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Zero-waste City Policy and the Short-Lived Efficiency Window in Urban Green Innovation: Evidence from China's Staggered Pilot

Peiwen Lu
China University of Geosciences
Shouting Zhang
zst@cugb.edu.cn

China University of Geosciences
Kailingli Liao
Tsinghua University
Jinping Tian
Tsinghua University
Lyujun Chen
Tsinghua University

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Abstract

Zero-waste city policy is expected to improve urban environmental performance, but whether direct waste-governance achievements translate into durable innovation-system efficiency remains unclear. This study evaluates China's Zero-waste City Pilot (ZWCP) as a staggered urban environmental policy and examines its effect on urban green innovation efficiency (GIE). Using a balanced panel of 210 Chinese prefecture-level cities from 2012 to 2023, we combine a two-stage slacks-based DEA model with the Callaway and Sant'Anna (2021) doubly robust difference-in-differences estimator. The DEA framework decomposes GIE into knowledge-production efficiency (E_1) and economic-conversion efficiency (E_2). The results show that ZWCP raises GIE only in the launch year: the 2020 calendar-time ATT is 0.0330 ($p = 0.043$; about 3.35%), with gains in E_1 (+4.64%) and E_2 (+2.26%), whereas the post-2020 effects and the overall ATT are insignificant. By contrast, the industrial solid waste comprehensive utilization rate increases by 5.93 percentage points and keeps rising, indicating successful achievement of a direct environmental policy target rather than a durable spillover to innovation-system efficiency. Channel evidence points mainly to policy-induced industrial restructuring, which lowers the tertiary-to-secondary output ratio and accounts for about 17% of the GIE effect. These findings suggest that zero-waste environmental policy can open a short-lived efficiency window, but lasting green innovation efficiency requires sustained technology diffusion, market incentives, and performance evaluation beyond waste-utilization compliance.

Keywords: Zero-waste city policy; Environmental policy evaluation; Urban green innovation efficiency (GIE); Two-stage DEA; Staggered difference-in-differences

1. Introduction

Urban waste governance has increasingly become a comprehensive environmental policy issue rather than a narrow end-of-pipe management task. Cities now use waste reduction, resource recovery, cleaner production, and industrial upgrading policies to reshape urban metabolism and the efficiency with which innovation inputs are converted into green knowledge and economic value (John et al., 2019; Mendizabal et al., 2018). In late 2018, China issued the Zero-waste City Pilot Work Plan (ZWCP) (General Office of the State Council, 2018). The first 11 pilot cities were announced in 2019, and the program expanded in 2022 (General Office of the Ministry of Ecology and Environment, 2022). The staggered rollout provides a useful setting for asking whether a Zero-waste environmental policy can improve the efficiency of urban green innovation systems.

From an environmental policy evaluation perspective, the key question is not only whether ZWCP increases green patents or reduces waste, but whether it improves the efficiency with which cities transform public and private innovation inputs into green knowledge and environmentally adjusted economic output. Earlier studies have examined ZWCP's effects on urban green and low-carbon transition and firm-level green transformation (Liu et al., 2024; Qian et al., 2025), but urban green innovation efficiency (GIE) remains underexplored. Few studies separate the knowledge-production stage from the economic-conversion stage, and even fewer examine whether the effects accumulate over time or concentrate in the launch window. What is still missing is a city-level planning account of how pilot status is translated into broader green innovation efficiency gains.

Identifying these effects is not straightforward. The first ZWCP cohort was announced in 2019 and rolled out in 2020, with a second cohort in 2022. The setting is therefore staggered, and conventional two-way fixed effects DID can be biased once treatment effects vary across cohorts and time (Callaway and Sant'Anna, 2021; De Chaisemartin and D'Haultfoeuille, 2020). We use the Callaway and Sant'Anna (2021) multi-period DID (CSDID) with doubly robust inverse probability weighting (Sant'Anna and Zhao, 2020) to recover group-time treatment effects. We also build a two-stage slacks-based DEA framework (Tone, 2001) that separates knowledge-production efficiency (E_1) from economic-conversion efficiency (E_2), allowing the analysis to identify both when the policy effect appears and where efficiency gains arise.

Using a balanced panel of 210 Chinese prefecture-level cities from 2012 to 2023, we find that ZWCP raised GIE significantly in the first cohort's 2020 launch year, but the effect did not carry through 2021 to 2023, and the overall ATT is insignificant. E_1 and E_2 both improved at launch, and the E_1 gain was larger. The industrial solid waste comprehensive utilization rate (ISWCU) rose sharply and kept climbing. Because its path does not match GIE's brief jump and decline, ISWCU looks like a direct policy target rather than a causal channel.

This paper makes four contributions. First, it identifies a launch-year efficiency window in which ZWCP temporarily improves urban GIE, rather than assuming that green innovation effects persist automatically. Second, it distinguishes direct environmental policy targets from broader innovation-system efficiency spillovers: ISWCU improves persistently, whereas GIE increases only briefly. Third, the two-stage DEA framework decomposes the policy effect into knowledge-production efficiency and

economic-conversion efficiency, showing where the short-run gains arise. Fourth, the staggered DID design identifies group-time treatment effects under staggered policy adoption and heterogeneous treatment dynamics. These contributions matter for environmental policy scholars and urban sustainability policymakers because they link empirical estimates to policy implementation, performance evaluation, and the design of long-term innovation incentives.

The paper is organized as follows. Section 2 reviews the literature and sets out the hypotheses. Section 3 describes the data and methodology. Section 4 reports the empirical results. Section 5 discusses mechanisms and heterogeneity, and Section 6 concludes.

2. Literature review and research hypotheses

2.1. Environmental policy, waste governance, and urban green innovation efficiency

This study lies at the intersection of environmental policy, waste governance, and urban green innovation efficiency. Environmental and infrastructure policies can be understood as instruments that coordinate regulation, investment, service coverage, and performance evaluation to achieve sustainability goals. Waste governance fits this perspective because it is not only an end-of-pipe environmental task, but also a policy system linking source reduction, resource recovery, industrial adjustment, and innovation incentives.

One strand of the literature examines waste governance as an environmental policy and resource-allocation problem. Studies of municipal solid waste facility location-allocation, medical waste collection, and food upcycling or food-bank collection-

distribution networks show that waste systems are shaped by public agencies, infrastructure constraints, service coverage, environmental risks, and sustainability trade-offs (Kale et al., 2025; Pham et al., 2026). They establish waste governance as a policy implementation problem, but usually focus on specific networks or services rather than broader innovation-system efficiency.

A second strand emphasizes efficiency measurement and process-oriented performance evaluation. Studies using two-stage efficiency analysis and network DEA diagnose where performance losses arise within multi-stage production or service systems (Zhang et al., 2026). This literature provides a methodological basis for evaluating environmental policy performance as a multi-stage process rather than as a single final outcome.

A third strand examines urban sustainability, technology transfer, green innovation, and environmental policy. Studies of technology-transfer efficiency and urban energy efficiency in China highlight the roles of technological innovation, industrial structure, and resource reallocation in urban green transformation (Liu et al., 2026; Wu et al., 2026). This literature shows that environmental policy can shape sustainability outcomes not only through pollution control, but also through efficiency changes in innovation and production systems.

These strands rarely meet in evaluations of city-level zero-waste environmental pilots. Existing waste-network studies ask where facilities should be located, how collection routes should be organized, or how service coverage and environmental risk should be balanced. They pay less attention to whether a comprehensive waste-governance policy improves the efficiency of the urban green innovation system. ZWCP

is broader than a single logistics or facility decision: it coordinates regulation, infrastructure, industrial upgrading, and innovation incentives at the city level. Evaluating such a package requires asking whether the policy improves the efficiency of the urban innovation process, whether the effect appears in knowledge production, economic conversion, or both, and whether direct waste-governance outcomes translate into durable system-efficiency spillovers. This is why the paper combines a two-stage DEA diagnosis with a staggered DID design.

2.2. Waste governance, ZWCP, and urban GIE

Waste reduction, resource recovery, and safe disposal do more than improve urban environmental quality. They also reshape regional green transitions through infrastructure, industrial organization, and local governance incentives. Recent studies show that ZWCP supports urban green and low-carbon transitions partly by promoting green technology innovation (Chai et al., 2025; Liu et al., 2024), and firm-level work documents effects on corporate green innovation activity (Huang et al., 2025; Qian et al., 2025). These studies suggest that ZWCP may affect innovation, but they mainly focus on green transformation, carbon reduction, firm behavior, or innovation activity rather than urban green innovation efficiency.

The broader GIE literature provides a basis for extending this discussion. Studies on green development efficiency and urban green economy efficiency show that innovation, industrial structure, and environmental constraints jointly shape green performance (Cheng et al., 2024; Liu and Dong, 2021). A separate strand connects circular economy governance to urban resource use and environmental performance (Bongers and Casas, 2022; Gao and Wang, 2025; Wei and Zhang, 2025), but it stops short of the urban

innovation system. As a result, we know relatively little about whether a comprehensive waste-governance pilot can improve the efficiency with which cities convert innovation inputs into green knowledge and environmentally adjusted economic output.

Counts of green patents or emission cuts capture only part of the policy story. GIE asks how efficiently innovation inputs turn into green outputs under environmental constraints (Zhao et al., 2024). A waste policy may not show up immediately as more patents or breakthrough technologies. It may instead improve resource allocation, reduce coordination frictions, create cleaner production scenarios, and strengthen the conversion of existing innovation inputs into green outputs. GIE is therefore a suitable outcome for evaluating whether ZWCP affects the performance of the urban innovation system. It shifts the focus from how much innovation a city produces to how efficiently innovation inputs become green knowledge and economic value under environmental constraints.

These gaps motivate the following theoretical analysis of how ZWCP may affect GIE, when the effect should appear, and through which policy channel it may operate.

2.3. Theoretical analysis and research hypotheses

ZWCP centers on source reduction, sorted collection, and resource recovery. Through public-sector coordination, technology diffusion, and local enforcement, it brings waste governance into routine environmental policy implementation and performance management. The system can shape urban green innovation through five channels. First, tighter reduction and recovery targets create market demand for clean production, recycling technology, and environmental services. Second, denser collection networks and stronger interdepartmental coordination cut organizational frictions and transaction costs. Third, performance assessment and tougher enforcement raise the rate

at which existing innovation inputs translate into outputs. Fourth, the policy opens new green governance scenarios and speeds up the rollout of green technology in production, disposal, and recycling. Fifth, the policy package includes mandatory resource recovery, industrial park upgrading, hazardous waste tracing, and waste stream reorganization. These instruments target the secondary sector and steer investment, energy, and managerial attention toward manufacturing upgrading rather than service expansion (Fig. 1). Together they imply that ZWCP raises urban GIE in the initial stage of implementation. Hence:

H1: ZWCP, as a zero-waste environmental policy, significantly improves GIE in pilot cities in the initial implementation stage.

The effect of ZWCP on GIE need not build up smoothly over time. The launch year usually brings strong administrative mobilization, concentrated fiscal and regulatory attention, fast infrastructure adjustment, intensive interdepartmental coordination, and rapid project implementation. These actions deliver immediate gains in coordination and efficiency. The momentum, however, is hard to sustain. Once later policy renewal weakens, market incentives remain insufficient, and technology diffusion is not institutionalized, the early gains may fade. The effect on GIE is therefore likely to cluster in the launch year rather than accumulate over time. Hence:

H2: The effect of ZWCP on GIE concentrates in the launch year and does not accumulate in later years.

Looking at the two stages separately, ZWCP can affect both E_1 and E_2. It directly reorganizes waste streams, builds recycling infrastructure, and deploys resource recovery facilities. These changes improve recycling efficiency and short-run knowledge

application, which raises E_2. The launch year also brings a wave of science and technology projects, talent recruitment, and green technology demonstrations, which feed E_1. Knowledge-production responds faster to policy support than application efficiency, which usually needs a longer adjustment period. E_1 should therefore rise more than E_2 in the launch year. Hence:

H3: ZWCP improves both knowledge-production efficiency (E_1) and economic-conversion efficiency (E_2) in the launch year, and the gain in E_1 exceeds the gain in E_2.

Beyond innovation, ZWCP also redefines waste itself. Reduction, sorting, and recovery shift waste from a governance burden toward a usable resource input. The pilot should therefore raise the industrial solid waste comprehensive utilization rate (ISWCU) in pilot cities. Hence:

H4: ZWCP significantly improves ISWCU as a direct waste-governance outcome in pilot cities.

Of the five channels, industrial restructuring is the most distinctive. Most ZWCP instruments target manufacturing, not services. Pilot cities are therefore likely to see value added shift from the tertiary sector toward the secondary sector. The shift can lift GIE because policy-driven investment in cleaner production, materials recovery, and emissions abatement raises green productivity in the upgraded secondary sector. Following work on Chinese cities (Tang et al., 2023; Wu et al., 2021), we capture this reallocation with the log ratio of tertiary to secondary value added. Hence:

H5: ZWCP improves GIE partly through policy-induced industrial restructuring, reflected in a relative shift of value added from the tertiary sector toward the secondary sector.

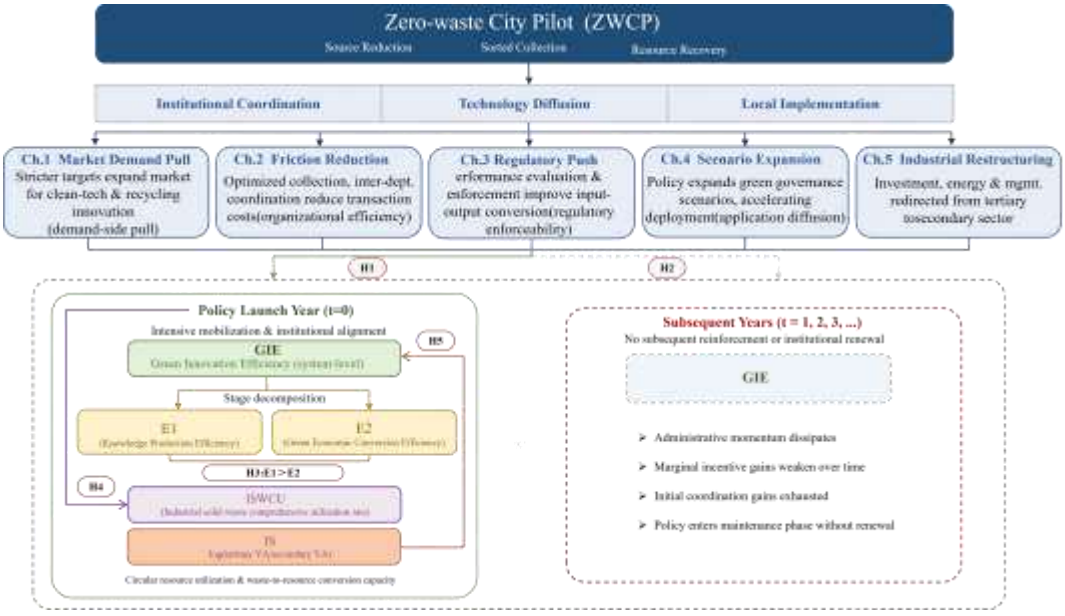


Fig. 1. Research framework and hypothesis logic.

3. Data and methodology

3.1. Sample construction

The sample is a balanced panel of 210 Chinese prefecture-level cities from 2012 to 2023. It contains 11 first-batch pilot cities, 70 cities added in the 2022 expansion, and 129 cities that remain untreated throughout the window (Online Resource 1, Table S1). Fig. 2 shows the spatial distribution of the pilot cities. We keep the 2022 expansion cities in the sample because CSDID requires later-treated cities to be coded explicitly. With this coding, only cities that are not-yet-treated serve as controls in each period.

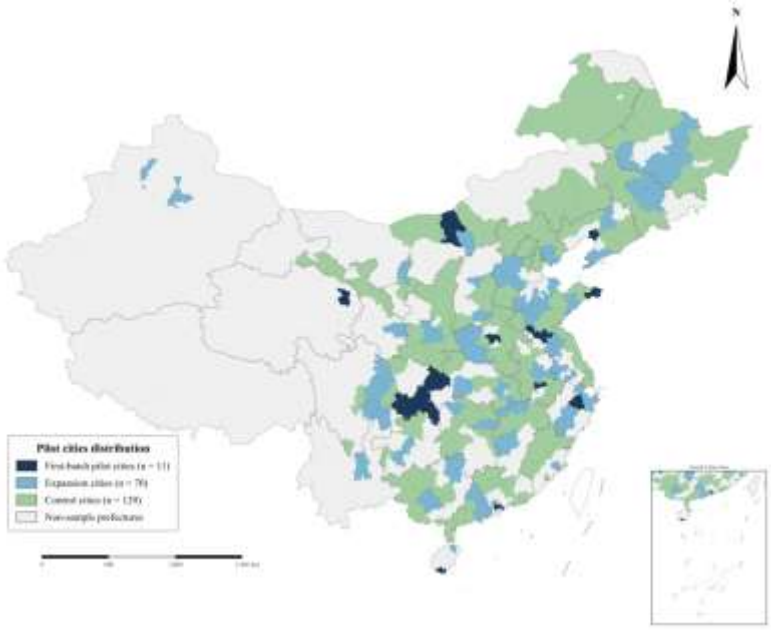


Fig. 2. Spatial distribution of ZWCP pilot cities across 210 Chinese prefecture-level cities.

Data come from the Ministry of Ecology and Environment, the China City Statistical Yearbook, the China Environmental Statistical Yearbook, and the EPS, CEIC, and CSMAR databases. We start from about 290 prefecture-level cities and drop those with major boundary changes, more than three consecutive years of missing key variables, or unresolved matching across databases. The cleaned sample is balanced. We convert GDP to constant 2012 prices, express energy consumption in 10,000 tons of standard coal equivalent, and measure industrial solid waste generation and utilization under the Ministry of Ecology and Environment's unified reporting rules.

3.2. Measurement of GIE

We measure GIE with a two-stage SBM-DEA model that allows undesirable outputs under variable returns to scale (VRS) (Tang et al., 2020). The first stage E_1 captures how efficiently innovation inputs become green knowledge. Inputs are science expenditure and employment in scientific research and technical services; the desirable output is the

total number of green patent applications. The second stage E_2 captures how efficiently green knowledge and resource inputs become economic output under environmental constraints. Inputs are total energy consumption and green patent applications; the desirable output is GDP; the undesirable outputs are industrial solid waste generation and industrial SO_2 emissions. Green patents link the two stages: they are the output of E_1 and an input of E_2. The standard SBM-DEA formulation for each stage is:

$$\rho^{*}=min\frac{1-\frac{1}{m}\sum_{i}\frac{s_i^{-}}{x_{i0}}}{1+\frac{1}{s_g+s_b}\left(\sum_{r}\frac{s_r^{g}}{y_{r0}^{g}}+\sum_{l}\frac{s_l^{b}}{b_{l0}}\right)} \tag{1}$$

$$s.t.\quad x_0=X\lambda+s^{-},\ y_0=Y\lambda-s^{g},\ b_0=B\lambda+s^{b}, \tag{2}$$

$$\lambda\geq 0,\ s^{-}\geq 0,\ s^{g}\geq 0,\ s^{b}\geq 0,\ 1'\lambda=1. \tag{3}$$

Where $\rho^{*}\in(0,1]$ denotes the efficiency score, s^{-}, s^{g}, s^{b} denote input excess, desirable output shortfall, and undesirable output excess, respectively, λ is the intensity vector, $1'\lambda=1$ represents the variable returns to scale constraint. When $\rho^{*}=1$, the decision-making unit is on the efficiency frontier under the VRS constraint. Compared with traditional radial DEA, SBM explicitly handles slack variables and is therefore better suited to GIE measurement with undesirable outputs (Tone, 2001).

We then aggregate the two-stage scores into a composite GIE index using equal weights:

$$GIE_{i,t}=0.5\,E_1+0.5\,E_2 \tag{4}$$

We use the natural log of GIE in the baseline regressions. The log transform preserves the ranking of observations, reduces heteroskedasticity, and reduces the influence of observations near the efficiency frontier.

GIE in this study is broader than R&D efficiency. It covers both green knowledge production and the environmentally constrained conversion of that knowledge into economic output. We keep industrial solid waste generation as an undesirable output in E_2 because it captures a key environmental constraint in urban production. The version without solid waste is used only as a sensitivity test.

We estimate year-specific frontiers so that the production frontier can shift over time. This choice is important for a 2012-2023 city panel because a pooled frontier would implicitly compare early observations with later technological possibilities and could mechanically penalize earlier years. E_1 and E_2 receive equal weights because they represent two equally important stages of the urban green innovation system. Green patent applications are identified from CNIPA records using the WIPO IPC Green Inventory, and applications are preferred to grants because they capture innovation activity more promptly and avoid grant-delay bias. Appendix A provides further details and alternative GIE constructions.

The two-stage structure also has an environmental policy interpretation. E1 indicates whether innovation inputs are transformed efficiently into green knowledge, whereas E2 indicates whether that knowledge is converted into economic output under environmental constraints. For policy evaluation, an E1 bottleneck points to R&D funding, talent, and knowledge-production capacity, while an E2 bottleneck points to technology adoption, cleaner production, resource recovery, and market conversion. The DEA decomposition therefore turns GIE from a single performance score into a diagnostic tool for evaluating where environmental policy effects arise.

3.3. Empirical strategy

For the 11 first-batch cities, 2020 is the treatment year and 2019 is the announcement year. The 2022 expansion cities are coded as later treated, and cities that have not entered the pilot by 2023 are coded as never-treated. Because treatment starts at different times and effects can vary across cohorts and event times, we use the Callaway and Sant'Anna (2021) CSDID estimator as the main design for evaluating this staggered environmental policy intervention.

$$ATT(g,t)=E\left[Y_{i,t}(g)-Y_{i,t}(0)\middle|G_i=g\right] \tag{5}$$

$$ATT^{DRIPW}(g,t)=E\left[\left(w_g(X_i)-w_C(X_i)\right)\left(\Delta Y_{i,t,g\text{-}1}-m_{g,t}(X_i)\right)\right] \tag{6}$$

$$ATT^{o}=\sum_g\sum_{t\geq g} w(g,t)\times ATT(g,t) \tag{7}$$

where i denotes the city, t denotes the year, $G_i=g$ denotes city i's first treatment year, $Y_{i,t}(g)$ and $Y_{i,t}(0)$ denote the potential outcomes of city under first treatment at year g and never-treated scenarios, respectively, $ATT(g,t)$ is the group–time average treatment effect for cohort g at period t. Eq. (6) is the DRIPW estimator adopted in this study, where $\Delta Y_{i,t,g\text{-}1} = Y_{i,t}-Y_{i,g\text{-}1}$, $m_{g,t}(X_i)$ is the untreated outcome change of the control group, conditional on covariates, from g-1 to t, $w_g(X_i)$ and $w_C(X_i)$ are standardized inverse probability weights constructed from propensity scores, and Eq. (7) gives the weighted aggregation form of the overall average treatment effect.

The key identifying assumption is conditional parallel trends: conditional on baseline covariates, the untreated outcome path of each treated cohort would track the path of the control group. We measure all covariates at the 2019 baseline and interact 2019 POP and AGDP with the 2020 and 2021 year dummies, which lets pandemic

recovery paths differ across cities. Online Resource 1, Table S2 reports covariate balance and propensity score overlap.

The first batch contains only 11 cities and shows visible baseline imbalance, so we rely on the doubly robust structure of DRIPW to tighten inference in a small-sample setting. Section 4.4 probes this further with leave-one-out tests, 1,000 random permutations, and alternative control group definitions.

The control group in the main specification consists of cities that are not-yet-treated. For robustness we also use never-treated cities as the control group, and we report TWFE-DID results from a clean sample that drops the 2022 expansion cities. The clean sample avoids control-group contamination. We aggregate the group-specific effects into calendar-time, event-time, and overall average effects with weights based on cohort size. Inference relies on a multiplier bootstrap.

3.4. Variables

We select control variables that capture urban economic scale, development level, openness, financial resources, human capital, education investment, and information infrastructure: log population density (PD), log per-capita GDP (AGDP), FDI/GDP ratio (FDI), financial depth (FI), higher-education enrollment per capita (HEDU), log education expenditure (EDU), and broadband subscribers (INT) (Du et al., 2021; Hao et al., 2020; Liu and Fan, 2017; Zhang et al., 2021). Table 1 reports the variable definitions.

Table 1. Variable definitions and measurement for the empirical analysis.

Variable types	Variable	Description	Definition

Dependent variable	$\ln(GIE_{i,t})$	Green innovation efficiency	Natural log of the composite GIE index
Control variables	POP	City size	Urban resident population. Used only in the pandemic interaction term
	PD	Population density	Natural log of population per square kilometer
	AGDP	Economic development	Natural log of per capita GDP
	FDI	Openness	Foreign direct investment divided by GDP
	FI	Financial development	End of year deposit and loan balance of financial institutions divided by GDP
	HEDU	Higher education level	Students enrolled in regular higher education institutions divided by total population at the end of the year
	EDU	Education investment	Natural log of education expenditure
	INT	Internet penetration	Broadband internet subscribers divided by resident population
Mechanism variable	ISWCU	Industrial solid waste comprehensive utilization rate	Industrial solid waste comprehensive utilization divided by industrial solid waste generation

EGOV	Government zero-waste attention	Frequency of more than 30 zero waste related keywords in government work reports divided by the total number of words
EEI	Environmental enforcement intensity	Environmental administrative penalty cases divided by resident population at the end of the year
IS	Industrial restructuring	Natural log of the ratio of value added in the tertiary sector to value added in the secondary sector

3.5. Mechanism variables

ISWCU is our primary direct waste-governance outcome and one of four candidate channel variables we examine in Section 5. It is the comprehensive utilization rate of industrial solid waste, and it captures a city's capacity to recover and reuse industrial waste while matching the policy focus on source reduction, collection, and resource recovery. Because E_2 already includes resource use and environmental constraints, ISWCU offers channel evidence rather than a strict causal mediator (VanderWeele, 2016). We choose ISWCU because it has the best time series coverage and the most consistent reporting standard across prefecture-level cities; indicators for other waste streams are much less comparable.

Among the candidate channel variables, IS captures industrial restructuring (the channel whose mediation effect we examine in Section 5.3), while government zero-waste attention (EGOV) and environmental enforcement intensity (EEI) serve as

alternative governance channels (Section 5.2)**.** EGOV captures government attention to zero-waste policy: it is the share of more than 30 zero-waste related keywords in each municipal government work report. EEI captures environmental enforcement intensity: it is the number of environmental administrative penalty cases divided by year-end resident population. Together these variables let us compare the waste utilization channel with policy attention, enforcement, and structural upgrading (Liu et al., 2024; Zheng et al., 2025).

For channel analysis, we use a standard three-equation TWFE framework on a clean sample of the 11 first-batch pilot cities and the 129 never-treated cities, which avoids contamination from later-treated cities. The TWFE framework applies only to the channel analysis; the main policy estimates continue to rely on the CSDID design. Because a fully developed mediation decomposition is not yet standard in multi-period DID settings, we interpret these estimates as complementary channel evidence rather than as a strict CSDID-based causal mediation design.

We evaluate the indirect effect a×b with 1,000 city-clustered bootstrap replications and treat it as significant when the 95% confidence interval excludes zero (Zhao et al., 2010). As a robustness check, we re-estimate the policy effect on IS in the full 210-city sample with the CSDID-DRIPW estimator; the 2022 expansion cohort is coded as later-treated and enters the control group only before treatment.

4. Empirical results

4.1. Policy setting and descriptive evidence

Before turning to the formal estimates, we plot annual mean GIE for the 11 first-batch pilot cities against the 129 never-treated cities (Fig. 3). The two series move

together before 2020, which gives preliminary support for the parallel trend assumption. After 2020, the treated series shows a short-lived rise and then converges back toward the control path, foreshadowing the launch-year pattern in the CSDID estimates.

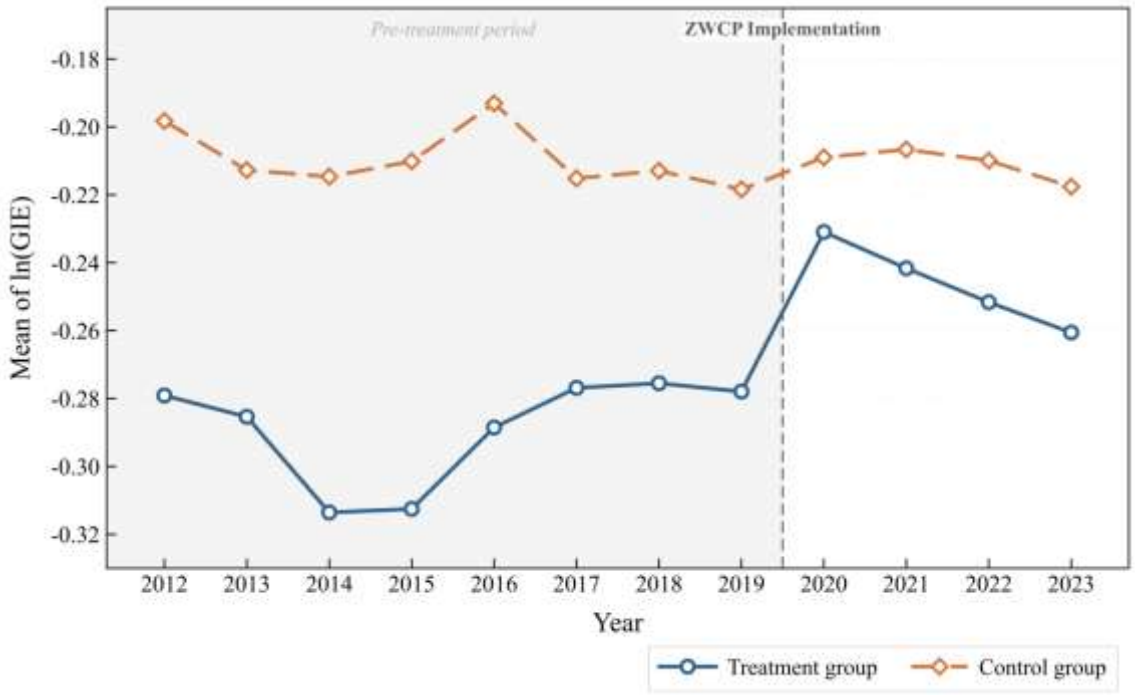


Fig. 3. Annual mean GIE for first-batch treated and never-treated cities from 2012 to 2023.

Online Resource 1, Table S3 shows clear pre-policy differences between the two groups. The pilot cities are larger, richer, and stronger in human capital and internet access, but they have lower baseline GIE. Pilot selection is therefore not random; it tracks governance pressure, city size, and environmental constraints.

4.2. TWFE-DID baseline

Online Resource 1, Table S4 reports TWFE-DID estimates for the sample that drops the 2022 expansion cities. Column 1 has city and year fixed effects only. Column 2 adds time-varying controls. Column 3 further adds interactions between 2019 POP and AGDP

and the 2020 and 2021 indicators. The DID coefficient is positive in all three columns, but it shrinks once controls enter and is never precisely estimated. Pandemic-related heterogeneity therefore matters for identification, which is why we treat TWFE as a benchmark only and rely on CSDID for the main analysis.

4.3. CSDID (DRIPW) main results

Table 2 reports the main CSDID estimates. The 2020 calendar-time ATT is 0.0330 (significant at the 5% level), implying that ZWCP raised GIE in first-batch cities in the launch year. ATTs for 2021 through 2023 are small and insignificant, and the overall average ATT is close to zero. The insignificant overall ATT should not be interpreted as evidence that ZWCP has no relevance for GIE. Instead, it shows that the policy effect is highly time-concentrated: ZWCP creates a measurable launch-year efficiency gain but does not generate a durable post-treatment trajectory over 2021–2023.

The policy effect therefore concentrates in the launch year and fades afterward. We interpret this as a short-run efficiency window rather than a persistent average effect across the full post-treatment window.

The 2022 cohort has only one or two follow-up years in our sample, so it speaks little to longer-run effects. The pattern matches H1 and H2, with the evidence concentrated in the first-batch cohort and the launch-year implementation window.

The result is robust to excluding industrial solid waste generation from E_2; Section 5.1 reports the full sensitivity analysis.

Table 2. CSDID (DRIPW) estimates of ZWCP effects on GIE: calendar-time, event-time, and cohort ATTs.

Year / Event time	ATT	SE	p-value	95% CI
Panel A: Calendar-time ATT				
2020	0.0330**	0.0163	0.043	[0.0010, 0.0649]
2021	0.0062	0.0205	0.761	[−0.0339, 0.0463]
2022	−0.0038	0.0240	0.875	[−0.0507, 0.0432]
2023	0.0016	0.0218	0.942	[−0.0412, 0.0443]
Panel B: Event-study ATT (base period, window)				
-4	−0.0077	0.0079	0.330	[−0.0230, 0.0077]
-3	0.0132	0.0140	0.345	[−0.0142, 0.0407]
-2	0.0094	0.0080	0.242	[−0.0064, 0.0252]
0	0.0052	0.0237	0.826	[−0.0413, 0.0517]
1	0.0069	0.0210	0.741	[−0.0342, 0.0480]
2	−0.0332	0.0234	0.157	[−0.0790, 0.0127]
3	−0.0332	0.0280	0.236	[−0.0882, 0.0217]
Panel C: Overall and Cohort average ATT				
Overall	0.0014	0.0196	0.944	[−0.0371, 0.0399]
Cohort2020	−0.0068	0.0174	0.695	[−0.0408, 0.0272]
Cohort2022	0.0040	0.0249	0.874	[−0.0449, 0.0528]

Note: Standard errors are clustered at the city level and inference uses the multiplier bootstrap. Panel A reports calendar time ATT. Panel B reports event study ATT, where k = 0 is the first treatment year for each cohort. Panel C reports the overall and cohort average ATT. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Fig. 4 reports the event-study estimates. The coefficients before treatment are small and not significant. The joint test of parallel trends gives p = 0.361. We therefore find no evidence against the parallel-trends assumption. The coefficient at k = 0 is positive, and the estimates decline quickly after treatment. The first-batch cohort shows the same pattern when compared with never-treated cities. In that sample, the k = 0 ATT is 0.0333 with p = 0.026. The full-sample event study is less precise because it pools cohorts with different exposure lengths. This difference reflects aggregation, not a contradiction in the results.

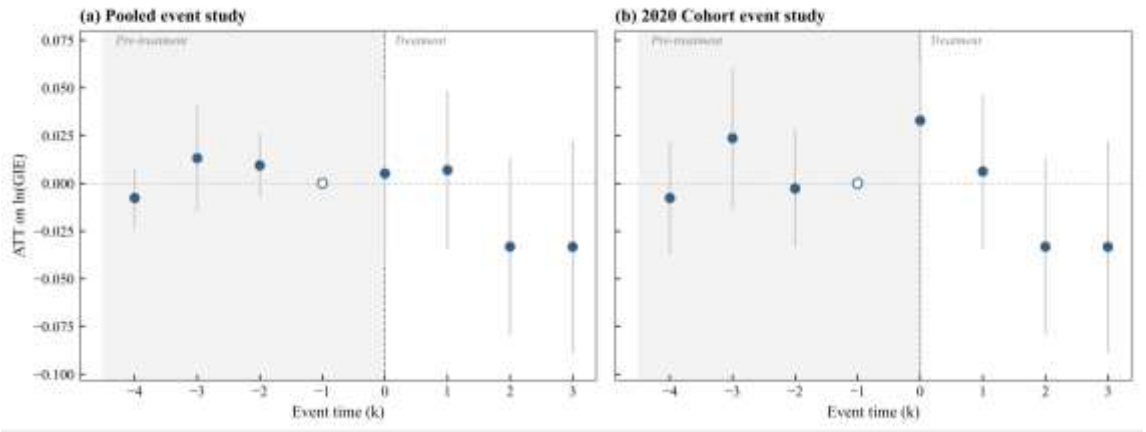


Fig. 4. Event-study estimates of ZWCP on GIE under CSDID (DRIPW).

4.4. Robustness checks

We perform seven robustness checks within the CSDID framework, while the full results are provided in Online Resource 1.

(1) Random permutation test (Fig. 5, left panel). We randomly assign pseudo pilot status across the sample 1,000 times, holding the panel structure fixed. The actual ATT(2020) lies in the far right tail of the resulting placebo distribution, with an empirical

two-sided p-value of 0.023. The baseline estimate is therefore unlikely to be a chance artifact of the data.

(2) Announcement effect test (Fig. 5, right panel). We treat 2019 as a placebo policy year with 2018 as the base period. The estimate is small and insignificant, which supports our choice of 2020 as the first year of policy effect.

(3) Leave-one-out analysis (Online Resource 1, Table S5). We iteratively drop one first-batch pilot city at a time and re-estimate the model on the remaining sample. The point estimates remain positive and quantitatively similar across all such specifications, suggesting that the baseline result is not being driven by any single city within the treatment group.

(4) Control group choice (Online Resource 1, Table S6). We re-estimate the baseline model using never-treated cities as the comparison pool, rather than the not-yet-treated default employed elsewhere. The two sets of estimates are quantitatively close in both magnitude and significance, indicating that our findings are insensitive to this aspect of the research design.

(5) Alternative GIE measures (Online Resource 1, Table S7). We reconstruct GIE using a multiplicative composite, one-period-lagged green patents in E_2, and Super-SBM. The estimates remain positive and close to the baseline in all three cases. Precision varies across measures, with the lagged patent version weakest, but the main result does not hinge on one index.

(6) COVID-related checks (Online Resource 1, Tables S8-S10). Excluding 2020 addresses whether the overall inference is driven by pandemic-year contamination; it does not provide a re-estimate of the 2020 launch-year effect. Additional checks exclude

the largest cities and add richer COVID interaction controls. These tests cannot fully separate policy from pandemic, but they do show that the main result is not driven by pandemic recovery alone.

(7) Concurrent policies and spatial spillovers (Online Resource 1, Tables S11-S13). Controlling for overlapping initiatives such as the Low-Carbon City, Carbon Trading, and Smart City pilots (He et al., 2024; Filiou et al., 2023; Zhang et al., 2022) leaves the 2020 ATT at 0.0340 ($p = 0.034$), close to the baseline. The spatial-spillover specifications, by contrast, are directionally consistent but considerably less precise and statistically insignificant. The evidence thus weighs strongly against policy confounding while offering only limited support for spillover effects.

Taken together, the checks support a timing-based interpretation. ZWCP generates a measurable launch-year gain in GIE, but the gain is not institutionalized into a durable post-treatment path. The null overall ATT should therefore be read as evidence of a short efficiency window rather than as evidence that zero-waste environmental policy is irrelevant to innovation efficiency.

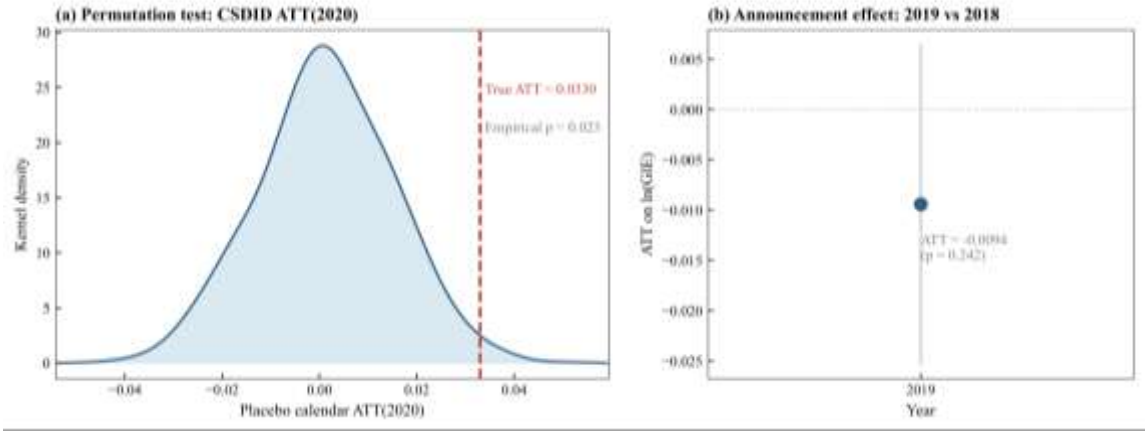


Fig. 5. Random permutation test and announcement-effect placebo for ATT(2020).

4.5. Decomposition of E_1 and E_2

Table 3 reports separate estimates for ln(E_1) and ln(E_2). Both stages rose in 2020: the ATT is 0.0454 for E_1 and 0.0224 for E_2, both significant at the 5% level. The policy therefore raises knowledge-production efficiency and economic-conversion efficiency at the same time in the launch year. The E_1 gain is larger, consistent with a stronger short-run response at the knowledge-production stage. After 2020, the estimates for both stages are small and insignificant. The stage-specific evidence shows that the efficiency window is broad-based across the green innovation system, but it also confirms that the launch-year gains are not sustained. This matches the main results and supports H3.

Table 3. Stage-specific CSDID (DRIPW) estimates for knowledge-production efficiency (E_1) and economic-conversion efficiency (E_2).

Indicator	E_1: ATT	E_1: SE	E_1: p-value	E_2: ATT	E_2: SE	E_2: p-value
Overall ATT	−0.0013	0.0275	0.962	0.0030	0.0142	0.831
Calendar ATT 2020	0.0454**	0.0226	0.044	0.0224**	0.0107	0.036
Calendar ATT 2021	0.0081	0.0282	0.775	0.0050	0.0148	0.736
Calendar ATT 2022	−0.0067	0.0329	0.839	−0.0017	0.0178	0.925
Calendar ATT 2023	−0.0036	0.0311	0.909	0.0048	0.0154	0.755
%Δ (2020) = 100×(exp(ATT)−1)		+4.64%			+2.26%	

Note: Dependent variables are ln(E_1) and ln(E_2). Standard errors are clustered at the city level. ***, **, * denote significance at 1%, 5%, and 10%.

Fig. 6 shows the event-study profiles for E_1 and E_2. Pre-treatment coefficients are small. Both stages jump at k = 0 and decline quickly afterward, with a larger jump in E_1. The launch-year effect is therefore broad-based across the two stages of GIE.

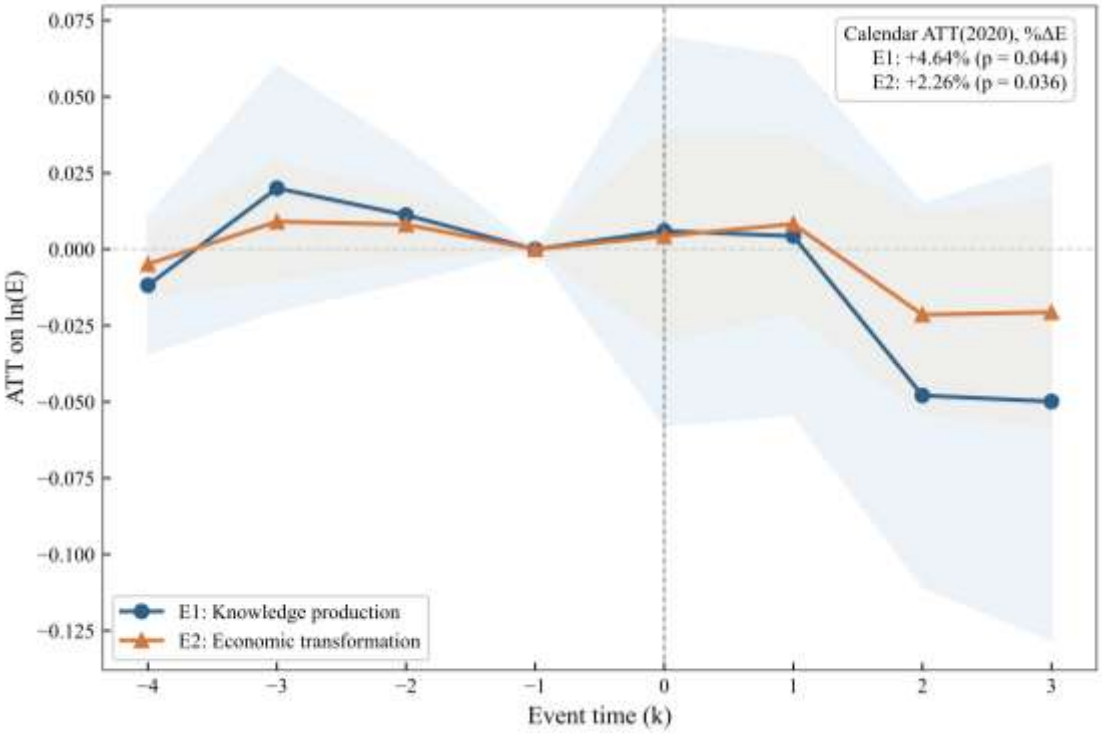


Fig. 6. Event-study estimates by stage (E_1 and E_2) under CSDID (DRIPW).

5. Multiple policy effects and channel analysis

5.1. Policy outcomes: circular resource utilization and the timing of GIE

To test H4, we estimate CSDID with ISWCU as the dependent variable. Table 4 reports the results. ZWCP raises ISWCU by 5.93 percentage points on average (SE = 1.70, p = 0.001, 95% CI [2.59, 9.27]), which is significant at the 1% level and supports H4. The event study for ISWCU follows a different path from GIE (Fig. 7): ISWCU rises from k = 0 to k = +3 and keeps accumulating, whereas GIE jumps in the launch year and then fades. The persistent ISWCU improvement confirms that ZWCP achieved a direct

waste-governance target. However, because this trajectory diverges from the short-lived GIE pattern, ISWCU should not be interpreted as the main mediator of innovation-system efficiency.

Table 4. CSDID (DRIPW) estimate of ZWCP on the industrial solid waste comprehensive utilization rate (ISWCU).

Mechanism variable	ATT	SE	p-value	95% CI	Significance
ISWCU (level, %)	5.9305	1.7047	0.001	[2.5894, 9.2716]	***

Note: Dependent variable is ISWCU. Standard errors are clustered at the city level. *** denotes significance at the 1% level.

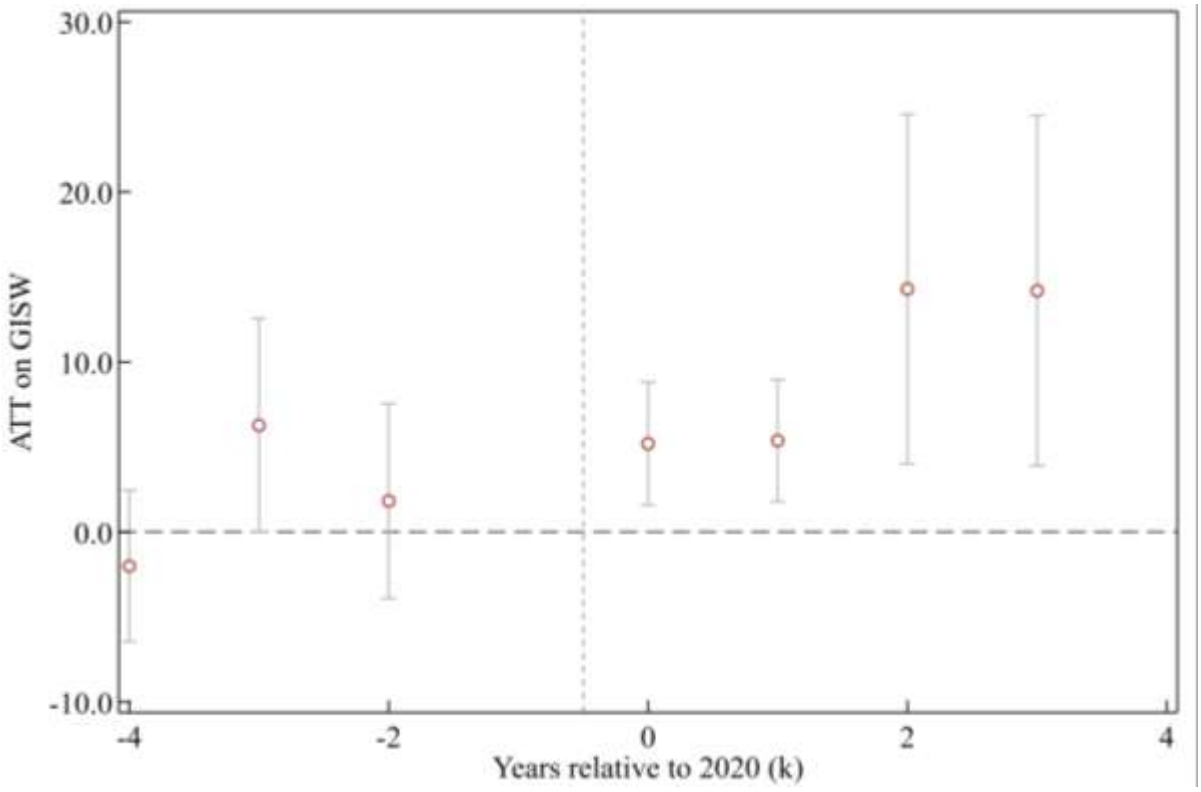


Fig. 7. Event-study estimates of ZWCP on the industrial solid waste comprehensive utilization rate (ISWCU).

We also test whether the solid waste term in E_2 mechanically drives the result. When we drop industrial solid waste generation from E_2, recalculate GIE, and re-estimate the CSDID model, the 2020 calendar-time ATT falls from 0.0330 to 0.0273, and the significance threshold drops from 5% to 10%. The decline is about 17% of the main estimate. The overlap in measurement is therefore real but limited; the main pattern survives. Online Resource 1, Tables S14-S15 and Online Resource 1, Figs. S1-S2 report the waste-excluded GIE results.

The ISWCU result itself is stable. With solid waste excluded, the overall CSDID ATT for ISWCU is 6.35 percentage points ($p = 0.001$), close to the original 5.93. In the clean-sample TWFE mediation test, the a path is significant at the 1% level (coefficient = 19.90, $p < 0.001$) but the b path is not (coefficient = 0.0001, $p = 0.628$), and the indirect effect is small and insignificant ($p = 0.649$). Taken together, these results indicate that ISWCU is a policy-aligned compliance outcome rather than a statistically supported mediator of the GIE effect.

5.2. Alternative channels: government attention and enforcement intensity

Section 5.1 showed that ISWCU improved after ZWCP, but its temporal trajectory did not match that of GIE. We now examine whether the policy works through two other governance channels: government attention to zero-waste issues, and environmental enforcement intensity. We estimate the mediation models in the clean sample using the TWFE three-equation framework, because a standard mediation decomposition is not available under CSDID. The CSDID ATTs for EGOV and EEI are reported as a supplementary check, with full results in Online Resource 1, Table S16.

Neither channel survives the mediation test. For EGOV, the first-step coefficient is 0.0001 (SE = 0.0003) and the second-step coefficient on GIE is −0.5678 (SE = 2.0814); the bootstrap confidence interval for the indirect effect is [−0.0015, 0.0013]. For EEI, the first-step coefficient is 0.0632 (SE = 0.1471) and the second-step coefficient on GIE is 0.0328 (SE = 0.0213); the indirect-effect interval is [−0.0115, 0.0157]. Both intervals include zero.

The supplementary CSDID estimates point the same way: the overall ATT is −0.0001 for EGOV ($p = 0.647$) and −0.0129 for EEI ($p = 0.850$), both insignificant. With the proxy measures available here, we find no evidence that ZWCP improves GIE through policy attention or enforcement intensity. The conclusion may partly reflect measurement noise, but the data do not support either channel within our sample period.

5.3. Industrial restructuring as the main channel

Sections 5.1 and 5.2 found no channel evidence for ISWCU, EGOV, or EEI that matches the launch-year GIE pattern. We now turn to industrial restructuring as the main detectable pathway. IS is measured as the log ratio of tertiary to secondary industry value added (Wu et al., 2021; Zhao and Zhu, 2022). This variable captures whether the pilot reshapes sectoral composition in a way that is relevant to GIE.

Table 5 reports the three-equation mediation results from the clean sample. The estimates come from TWFE OLS with city and year fixed effects, controls, and city-clustered standard errors. Path a is negative and significant: ZWCP reduces IS by 0.1940 log points ($p < 0.01$). Path b is also negative and marginally significant, indicating that a lower tertiary-to-secondary ratio is associated with higher GIE ($b = −0.0495$, $p < 0.10$). The indirect effect $a \times b = 0.0096$ is positive and significant under 1,000 city-clustered

bootstrap replications (95% CI [0.0008, 0.0257]), accounting for 17% of the total effect. The direct effect is statistically insignificant. These results support H5.

The full-sample CSDID-DRIPW estimate supports the same first-step result: the ATT on IS is −0.0474 (p = 0.007, 95% CI [−0.0822, −0.0127]), so the decline in IS is not an artifact of the clean-sample design. The negative a path does not mean that the service sector shrinks in absolute terms; it indicates a short-term relative reallocation toward the secondary sector, where ZWCP instruments such as industrial park upgrading, cleaner production, and resource recovery are more directly deployed. This interpretation connects the statistical channel to environmental policy implementation choices rather than treating industrial restructuring as an automatic market response.

Table 5. Industrial restructuring channel: TWFE three-equation estimates and first-step CSDID estimate.

Variable	(1)	(2)	(3)
Dependent variable	IS	ln(GIE)	ln(GIE)
Path	(a) DID → M	(c) Total effect	(c′, b) With M
DID (ZWCP × Post)	−0.1940***	0.0564	0.0468
	(0.0723)	(0.0486)	(0.0490)
IS			−0.0495*
			(0.0295)
Indirect effect (a × b)			0.0096*
Bootstrap 95% CI			[0.0008, 0.0257]
Proportion mediated			17.0%
CSDID (DRIPW) ATT	−0.0474***		

on IS			
Robustness 95% CI	[−0.0822, −0.0127]		
Control variables	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	1,680	1,680	1,680
Adjusted R^2	0.842	0.745	0.747

Notes: TWFE OLS estimates on the clean sample (11 first-batch pilots and 129 never-treated controls, 140 cities, 1,680 city-year observations). City-clustered standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The indirect-effect confidence interval comes from 1,000 city-clustered bootstrap replications (Zhao et al., 2010). Proportion mediated is (a × b) / c. The CSDID-DRIPW ATT on IS is estimated on the full 210-city sample with the 2022 expansion cohort coded as not-yet-treated controls. Covariates include 2019 baseline city characteristics, COVID-year interactions, and the 2019 baseline level of ln(GIE).

5.4. Heterogeneity analysis

With only 11 cities in the first-batch, we treat the heterogeneity analysis as descriptive and do not interpret the subgroup estimates as fully identified causal effects. We use clean-sample TWFE Spec 3 and split the sample by region and by the 2019 level of INT. Online Resource 1, Table S17 reports the results.

Regional patterns differ. The joint test rejects homogeneous effects across East, Central, and West ($p = 0.011$). The Central region estimate is negative and significant

(−0.0345, $p < 0.01$). The East estimate is positive but insignificant, and the West estimate is also positive but imprecise and should be read with caution.

The informatization split shows a similar contrast. High-INT cities have a negative coefficient (−0.0365, $p < 0.05$), while Low-INT cities have a positive but insignificant one (0.0449), and the difference between the two groups is significant at the 1% level. One reading is that ZWCP works more easily where there is greater room for governance improvement (Qian et al., 2026), but the treated sample is small and these results remain exploratory.

The evidence in Section 5 points to a clear pattern: ZWCP improves ISWCU as a direct waste-governance outcome, does not work through EGOV or EEI within the sample period, and operates on GIE mainly through industrial restructuring. The restructuring channel is the only one with a statistically detectable indirect effect, accounting for about 17% of the total effect on GIE.

This pattern has an additional environmental policy implication. Direct administrative targets can be improved through monitoring, reporting, and compliance pressure, as the ISWCU result shows. Broader system efficiency, however, depends on how the policy reallocates resources across sectors and stages of innovation. The contrast between persistent ISWCU gains and transitory GIE gains therefore suggests that zero-waste environmental policy should not be evaluated only by target compliance. It should also be evaluated by whether temporary coordination effects are converted into stable innovation capacity, especially through technology diffusion and sustained industrial upgrading.

5.5. Why does the efficiency window close?

The closing of the efficiency window can be understood as a difference between administrative mobilization and institutionalized innovation capacity. In the launch year, ZWCP concentrates targets, fiscal attention, infrastructure adjustment, and interdepartmental coordination, which can temporarily reduce allocation frictions and raise the efficiency with which existing innovation inputs are used. After the launch stage, however, these coordination gains may weaken if they are not supported by sustained R&D investment, technology diffusion, market incentives, and industrial application platforms. The persistent rise in ISWCU shows that direct compliance targets can be maintained, but the fading GIE effect shows that compliance does not automatically become durable innovation-system efficiency.

6. Conclusions and policy implications

6.1. Main findings

This study applies the Callaway and Sant'Anna (2021) CSDID estimator with DRIPW to a balanced panel of 210 prefecture-level Chinese cities from 2012 to 2023, treating ZWCP as a staggered urban environmental policy. We ask whether direct waste-governance achievements translate into improvements in GIE, when the effect appears, and through which channel it operates. The main findings are as follows.

First, ZWCP raises GIE in the 11 first-batch cities in 2020, the first year of implementation. The calendar-time ATT is 0.0330 ($p = 0.043$), a relative gain of about 3.35%. The 2020 cohort event study at $k = 0$ is also significant, and the random permutation test gives an empirical $p = 0.023$. The effect does not persist through 2021–2023, and the overall average ATT is close to zero and insignificant. The launch-year

concentration holds across seven robustness checks, although precision varies across specifications.

Second, the E_1/E_2 decomposition shows gains in both stages in 2020. ZWCP raises knowledge-production efficiency by 4.64% and economic-conversion efficiency by 2.26%. The E_1 point estimate is larger, but we do not formally test the difference. Both effects fade quickly after 2020, so the launch-year pattern appears in both dimensions of the green innovation system.

Third, ZWCP raises ISWCU by 5.93 pp and the effect persists over time, but the time path differs from that of GIE. When solid waste is excluded from E_2, the 2020 ATT falls to 0.0273 and becomes only marginally significant, and the TWFE mediation test gives an insignificant indirect effect. We therefore read ISWCU as a direct policy outcome, not as a causal mediator of GIE: improvements in circular resource use and the short-run gain in GIE move in the same direction, but they do not follow the same time pattern.

Fourth, among the four channel variables, only industrial restructuring shows a detectable indirect effect. The pilot lowers the tertiary-to-secondary value-added ratio, and the CSDID robustness test confirms it. The indirect effect accounts for 17% of the total effect on ln(GIE) and the bootstrap confidence interval excludes zero, while the direct effect is not significant. The launch-year gain in GIE therefore works mainly through temporary sectoral reallocation, which helps explain why the policy can lift innovation efficiency even when direct waste-utilization gains do not generate a durable GIE path.

6.2. Theoretical contributions

This study makes three contributions.

First, it brings zero-waste environmental policy into the analysis of urban innovation efficiency. Existing research often treats waste management as an end-of-pipe environmental issue and pays less attention to its link with innovation and industrial upgrading (Nie et al., 2025). Our results show that ZWCP affects GIE mainly through governance-led resource reallocation rather than through direct innovation subsidies, with industrial restructuring as the clearest channel in the data. Environmental governance can therefore shape urban innovation efficiency through policy-induced resource allocation and institutional coordination.

Second, the paper shifts attention from innovation quantity to innovation efficiency. The two-stage framework separates knowledge-production from economic-conversion under environmental constraints, which helps distinguish more innovation activity from more efficient use of existing inputs and provides a more precise basis for evaluating environmental policies.

Third, the paper documents a clear time pattern. The gain in GIE concentrates in the launch year and then fades, suggesting that comprehensive governance policies can open a short efficiency window through administrative mobilization and institutional coordination. These gains may not last unless they are followed by stronger market incentives and wider technology diffusion. The contrast between the persistent ISWCU effect and the short-run GIE effect supports this reading: direct policy targets can keep improving under administrative pressure, while broader system efficiency depends on

deeper structural conditions. Whether the same pattern shows up in other urban environmental policies is an open question.

6.3. Policy implications

First, **environmental policy evaluation systems** should include innovation efficiency, not just innovation output or waste-compliance indicators. Our results show that ZWCP improves efficiency mainly through better allocation and coordination in the launch year, not through immediate growth in green patent output. Evaluation systems should therefore track how effectively cities convert innovation inputs into economic and environmental outcomes, and the two-stage results suggest that knowledge-production efficiency and economic-conversion efficiency should be assessed separately.

Second, implementation should move from administrative mobilization to staged environmental policy support. The launch stage can focus on clear targets, interdepartmental coordination, and rapid infrastructure alignment. The consolidation stage should support R&D, technology adoption, cleaner production, and platforms that connect green patents with industrial applications. The diffusion stage should reward efficiency improvements beyond waste-utilization compliance and translate pilot experience into durable policy instruments.

Third, transition management should account for industrial restructuring and local heterogeneity. Because industrial restructuring is the main detectable channel, local governments should institutionalize the temporary shift toward industrial upgrading through skills training, support for affected service activities, cleaner-production investment, and a clear plan for rebalancing once industrial retrofits are completed. The heterogeneity results are exploratory because the first batch contains only 11 pilot cities,

but they still suggest that differentiated policy implementation may be more effective than a uniform design.

Finally, the lesson for cities outside China is not to copy the exact ZWCP instruments, but to distinguish immediate compliance outcomes from broader system-efficiency outcomes. Waste-utilization rates, facility completion, and enforcement counts can show whether a policy has been implemented, but they do not show whether it has strengthened the innovation system. An environmental policy evaluation should therefore combine operational indicators with system-efficiency indicators and revisit pilots after the launch stage, when administrative mobilization has weakened and market-based incentives become more important.

6.4. Limitations and future research

This study has several limitations.

First, the first batch contains only 11 pilot cities, which limits the precision of subgroup estimates and keeps the heterogeneity results descriptive. A longer post-treatment window for the 2022 cohort will make it possible to test these differences more systematically.

Second, the first year of treatment coincides with the COVID-19 shock. Because E_2 includes GDP, industrial solid waste generation, and SO_2 emissions, part of the measured efficiency change in 2020 may reflect pandemic-related shifts in economic activity rather than pure policy effects. The interaction terms and the drop-2020 check reduce this concern but do not remove it. The 2020 estimate should therefore be read as the launch-year effect observed under pandemic conditions, not as a fully pandemic-free policy effect.

Third, the analysis is at the city level. Future research can use firm-level or industry-level data to examine the micro channels behind the aggregate effects we observe.

Fourth, the long-run effect of ZWCP is still uncertain. As more post-treatment years become available for the 2022 expansion cohort, any difference in their effect path will provide useful evidence on cohort differences in staggered policy settings.

Fifth, the main results come from China's first-batch pilot cities in a specific administrative setting, so external validity is limited. The finding that efficiency gains concentrate in the administrative launch period may still matter for other settings with similar top-down environmental policy structures, but this requires direct empirical testing.

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Data availability

Data associated with this article are available at Mendeley Data (V2), DOI: https://doi.org/10.17632/rpmfmk8fcg.1.

Conflict of interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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