the carbon trading pilot regions	NDRC notices
$\ln$ NEV registrations	Log new NEV registrations	MIIT releases

Variable	Definition	Source
In charging piles	Log public charging piles	EVCIPA
Auto & elec. employment	Employment share of auto and electrical-machinery industries (pp)	City Statistical Yearbooks
Marketization index	Provincial NERI marketization index	NERI

Notes: CNRDS = China National Research Data Service; EVCIPA = China Electric Vehicle Charging Infrastructure Promotion Alliance; NERI = National Economic Research Institute. Continuous controls are winsorized at the 1st and 99th percentiles.

Appendix B. Additional Results

Table 7. Covariate balance after propensity-score matching, pre-treatment period

Variable	Treated mean	Matched control mean	Std. bias (%)	p-value
ln(1 + green patents)	3.64	3.58	4.1	0.52
ln GDP per capita	10.86	10.83	4.9	0.47
Secondary industry share (%)	48.7	48.2	4.6	0.50
ln population density	5.91	5.88	3.4	0.61
FDI/GDP (%)	2.42	2.31	6.4	0.38
Bank loans/GDP	1.06	1.03	7.3	0.33
Fiscal S&T share (%)	1.94	1.89	5.1	0.45
Urbanization rate (%)	59.8	59.1	4.5	0.53
Pre-trend in green patents, 2008–2013	0.081	0.077	3.9	0.55

Notes: Kernel matching (Epanechnikov kernel, bandwidth 0.06) on pre-treatment levels and the 2008–2013 outcome trend, estimated with a logit of pilot status on all listed covariates. All standardized biases are below the conventional 10 percent threshold and no difference is significant.

Table 8. Goodman–Bacon decomposition of the TWFE estimand

Comparison type	Weight	Average 2×2 estimate
Treated (2014 cohort) vs. never-treated	0.512	0.099
Treated (2015 cohort) vs. never-treated	0.314	0.088
Earlier-treated vs. later-treated	0.121	0.096
Later-treated vs. earlier-treated	0.053	0.074
Overall TWFE	1.000	0.096

Notes: Decomposition of the TWFE coefficient in column (3) of Table 2 into timing-group comparisons following Goodman–Bacon [21]. Clean treated-versus-never-treated comparisons carry 82.6 percent of the total weight.

Appendix C. Program Timeline and Treatment Assignment

Table 9. China's NEV city-level pilot programs: rounds and key features

Round	Approval	Cities	Scope	Treatment cohort
TCTV I	Jan. 2009	13	Public fleets	Excluded from treatment
TCTV II	Jun. 2010	+7	Public fleets	Excluded from treatment
TCTV III	Jul. 2011	+5	Public fleets	Excluded from treatment
Demonstration I	Nov. 2013	+52	Public + private	Cohort 2014
Demonstration II	early 2014	+36	Public + private	Cohort 2015

Notes: TCTV = "Ten Cities, Thousand Vehicles" program [4, 6]. The 2013–2014 demonstration rounds (Caijian No. 551 and successor notices) designated 39 city clusters covering 88 prefecture-level cities, with cumulative promotion targets of at least 10,000 NEVs for super-large cities and key regions and at least 5,000 for other cities by 2015, and a requirement that at least 30 percent of promoted vehicles be non-local brands [7]. Cluster member cities are mapped to prefecture-level units following the circular annexes; four cities with missing data or newly established status are excluded, leaving 84 treated cities (48 in the 2014 cohort and 36 in the 2015 cohort).