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Can smart city construction enhance urban disaster resilience? A staggered difference-in-differences test based on China's national smart city pilots

Xianzhu Nie

Beijing University of Posts and Telecommunications, Beijing, China

15203501237@163.com

Abstract. Whether digital governance can be converted into measurable disaster-mitigation performance is an important criterion for evaluating smart-city development. Taking China's national smart-city pilot program as a quasi-natural experiment, this study uses a balanced panel of 300 prefecture-level cities from 2009 to 2023 and applies a staggered difference-in-differences design to estimate its effect on urban disaster resilience. The results show that the pilot significantly reduces direct disaster losses as a share of GDP and also lowers disaster fatalities, the share of the population affected, and a composite disaster-damage index. The findings remain robust after accounting for meteorological conditions, region-specific annual shocks, and differential trends associated with pre-policy disaster risk. Event-study estimates, placebo tests, and alternative estimators for staggered treatment timing provide further support for the baseline conclusion. Channel analysis indicates that improved emergency-resource allocation and technological innovation are the clearest pathways, whereas monitoring, early warning, and interdepartmental data sharing are reflected mainly in broader improvements in governance capacity. The evidence suggests that smart-city infrastructure contributes to disaster resilience when digital systems are integrated into emergency procedures, resource coordination, and cross-departmental collaboration.

Keywords: smart city, urban disaster resilience, disaster losses, staggered difference-in-differences, digital governance

1. Introduction

Natural disasters pose a continuous threat to the safe operation of cities. Because population, industry, and infrastructure are highly concentrated, local disasters can spread through transportation, communication, healthcare, and energy networks, and the traditional governance approach that relies on department-by-department reporting and experience-based judgment is therefore prone to problems such as delayed information, unclear resource status, and high coordination costs. Resilience research emphasizes that cities should not only withstand shocks but also identify risks before disasters, organize responses during disasters, and restore key functions after disasters [1-4].

The national smart city pilot provides a policy context for addressing the bottlenecks above. Its core is not simply adding equipment, but incorporating communication networks, public databases, urban facilities, and government procedures into a unified governance system [5, 6]. If information such as road accessibility, hospital capacity, rescue forces, and shelter space can be gathered in a timely manner, it can be translated into faster early warning, dispatch, and coordination. However, duplicated platform construction, inconsistent data standards, and departmental barriers may also leave the technology at the level of being "built" rather than "effectively used".

Existing studies have found that smart cities can improve development quality, environmental governance, innovation and entrepreneurship, and residents' sense of gain [7-10], and digital government may also improve public service performance through transparency and resource allocation [11]. By contrast, whether smart cities can reduce actual disaster-related economic losses, fatalities, and the scope of affected populations still lacks systematic testing based on long-term urban panel data.

This paper treats the three batches of national smart city pilots organized and implemented by the Ministry of Housing and Urban-Rural Development as quasi-natural experiments and constructs a staggered difference-in-differences model using 300 prefecture-level cities and 4,500 city–year observations from 2009 to 2023. The results show that the pilot significantly reduces the disaster economic loss rate, with a decline of about 18.4% of the pre-policy mean of the treatment group; the number of disaster fatalities, the share of the affected population, and the composite disaster damage index also decline simultaneously. The conclusions remain stable after adding extreme temperature, extreme precipitation, annual precipitation, and annual trends for pre-policy disaster-risk groups. The mechanism results provide more support for the two channels of emergency resource allocation and technological innovation, while the formal between-group tests of precipitation and extreme climate exposure find no significant differences.

The contributions of this paper are mainly reflected in three aspects. First, it extends the evaluation of smart city policies to natural disaster governance. Second, it measures resilience using actual outcomes such as economic losses, fatalities, and the affected population, avoiding overlap between composite indices and policy content. Third, it brings resource allocation, monitoring and early warning, technological innovation, and data sharing into the same mechanism framework, and uses modern staggered DID methods to test the reliability of estimates under staggered implementation [12-14].

The "resilience improvement" discussed in this paper does not mean that smart cities can reduce the probability of natural events such as typhoons, earthquakes, or extreme precipitation. Rather, it means that under a given disaster shock, cities control economic losses, casualties, and the scope of affected areas as much as possible through more timely information acquisition, more orderly resource organization, and more effective maintenance of public services. Based on this understanding, pre-disaster, during-disaster, and post-disaster stages constitute the governance process through which the policy takes effect, and the three outcome variables test, from the consequence end, whether these governance capabilities are truly translated into loss-reduction performance.

The empirical identification of this question faces two kinds of difficulties. On the one hand, cities that entered the smart city pilot earlier may have originally had better fiscal conditions, governance capacity, and digital foundations. On the other hand, temperature, precipitation, and disaster exposure in different years can directly affect loss outcomes. In addition to controlling for city and year fixed effects, this paper gradually adds socioeconomic characteristics, time-varying meteorological conditions, regional annual shocks, and differentiated time trends for pre-policy disaster-risk groups, and uses event studies, random permutation, and multiple staggered DID estimators to check whether the conclusions depend on particular model specifications.

2. Literature review

Smart city research has shifted from discussions of concepts and evaluation systems to the identification of policy effects. Early literature emphasized the joint role of information infrastructure, human capital, and governance participation [5, 6, 15, 16]; quasi-experimental studies at the Chinese city level found that the pilot policy can reduce pollution, improve development quality and innovation, and promote entrepreneurship and residents' sense of gain [7-10, 17]. These findings indicate that digital construction has broad external effects, but public safety and natural disaster consequences are still not the mainstream objects of evaluation.

Urban disaster resilience is usually measured through composite indices or actual outcomes. Composite indicators are convenient for presenting economic, social, infrastructure, and governance capabilities, but are affected by indicator selection and weight setting; when the explanatory variable is itself a digital policy, the informatization indicators in the index may also overlap with policy content. Outcome-oriented methods directly observe losses, fatalities, the scope of affected areas, or recovery time, and are closer to the meaning of "smaller losses under the same shock" [2, 3, 18, 19]. Therefore, this paper uses the disaster economic loss rate as the main outcome and cross-validates it with the number of fatalities and the share of the affected population. Related resilience research also shows that innovation, coordination, and structural adjustment often jointly shape the ability of a system to respond to and recover from shocks [20].

The key to how digital technology affects disaster governance lies not only in increasing the speed of information collection, but also in whether it can change organizational action. The Internet of Things, remote sensing, and data analysis can serve risk identification and early warning [21, 22]; cross-departmental information systems can reduce coordination costs, but their performance depends on whether data standards, organizational tasks, and on-site needs match [11, 23]; the application scenarios and government demand generated by smart cities may also promote related technological innovation [9]. Existing discussions mostly remain at the level of technological potential or case studies, and rarely test these channels simultaneously within a unified quasi-experimental framework.

Accordingly, this paper identifies the average policy effect of smart cities from the actual disaster-outcome end, and distinguishes two levels: whether the policy changes the mechanism variables, and whether the mechanism variables are further associated with disaster losses. Considering that the pilots were implemented in batches, in addition to the traditional two-way fixed effects, this paper also adopts modern staggered DID estimators and the Goodman–Bacon decomposition; supplementary methodological results are reported in the Appendix.

Combining smart city policy research with disaster resilience research, it can be seen that there is still an obvious gap between the two. The former literature discusses digital infrastructure, innovation, and public services more, while the latter emphasizes risk exposure, coping capacity, and recovery capacity, but rarely directly answers whether digital governance investment reduces real disaster losses at the city level. Merely proving that platforms, patents, or network coverage have increased cannot automatically imply an improvement in disaster governance performance; only when losses, fatalities, and the scope of affected areas decline simultaneously can it be shown that digital capabilities have produced observable public safety benefits in emergency processes.

Existing studies also often directly connect technical conditions with governance outcomes in mechanism identification, rarely distinguishing between "whether the policy improves a certain capability" and "whether that capability is further associated with disaster consequences". Accordingly, this paper adopts a hierarchical test: it first observes the effects of the pilot on resource allocation, monitoring and early warning, technological innovation, and data sharing indicators, and then examines the associations of these variables after entering the loss equation. Such a treatment cannot replace rigorous causal mediation identification, but it can avoid loosely describing all outcomes of digital construction as established mechanisms.

3. Policy background, theoretical analysis, and research hypotheses

3.1. Policy background

The Ministry of Housing and Urban-Rural Development launched the national smart city pilot in 2012, requiring applicant regions to prepare development plans and implementation schemes, build public information platforms and basic databases, and form smart applications in transportation, healthcare, public safety, drainage and waterlogging prevention, and underground pipelines [24, 25]. This paper assigns treated cities to three cohorts, 2013, 2014, and 2015, according to the year of first inclusion. Cities remain in the treatment state after inclusion, and cities never included over the 40 sample periods serve as the control group. The policy evolution and its links to disaster governance are shown in Table 1. The application arrangements and selection lists of the subsequent two batches of pilots were determined according to the relevant notices issued by the Ministry of Housing and Urban-Rural Development and the Ministry of Science and Technology [26-28].

Table 1. Policy evolution of the national smart city pilot and its links to disaster governance

Time	Policy document or institutional arrangement	Main content related to urban disaster governance
2012	*Notice on Carrying Out the National Smart City Pilot Work and the Pilot Management Measures*	Launched the pilot; required the preparation of plans and implementation schemes, the construction of public information platforms and basic databases, and the establishment of organizational, financial, and institutional safeguards.
2013	National smart city pilot lists announced in batches	Expanded the scope of the pilot; promoted the intellectualization of urban management, public services, and infrastructure, forming differentiated construction scenarios.
2014	*National Plan on New Urbanization (2014–2020)* and annual pilot applications	Incorporated smart cities into the new urbanization deployment, emphasizing the intellectualization of information networks, public services, urban management, and infrastructure operation.
2015	Subsequent annual pilot lists announced and construction continued	Further expanded the scope of the pilot and applications, promoting the aggregation of urban operation information, departmental business coordination, and the implementation of smart applications.

Note: Compiled based on relevant policy documents of the *Ministry of Housing and Urban-Rural Development and the National Plan on New Urbanization (2014–2020)*.

Table 1 shows that the pilot gradually shifted from policy initiation to platform construction, infrastructure intellectualization, and departmental business coordination. The three cohorts of cities first received the policy shock in different years, providing temporal variation for staggered difference-in-differences identification.

3.2. Theoretical analysis and research hypotheses

Smart city construction may run through the entire process of disaster governance (see Figure 1). Before a disaster, multi-source sensing and public databases help identify risks, issue early warnings, and carry out evacuation preparations; during a disaster, platforms can integrate the status of roads, healthcare, rescue forces, and supplies, shortening the time for information verification and resource matching; after a disaster, digital loss assessment and public service monitoring can help determine recovery priorities. If these capabilities are embedded in governance processes, pilot cities should show lower economic losses, fatalities, and shares of affected population.

H1: Smart city construction can significantly improve urban disaster resilience by reducing the economic loss rate from natural disasters, disaster casualties, and the share of the affected population.

In terms of emergency resource allocation, public information platforms improve the visibility of rescue personnel, healthcare, transportation, supplies, and shelter facilities, reducing the costs of searching and verification and enabling resources to flow more quickly to areas with higher demand; public administration and social organization teams can also undertake risk screening, mobilization, and post-disaster coordination [11, 18].

H2: Smart city construction improves urban disaster resilience by improving emergency resource allocation.

In terms of monitoring and early warning, multi-source data can improve anomaly identification and spatial positioning capabilities, while network coverage affects whether risk information can reach residents and grassroots organizations in a timely manner. This paper uses Internet user density to characterize the information transmission base, rather than directly equating it with professional early warning systems [15, 23].

H3: Smart city construction improves urban disaster resilience by enhancing monitoring and early warning capabilities.

In terms of technological innovation, open application scenarios, government procurement, and platform construction generate technological demands in communication, sensing, algorithms, emergency logistics, and facility safety. Technological progress can both improve prediction and response tools and enhance cities' ability to learn and adapt to new risks through knowledge accumulation [9].

H4: Smart city construction improves urban disaster resilience by promoting technological innovation.

In terms of data sharing, unified catalogs, interfaces, and permission rules help meteorological, water conservancy, emergency management, transportation, healthcare, and public utility departments form a common situational assessment, reducing repeated verification and command conflicts. Only when data enters the division of responsibilities and emergency processes can it be translated into coordinated action.

H5: Smart city construction improves urban disaster resilience by promoting cross-departmental data sharing.

The four pathways above do not correspond mechanically to the three stages of pre-disaster, during-disaster, and post-disaster. Monitoring and early warning mainly serve pre-disaster identification and also affect information updating during a disaster; resource allocation is most concentrated during a disaster and also determines whether pre-disaster preparation is sufficient and whether post-disaster recovery is timely; technological innovation and data sharing run through the entire process of risk identification, coordinated response, and experience feedback. Therefore, the theoretical framework emphasizes the complementary relationships among capabilities, rather than a one-to-one mapping of four proxy variables to three stages.

More importantly, digital technology can generate loss-reduction effects only when it is embedded in organizational processes. If a platform lacks unified data standards, clear permissions, and routine maintenance, even if it is connected to large amounts of data, problems such as unverifiable information, difficulty for departments to access data, or failure to feed back on-site needs may arise in emergency situations. Conversely, when data catalogs, business rules, resource ledgers, and emergency plans can be linked with each other, digital infrastructure can be transformed from static construction into dynamic governance capability. Figure 1 summarizes this transmission logic from policy input and governance capability to disaster consequences.

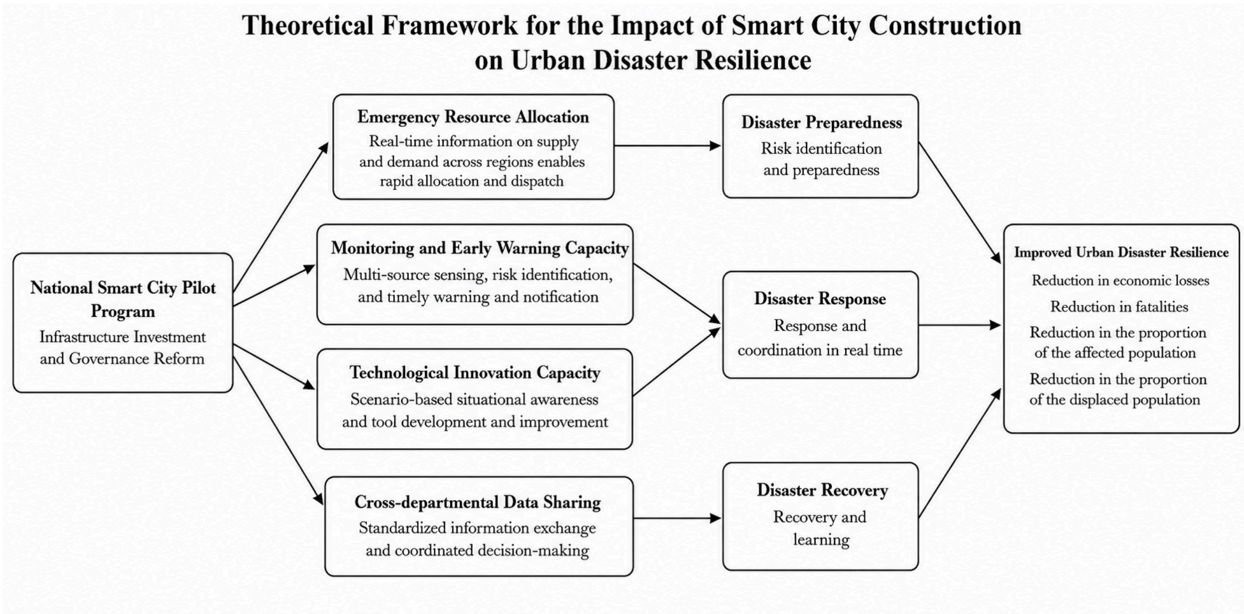

Figure 1. Theoretical analysis framework of smart city construction affecting urban disaster resilience

4. Research design

4.1. Sample, data, and variables

The sample covers 300 prefecture-level cities from 2009 to 2023, with a total of 4,500 observations. In 2013, 2014, and 2015, 94, 92, and 74 cities entered the pilot for the first time, respectively, totaling 260 treated cities. Disaster economic losses, fatalities, and the affected population come from the *China Civil Affairs Statistical Yearbook*; urban economic, social, and mechanism variables mainly come from the *China City Statistical Yearbook* (Table 2); the government data openness index comes from the *China Local Government Data Openness Repor*t; annual precipitation, extreme high-temperature days, extreme low-temperature days, and extreme precipitation days come from an existing urban meteorological panel [29-31]. The data are matched by city–year and checked against administrative division changes; after merging, the sample size remains 4,500 with no missing values.

The main outcome loss_gdp is the ratio of direct economic losses from natural disasters to GDP in the current year; the alternative outcomes are ln_death = ln (fatalities + 1) and pop_rate = affected population / year-end resident population. The core explanatory variable did equals 1 in and after the year in which a city first entered the pilot, and is always 0 for cities never treated. The baseline model controls for economic development, fiscal self-sufficiency, population density, urbanization, healthcare resources, transportation infrastructure, financial development, and government intervention; the extended specification further controls for extreme high-temperature days, extreme low-temperature days, extreme precipitation days, and ln (annual precipitation + 1). The mechanism variables are, in order, public administration and social organization employees per 10,000 people, Internet user density, invention patent authorizations per 10,000 people, and the government data openness index.

Table 2. Definitions and data sources of main variables

Variable type	Variable name (symbol)	Definition and calculation method	Unit/form	Data source
Dependent variable	Disaster economic loss rate (loss_gdp)	Direct economic losses from natural disasters in the current year / regional GDP in the current year	Ratio	*China Civil Affairs Statistical Yearbook China City Statistical Yearbook*
Dependent variable	Log of disaster fatalities (ln_death)	ln (disaster fatalities in the current year + 1)	Log value	*China Civil Affairs Statistical Yearbook*
Dependent variable	Share of affected population (pop_rate)	Affected population in the current year / year-end resident population	Ratio	*China Civil Affairs Statistical Yearbook China City Statistical Yearbook*
Dependent variable	Composite disaster damage index (damage_index)	Arithmetic mean of standardized loss_gdp, ln_death, and pop_rate	Index	Calculated from basic data
Core explanatory variable	Smart city pilot (did)	Equals 1 in and after the year of first inclusion in the pilot, and 0 otherwise	0–1 variable	*Pilot list of the Ministry of Housing and Urban-Rural Development*
Control variable	Economic development level (pgdp)	ln (per capita regional GDP)	Log value	*China City Statistical Yearbook*
Control variable	Fiscal self-sufficiency rate (fiscal)	General public budget revenue / general public budget expenditure	Ratio	*China City Statistical Yearbook*
Control variable	Population density (ln_popden)	ln (population per square kilometer)	Log value	*China City Statistical Yearbook*
Control variable	Urbanization rate (urban)	Urban resident population / resident population	Ratio	*China City Statistical Yearbook*
Control variable	Medical resources (bed)	Hospital beds per 10,000 people	Beds/10,000 people	*China City Statistical Yearbook*
Control variable	Transportation infrastructure (road)	Highway mileage / administrative area	km/km²	*China City Statistical Yearbook*
Control variable	Financial development level (finance)	(Year-end deposit balance + year-end loan balance) / GDP	Ratio	*China City Statistical Yearbook*
Control variable	Degree of government intervention (gov_inter)	General public budget expenditure / GDP	Ratio	*China City Statistical Yearbook*
Meteorological control	Extreme temperature, extreme precipitation, and annual precipitation	Extreme high-temperature days, extreme low-temperature days, extreme precipitation days, and ln (annual precipitation + 1)	Days/log value	Urban meteorological panel

Table 2. Continued

Grouping variable	High disaster risk group (high_risk)	Equals 1 if the mean loss_gdp over 2009–2012 is higher than the city-level median	0–1 variable	Calculated from basic data
Grouping variable	Precipitation/extreme climate exposure	Divided by the city-level median based on the mean or standardized index of the pre-policy fixed period	0–1 variable	Calculated from meteorological data
Mechanism variable	Emergency resource allocation (resource)	Public administration and social organization employees per 10,000 people	Persons/10,000 people	*China City Statistical Yearbook*
Mechanism variable	Monitoring and early warning capability (warn)	International Internet users / administrative area	Subscribers/km²	*China City Statistical Yearbook*
Mechanism variable	Technological innovation level (patent)	Invention patent authorizations per 10,000 people	Items/10,000 people	*China City Statistical Yearbook*
Mechanism variable	Cross-departmental data sharing (data_share)	Comprehensive index of local government data openness	Index	*China Local Government Data Openness Report*

Note: All variables cover 2009–2023; continuous variables are not winsorized in the main analysis. The construction of the composite damage index, meteorological controls, and disaster exposure groupings is described in the text.

The composite disaster damage index (damage_index) is the arithmetic mean of loss_gdp, ln_death, and pop_rate after each is standardized over the full sample; a larger value indicates heavier damage and weaker resilience. Pre-policy disaster risk (baseline_risk) is the mean loss_gdp of each city over 2009–2012, and the high-risk group is defined according to the city-level median. Precipitation exposure is defined by the mean pre-policy annual precipitation, and extreme climate exposure is defined by the standardized sum of pre-policy extreme high- and low-temperature days divided at the median; all groupings are fixed at the city level.

The setting of outcome variables follows the principle of "a clear main indicator and complementary alternative indicators". The disaster economic loss rate links the scale of losses to the economic size of a city, facilitating comparison of relative shocks across cities of different sizes; the logarithm of fatalities reflects extreme consequences and life safety; and the share of the affected population characterizes the coverage of disaster impacts. The three correspond to different aspects of property, life, and population exposure and cannot substitute for one another. Therefore, this paper both estimates them separately and constructs a composite damage index to check the overall direction.

Natural geographic features such as whether a city is coastal, whether it is located in a seismic zone, and its topography and geomorphology are basically unchanged over the sample period and have been absorbed as a whole by city fixed effects, so their coefficients cannot be separately identified in the same model. What may truly cause omitted-variable bias are disaster and meteorological shocks that vary by year. To this end, the extended specification adds extreme high temperature, extreme low temperature, extreme precipitation, and annual precipitation, and allows different regions as well as high- and low-disaster-risk cities before the policy to have their own annual change paths. This treatment cannot replace disaster-type-specific loss data, but it can more directly control the time-varying natural conditions faced by cities. Existing studies also show that digital technology may improve the economic recovery capacity of cities after major shocks [32], which provides supplementary grounds for this paper's identification of resilience from actual disaster consequences.

Natural disaster losses are characterized by low frequency, sharp peaks, and long tails, and extreme observations are often the research object itself. The main analysis therefore does not winsorize continuous variables uniformly, so as to avoid mechanically suppressing the true effects of years with major disasters; at the same time, by standardizing by GDP and resident population, adopting logarithmic transformations, and using the composite damage index, it reduces the dominance of city-size differences and abnormal fluctuations in any single indicator over the conclusions. After the newly added meteorological variables are matched with the original panel, 4,500 observations are retained, avoiding changes in sample composition in the extended model due to missing values.

4.2. Model specification

The baseline model is given by Equation (1):

$$Y_{it} = \alpha + \beta DID_{it} + \gamma\prime X_{it} + \mu_i + \lambda_t + \varepsilon_{it} \tag{1}$$

In Equation (1), Y_{it} denotes the disaster outcome of city i in year t, DID_{it} is the smart city pilot variable, X_{it} are control variables, μ_i and λ_t denote city fixed effects and year fixed effects, respectively, and ε_{it} is the random disturbance term. Standard errors are clustered at the city level [33-35]. The extended model further adds time-varying meteorological controls, region × year fixed effects, and pre-policy high-disaster-risk group × year fixed effects. If the core coefficient β is significantly less than 0, it indicates that the pilot reduces disaster losses.

Parallel trends and dynamic effects are examined using the event study model in Equation (2):

$$Y_{it} = \alpha + \sum(k \in K, k \neq -1)\beta_k Event_{it}^k + \gamma\prime X_{it} + \mu_i + \lambda_t + \varepsilon_{it} \tag{2}$$

In Equation (2), the set of relative policy periods is $K = \{-5, -4, -3, -2, 0, 1, 2, 3, 4, 5\}$, and the year before the policy ($k = -1$) is the base period and thus is not separately estimated; the two ends of the window are merged to avoid too few distant-period samples. The mechanism test is divided into two steps: first, Equation (3) is estimated with the mechanism variable as the dependent variable; then, the mechanism variable is included in the disaster loss rate equation to estimate Equation (4). The modern difference-in-differences literature further points out that when treatment timing is staggered and effects may be heterogeneous, alternative estimators and dynamic tests should be combined to judge whether the results depend on the traditional two-way fixed effects specification [36].

$$M_{it} = \alpha + \theta DID_{it} + \gamma' X_{it} + \mu_i + \lambda_t + \nu_{it} \tag{3}$$

$$Y_{it} = \alpha + \beta' DID_{it} + \rho M_{it} + \gamma' X_{it} + \mu_i + \lambda_t + \varepsilon_{it} \tag{4}$$

In Equation (3), M_{it} denotes the corresponding governance capability variable, and θ measures the effect of the pilot on that capability; in Equation (4), ρ characterizes the conditional association between the mechanism variable and the disaster loss rate, and β' is the policy coefficient after the mechanism variable is included. The two steps above can only provide evidence on the channels of action and cannot be used to calculate a strict causal mediation share.

4.3. Descriptive statistics

Table 3 reports the distributions of the main variables. The mean of the disaster economic loss rate is 0.001387, with a standard deviation of 0.001169; the mean of did is 0.582222. Both control variables and mechanism variables exhibit sufficient within-city and within-time variation. After the newly added meteorological data are matched with the original panel, 4,500 valid observations are retained, and the composite damage index and pre-policy disaster exposure groupings are constructed on a unified basis.

Table 3. Descriptive statistics of main variables

Variable	Observations	Mean	Std. Dev.	Min	Max
loss_gdp	4,500	0.001387	0.001169	0.000000	0.012952
ln_death	4,500	1.715635	0.695947	0.000000	3.663562
pop_rate	4,500	0.043326	0.029912	0.000000	0.290574
did	4,500	0.582222	0.493248	0.000000	1.000000
resource	4,500	148.536447	90.978785	20.697786	482.999745
warn	4,500	161.924972	146.139111	11.305934	1,546.355556
patent	4,500	3.776390	1.787685	0.010000	8.014300
data_share	4,500	63.570399	21.483285	4.212359	123.114323
pgdp	4,500	11.185615	0.549152	9.516702	12.790194
fiscal	4,500	0.438967	0.149805	0.057263	0.910946
ln_popden	4,500	5.793712	0.630656	4.369454	7.772585
urban	4,500	0.650361	0.133283	0.276096	0.990000
bed	4,500	45.194981	12.576704	8.000000	86.714481
road	4,500	1.778745	0.626185	0.470358	3.049170
finance	4,500	2.263932	0.547027	0.400000	4.066100
gov_inter	4,500	0.190820	0.050401	0.060000	0.353553

Note: All variables have 4,500 valid observations; variable definitions are shown in Table 2.

5. Baseline results and multidimensional disaster consequences

5.1. Baseline regression

Table 4 gradually adds standard controls, meteorological conditions, and differentiated annual trends on the basis of two-way fixed effects. Without standard controls, the did coefficient is about −0.00029; after adding eight standard controls, it is −0.00030; and after further controlling for extreme high temperature, extreme low temperature, extreme precipitation, and annual precipitation, it is −0.00031. After absorbing region × year fixed effects and pre-policy high-disaster-risk group × year fixed effects, the coefficient is about −0.00035. The direction, magnitude, and significance across columns remain stable, and no abnormal expansion of standard errors is observed.

Table 4. Baseline regression with meteorological and disaster risk controls

Variable	(1)	(2)	(3)	(4)	(5)	(6)
did	−0.00029***	−0.00030***	−0.00031***	−0.00032***	−0.00034***	−0.00035***
	(0.00009)	(0.00009)	(0.00009)	(0.00009)	(0.00009)	(0.00009)
Standard controls	No	Yes	Yes	Yes	Yes	Yes
Meteorological controls	No	No	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Common trend controls	Year FE	Year FE	Year FE	Region × year FE	Year FE + risk × year FE	Region × year FE + risk × year FE

Note: Each column has 4,500 observations, and standard errors are clustered at the city level; the meteorological variables and differentiated trend specifications are described in the text. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Looking at the variation across columns, the core coefficient is gradually adjusted from about −0.00029 to about −0.00035 after different controls are added, but it always remains negative and the standard errors are basically stable. Socioeconomic controls mainly address the differences between pilot and non-pilot cities in development level and public service foundation; meteorological controls correspond to the natural conditions of the year; region × year fixed effects absorb common shocks at the regional level; and high-risk group × year fixed effects allow cities with different pre-policy disaster exposure to have different time paths. Since the various specifications target different sources of confounding, the consistency of the results, more than the significance in any single column, indicates that the estimates are not driven by a particular set of control variables.

It should be noted that controlling for high-risk annual trends does not mean that every specific disaster has been identified. What it does is relax the restriction that cities with high and low initial risk must share the same time trend. If smart city pilots happened to be concentrated in cities where disaster losses were naturally declining faster, the policy coefficient should have shown an obvious attenuation after the group annual trends were added; the current results do not show such a change, providing targeted supplementary support for the baseline conclusion.

5.2. Economic significance and alternative outcomes

The mean pre-policy disaster economic loss rate of the treatment group is 0.00160. The baseline coefficient corresponds to an absolute decline of 0.000295, that is, about 0.0295 percentage points, equivalent to 18.4% of the pre-policy mean. This result is not only statistically significant but also has practical implications for loss reduction.

Table 5 reports multidimensional disaster outcomes under a unified control specification. The coefficients of the pilot on the disaster economic loss rate, ln (fatalities + 1), and the share of the affected population are −0.00031, −0.3511, and −0.01008, respectively, all significant at the 1% level. After the three outcomes are standardized and averaged, the coefficient on the composite disaster damage index is −0.3678, also significantly negative. Since the composite index and the individual outcomes have different dimensions, their coefficients should not be directly compared in magnitude.

Table 5. Multidimensional disaster outcomes and the composite disaster damage index

Variable	(1) Disaster economic loss rate	(2) Log of disaster fatalities	(3) Share of affected population	(4) Composite damage index
did	−0.00031***	−0.3511***	−0.01008***	−0.3678***
	(0.00009)	(0.0373)	(0.00226)	(0.0436)
Standard controls	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes
Region × year fixed effects	Yes	Yes	Yes	Yes
Observations	4,500	4,500	4,500	4,500

Note: The composite disaster damage index is the arithmetic mean of the standardized disaster economic loss rate, logarithm of fatalities, and share of the affected population; a larger value indicates heavier damage and weaker resilience. Robust standard errors clustered at the city level are in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

5.3. Discussion of results

The consistency of the multidimensional results helps rule out the narrow interpretation that "book losses decline but human consequences do not improve". The decline in the economic loss rate may come from infrastructure protection, resource dispatch, or statistical scale effects; the simultaneous decline in fatalities and the share of the affected population shows that the policy impact is not limited to economic accounting. In particular, the significant decline in the logarithm of fatalities suggests that the potential benefits of smart city construction may extend to links directly related to life safety, such as early warning reach, evacuation organization, medical rescue, and on-site coordination.

The role of the composite damage index is to aggregate the three results that are consistent in direction but different in dimension, rather than to replace individual indicators. Its significant decline shows that the conclusion is not driven independently by any single disaster consequence indicator. At the same time, this paper still uses the economic loss rate as the main outcome because its city–year variation is more continuous and more suitable for dynamic effect, placebo, and staggered DID tests; the fatality and affected-population outcomes mainly serve to verify external consistency and practical meaning.

6. Identification and robustness checks

6.1. Raw trends, parallel trends, and dynamic effects

Before turning to the event study, this paper first compares the annual means of the treatment and control groups. Figure 2 shows that, before the launch of the three batches of pilots, the loss rates of the two groups generally moved in the same direction; after the pilots were implemented successively, the decline was more pronounced in the treatment group. Because cities in different batches were at different policy stages in the same calendar year, the raw trends can only provide intuitive evidence, and the formal judgment still relies on estimates relative to policy time.

The three vertical lines in Figure 2 correspond to the launch time points of the three batches of pilots. Because the treatment group contains cities from different batches, in the same calendar year there are both treated cities that have not yet implemented the policy and cities that have already entered the policy period; therefore, changes in group means are affected by cohort composition. The purpose of the raw trend figure is to observe whether the two groups show an obvious long-term divergence and whether an intuitive divergence appears after the policy launch, rather than to directly provide the policy effect.

Furthermore, natural disaster losses themselves have strong annual fluctuations, and a single major disaster in a particular year can raise the group mean. Judging parallel trends solely from the levels of the two curves or the turning point of a certain year is not reliable. The subsequent event study converts each city to relative policy time, takes the year before the policy as the base, and estimates period by period after controlling for city fixed effects and common time shocks, thereby placing the policy dynamics of different batches in the same comparison framework.

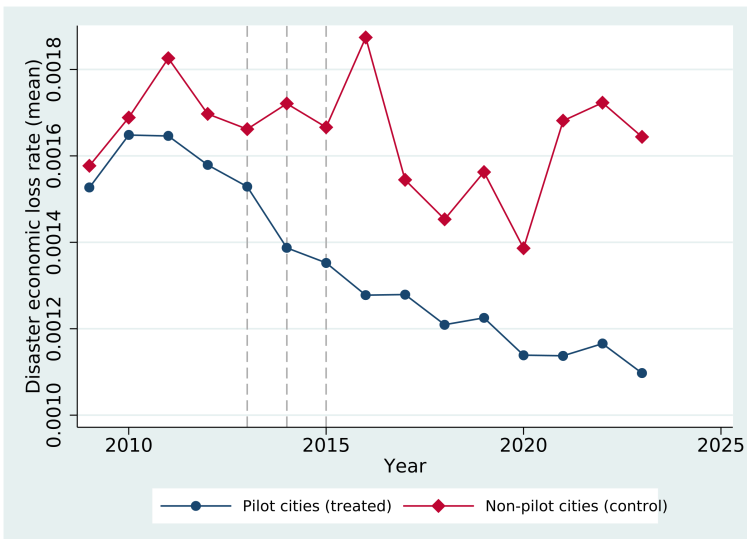

Figure 2. Raw annual trends of disaster economic loss rates in the treatment and control groups

Taking the year before the policy as the base, the coefficients for periods −5 to −2 before the policy are all insignificant, with a joint test $F(4,299) = 1.22$ and $p = 0.30$, and no systematic decline in pilot cities before the policy is found. The coefficient in the current policy period is −0.00027, and the subsequent periods remain negative overall, indicating that the loss-reduction effect appears in the implementation period and persists. Figure 3 reports the period-by-period estimates, and the complete coefficients are shown in Table A1.

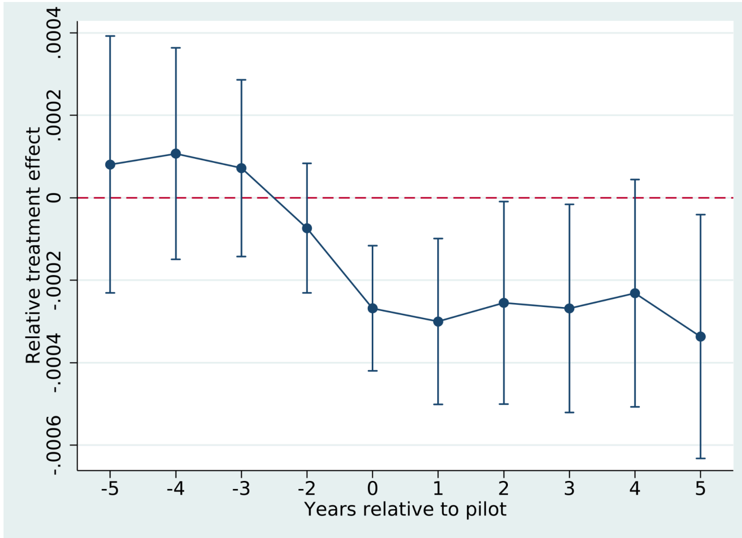

Figure 3. Event study results of the smart city pilot

Note: The year before policy implementation ($t = -1$) is the base period and is normalized to 0, so it is not reported separately in the figure. Error bars are 95% confidence intervals.

To test whether the dynamic conclusions depend on the construction of the economic loss rate, this paper repeats the event study with ln (disaster fatalities + 1). The pre-policy joint test gives $F(4,299) = 1.03$ and $p = 0.39$, and the leading periods remain insignificant; after policy implementation, the fatality coefficients turn negative overall. Figure 4 shows that when disaster fatalities are used as the outcome, the pre-policy coefficients also fluctuate around zero and turn negative overall after policy implementation, with the dynamic direction consistent with the main results.

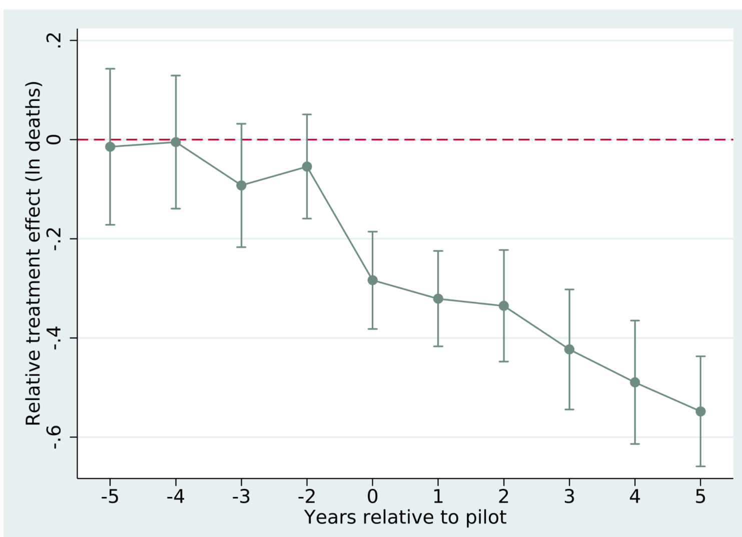

Figure 4. Event study results with disaster fatalities as the outcome

The two sets of event studies together convey two pieces of information. First, the pre-policy coefficients do not show a pattern of continuous decline, reducing the concern that "pilot cities had already entered a loss-reduction trajectory". Second, the policy effect appears in the implementation period and remains negative overall afterward, but the confidence intervals widen as relative time extends. This change is consistent with the reduction in available cohorts and control samples in the distant periods; therefore, this paper pays more attention to the average direction and overall persistence after the policy, rather than overinterpreting any single distant point estimate.

6.2. Placebo tests

This paper performs 1,000 full permutations of the city-level vector of first treatment years and re-estimates while keeping the cohort sizes and the irreversible structure of the policy unchanged. Figure 5 shows that the spurious coefficients are concentrated near zero, the true coefficient lies in the extreme left tail of the empirical distribution, and the two-sided empirical p-value is less than 0.001. When the true policy timing is advanced by 3 years and only pre-policy samples are used, the pseudo-policy coefficient is −0.00004 with p = 0.61, and no anticipation effect is found either. The statistical results of the two tests are reported in Table A2.

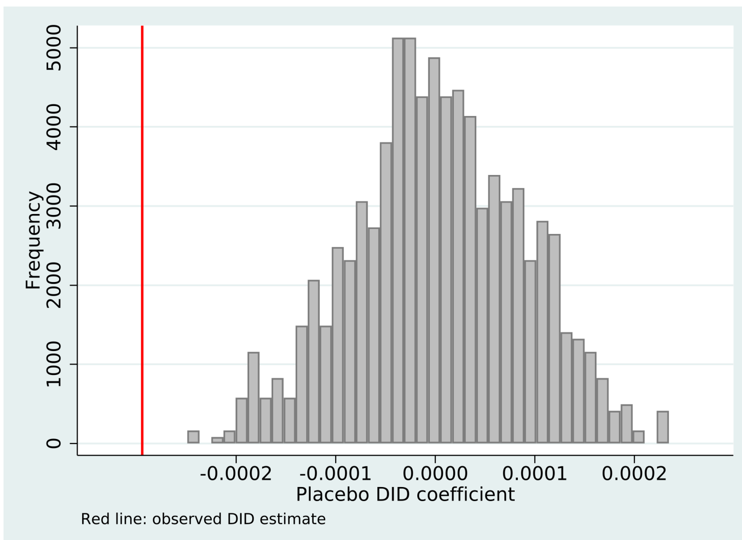

Figure 5. Distribution of placebo coefficients from 1,000 random permutations

The random permutation test retains both the size of each treatment cohort and the structure in which a policy remains in effect once implemented, and is therefore closer to the original policy design than simply randomly generating a treatment dummy variable. The estimates under spurious assignments are concentrated near zero, while the true estimate lies in the left tail of the distribution, indicating that the baseline results are unlikely to be produced by an accidental combination of the pilot list and implementation years. The in-time placebo further rules out the possibility that same-direction changes had already appeared before the formal implementation of the policy.

6.3. Robustness of staggered DID

Given the staggered implementation of the pilots and the possibility that treatment effects vary by cohort and time, Table 6 compares the TWFE, Callaway–Sant'Anna, and Sun–Abraham estimators. The overall effects of the three methods are −0.000295, −0.000310, and −0.000300, respectively, with basically consistent direction, magnitude, and significance, indicating that the baseline conclusion does not depend on any single staggered DID estimator.

Table 6. Comparison of policy effects across different staggered DID estimators

Estimator	Policy effect	Std. error	*p*-value	Control group/aggregation
Two-Way Fixed Effects (TWFE)	−0.000295***	(0.000093)	0.0017	City and year fixed effects
Callaway–Sant'Anna	−0.00031**	(0.00014)	0.025	Never-treated group; Simple ATT
Sun–Abraham	−0.00030**	(0.00014)	0.032	Interaction-weighted; post-policy average

Note: A negative policy effect indicates a decline in the disaster economic loss rate. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

The Callaway–Sant'Anna method uses never-treated cities as the comparison group, first estimates group–time average treatment effects, and then aggregates them. In Figure 6, the pre-policy estimates fluctuate around zero, and most post-policy point estimates are negative; the confidence intervals widen in the distant periods, reflecting the reduction in available comparison samples, but no sustained positive reversal appears.

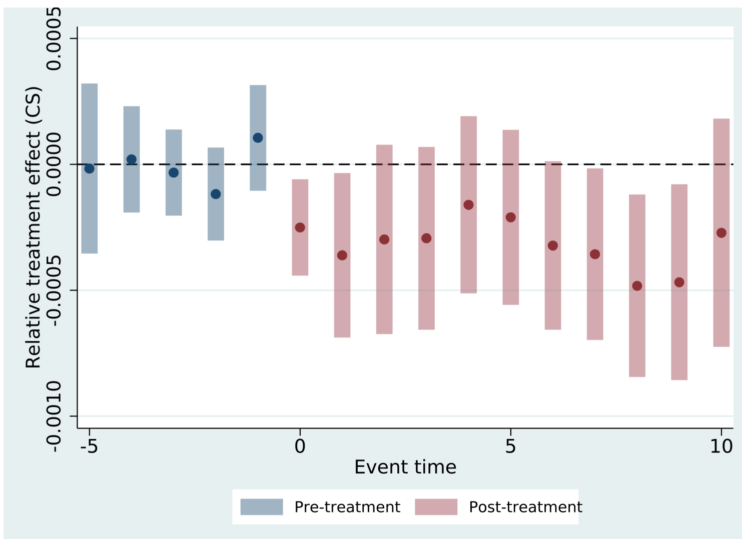

Figure 6 Group–time dynamic ATT of the Callaway–Sant'Anna estimator

The Sun–Abraham interaction-weighted estimator further avoids the contamination between different cohorts and relative periods in traditional event studies. Figure 7 shows that all pre-policy periods are insignificant, the current policy period and most post-policy periods are negative, and the average post-policy effect is −0.00030 (p = 0.032). The complete period-by-period results are shown in Table A3.

The Goodman–Bacon decomposition is used to illustrate the sources of comparison in the traditional TWFE. Table 7 shows that the relatively clean comparisons between the treatment group and the never-treated group and between the early-treated and late-treated groups account for 69.9% in total, while the comparisons between the late-treated and already-treated groups account for 30.1%; the estimated directions of all three types of comparisons are negative, and the weighted result is −0.0002945, almost identical to the baseline TWFE.

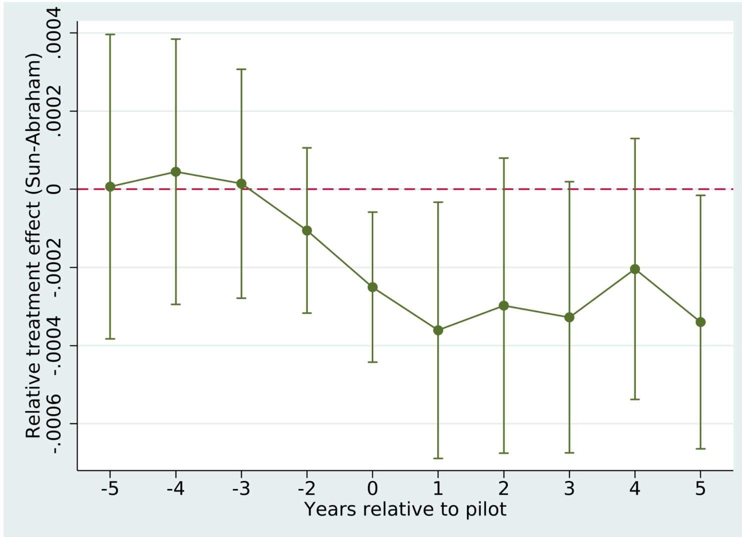

Figure 7. Sun–Abraham interaction-weighted event study

The similar results obtained by modern staggered DID and traditional TWFE do not mean that the staggered-implementation problem can be ignored; rather, they show that, in this sample, the alternative estimators do not find policy effects with opposite directions or vastly different magnitudes. Callaway–Sant'Anna relies on never-treated cities to form group–time comparisons, while Sun–Abraham reorganizes the interaction between event periods and cohorts; the average post-policy effects of both methods are close to −0.00030, providing cross-validation of the baseline estimate under different identification weights.

The Goodman–Bacon decomposition shows that about 30% of the weight comes from comparisons between the late-treated and already-treated groups, and such comparisons may introduce bias when treatment effects change over time. However, the estimated directions of all types of 2 × 2 comparisons in this study are negative, and the weighted result obtained from the decomposition is close to the modern staggered DID estimates. Therefore, this paper uses TWFE as the baseline for ease of presentation while relying mainly on modern methods as the robustness evidence, and does not interpret the decomposition results as an unconditional defense of the traditional model.

Table 7. Summary of the Goodman–Bacon decomposition

Comparison type	Total weight	Weighted average 2 × 2 DID	Nature of comparison
Treatment vs. never-treated	0.561797	−0.000312	Clean comparison
Early-treated vs. late-treated	0.137095	−0.000218	Relatively clean comparison
Late-treated vs. already-treated	0.301107	−0.000297	Forbidden comparison
Total	≈1.000000	−0.0002945	Consistent with TWFE

7. Heterogeneity analysis

This paper conducts exploratory analysis along four dimensions: region, city size, pre-policy precipitation exposure, and pre-policy extreme climate exposure; the results are shown in Table 8. The *p*-values of the formal between-group difference tests for region and city size are 0.13 and 0.14, respectively. Under the precipitation exposure dimension, the policy effects of the low-exposure and high-exposure groups are

−0.00023 and −0.00037, respectively, with an interaction *p*-value of 0.470; under the extreme climate exposure dimension, the effects of the two groups are −0.00039 and −0.00021, respectively, with an interaction *p*-value of 0.326. The estimates for all groups are negative overall, but no systematic change in the effect with the degree of disaster exposure is found.

Therefore, the subgroup results are mainly used to test whether the policy effect is systematically affected by initial disaster exposure; one cannot assert stable heterogeneity merely because a certain subsample is significant. Figure 8 further displays the point estimates and confidence intervals for each subsample.

Table 8. Heterogeneity analysis of the policy effects of the smart city pilot

Heterogeneity dimension	Subsample	DID coefficient	Between-group difference *p*-value
Region	Eastern	−0.00008	
	Central	−0.00018	
	Western	−0.00056***	
	Northeastern	−0.00047*	0.13
City size	Large	−0.00016	
	Small and medium	−0.00043***	0.14
Pre-policy precipitation exposure	Low exposure	−0.00023*	
	High exposure	−0.00037**	0.470
Pre-policy extreme climate exposure	Low exposure	−0.00039***	
	High exposure	−0.00021	0.326

Note: ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels respectively.

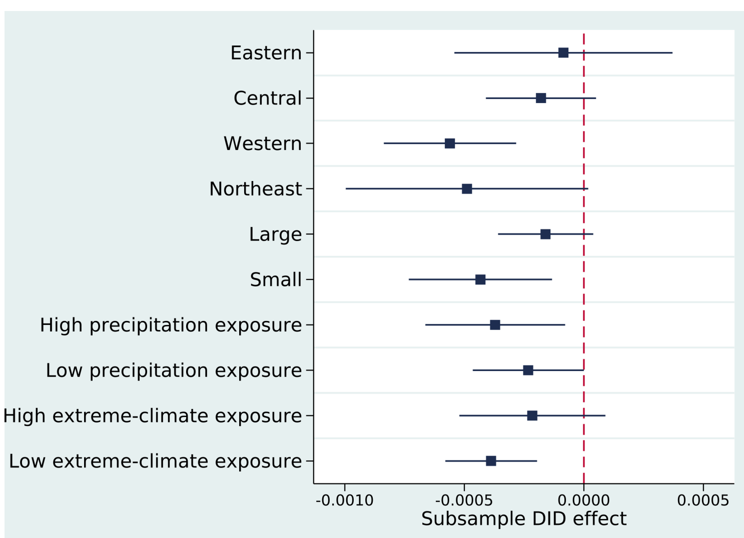

Figure 8. Heterogeneity across regions, city sizes, precipitation exposure, and extreme climate exposure

In the region and city-size subgroups, the absolute values of the point estimates for the western, northeastern, and small and medium-sized cities are relatively large, but the formal between-group tests do not reach conventional significance levels. This means that the differences in point estimates within the sample can be described, but one cannot conclude that the policy is necessarily more effective in a certain type of city. The number of subsample observations, the frequency of disaster events, and the fluctuation of losses all affect the confidence intervals, and simply comparing asterisks can easily misread differences in statistical precision as differences in policy effects.

After grouping by pre-policy precipitation and extreme climate exposure, the interaction terms are also insignificant. This result at least shows that the baseline conclusion is not entirely driven by cities with high precipitation or high extreme temperatures. A more cautious interpretation is that smart cities improve general governance capabilities such as information, resources, and coordination, which may be valuable under different natural exposure conditions; as for whether there are different effects across specific disaster types such as floods, typhoons, and earthquakes, this still requires disaster-type-specific loss data on a unified basis.

8. Channel tests

Table 9 reports the two-step channel tests under the updated control specification, and Figure 9 further presents the standardized comparison of the first-step results. In the first step, the effects of the pilot on emergency resource allocation, monitoring and early warning capability, technological innovation level, and cross-departmental data sharing are all significantly positive. In the second step, the coefficients of emergency resource allocation and technological innovation are about −0.000007 and −0.000046, respectively, and are significant at the 5% level; the coefficients of monitoring and early warning and data sharing are negative but do not reach conventional significance standards. The did coefficient remains significantly negative in all four models.

Table 9. Channel tests of the effects of the smart city pilot on disaster resilience

Test content	Emergency resource allocation	Monitoring and early warning capability	Technological innovation level	Cross-departmental data sharing
Panel A: did	7.4383*** (0.4540)	18.5645** (7.9024)	0.9495*** (0.0371)	11.7023*** (0.3717)
Panel B: did	−0.00027*** (0.00009)	−0.00032*** (0.00009)	−0.00028*** (0.00009)	−0.00030*** (0.00009)
Panel B: Corresponding channel variable	−0.000007** (0.000004)	−0.000000 (0.000001)	−0.000046** (0.000023)	−0.000002 (0.000003)
Standard controls	Yes	Yes	Yes	Yes
Meteorological controls	Yes	Yes	Yes	Yes
Fixed effects	City + region × year	City + region × year	City + region × year	City + region × year
Observations	4,500	4,500	4,500	4,500

Note: Both Panel A and Panel B control for standard control variables, meteorological control variables, city fixed effects, and region × year fixed effects; robust standard errors clustered at the city level are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

To facilitate comparison of channel variables with different dimensions, Figure 9 standardizes the first-step regression coefficients by the sample standard deviation of each variable. All four point estimates are positive, with relatively larger standardized effects for technological innovation and cross-departmental data sharing. This figure only reflects the effects of the pilot on governance capability variables and does not mean that all four channels have produced loss-reduction effects of equal strength; whether the channels are associated with the decline in disaster losses still depends on the results of Panel B in Table 9.

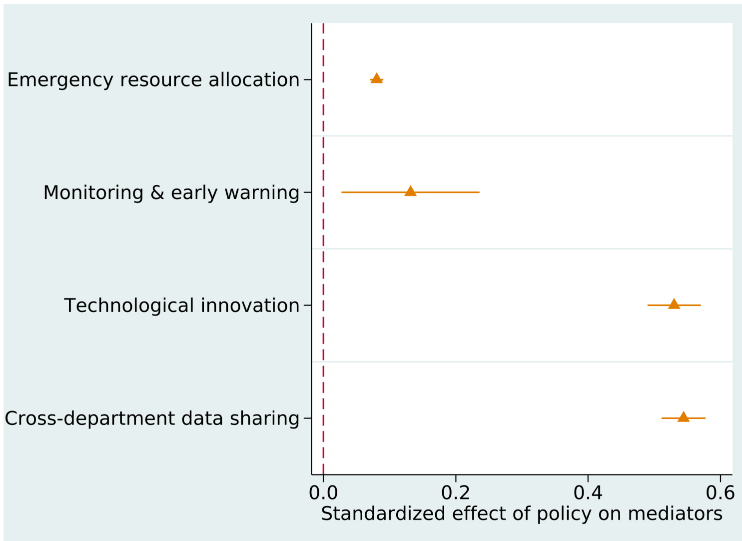

Figure 9. Standardized effects of the smart city pilot on channel variables

The results for emergency resource allocation are fairly consistent with the theoretical expectations. After smart city platforms improve the visibility of resource status, management departments can more easily grasp rescue personnel, healthcare capacity, road accessibility, and material reserves, and adjust the order of deployment as disaster conditions change. After the channel variable enters the loss equation, it is significantly negative, and the absolute value of the did coefficient narrows to some extent, suggesting that resource organization may bear part of the policy effect. However, this variable is still a composite proxy at the city level and cannot be directly equated with the dispatch speed in a specific disaster.

Technological innovation also obtains relatively clear two-step evidence. The smart city pilot creates demand for sensing equipment, communication systems, algorithm applications, and emergency scenarios, and patents and technological accumulation may improve risk identification and response tools. The coefficient of technological innovation is significantly negative, indicating that a higher level of innovation is associated with lower disaster losses. The effect here is not limited to pre-disaster prediction, but also includes during-disaster communication, facility safety, and post-disaster assessment; it is therefore more appropriately understood as an adaptive capability running through different stages.

The evidence for monitoring and early warning and for data sharing needs a more restrained interpretation. The pilot significantly improves the two governance capability indicators, but their independent coefficients after entering the loss equation do not reach conventional significance levels. One possibility is that the existing proxy variables are relatively broad: Internet user density cannot fully capture the accuracy, timeliness, and resident response of professional early warning, and the data openness index can hardly reflect the depth of internal sharing under emergency conditions; another possibility is that the two capabilities must work together with resource dispatch and plan implementation and are difficult to separate in a single-variable model. Therefore, this paper treats them as supplementary evidence of governance capability improvement and does not claim that all four channels have been verified to the same extent.

9. Conclusion and policy implication

9.1. Research conclusion

Based on data from 300 prefecture-level cities over 2009–2023, this paper finds that the national smart city pilot significantly reduces the disaster economic loss rate, with a decline of about 18.4% of the pre-policy mean of the treatment group; the number of disaster fatalities, the share of the affected population, and the composite disaster damage index also decline significantly. After adding meteorological conditions, regional annual shocks, and annual trends for pre-policy disaster-risk groups, the core results remain stable, and parallel trends, placebo tests, and multiple staggered DID estimators also support the baseline conclusions. The channel evidence provides more support for emergency resource allocation and technological innovation, and the formal tests find no significant differences in the policy effect across cities with different precipitation and extreme climate exposure. This conclusion also corroborates domestic quasi-natural-experiment studies on smart city construction improving urban resilience [37].

Taken together, what this paper identifies is not the effect of a certain hardware device or a single platform, but the overall impact of the digital infrastructure, information integration, and governance process adjustment represented by the smart city pilot. The policy coefficient remains consistent across multiple outcomes, control specifications, and staggered DID methods, indicating that digital governance has the potential to be translated into real loss-reduction performance; at the same time, the mechanism results remind us that this translation does not happen automatically, and resource ledgers, technological applications, data rules, and departmental responsibilities need to be continuously maintained in daily governance.

9.2. Policy implication

First, the evaluation of smart cities should further shift from "what has been built" to "what has actually been solved". The number of platforms, the amount of connected equipment, and the scale of data can reflect construction progress, but cannot directly represent disaster-mitigation performance. Outcome indicators such as early warning reach, response time, resource availability, continuous operation of key facilities, and disaster losses can be added to the assessment, and regular drills can be used to test whether the system can actually be used under high-pressure situations.

Second, the emergency resource ledger should be treated as a basic task of digital governance. Data such as rescue personnel, hospital beds, road accessibility, material reserves, and shelters need clear update responsibilities, verification cycles, and access permissions, to avoid the situation in which the platform is rich in data in normal times but fails during a disaster because the information is outdated. Cross-departmental drills should be organized around specific tasks, rather than merely testing whether the platform can log in and display.

Third, the construction of monitoring and early warning should form a complete chain of "identification, issuance, reach, and action". Professional monitoring and risk models are responsible for improving the quality of identification; unified issuance rules reduce information conflicts; multi-channel reach ensures that different groups can receive the information; and grassroots plans and evacuation organization determine whether the warning can be translated into action. General network coverage can only provide transmission conditions and cannot replace professional early warning capability.

Fourth, real governance scenarios should be used to drive emergency technological innovation and data coordination. The government can promote the application of sensing, communication, algorithm, and facility safety technologies in the field through open scenarios, procurement verification, and industry–university–research cooperation, while improving data catalogs, interface standards, and authorization mechanisms. In areas where the evidence for the monitoring and early warning and data sharing channels is relatively limited, priority should be given to improving the professionalism of the indicators and the actual depth of system use, rather than simply expanding the scale of data access.

9.3. Research limitation

The disaster data in the yearbook may have statistical-caliber differences across cities, and the main model does not distinguish specific disaster types such as floods, typhoons, and earthquakes or disaster intensity; the composite damage index is still an outcome-oriented measure and cannot directly separate pre-disaster, during-disaster, and post-disaster capabilities. The mechanism variables are also proxy indicators at the city level and cannot fully reflect the depth of professional early warning, technological application, and departmental coordination. Future research can combine disaster-type-specific losses, remote sensing, insurance claims, high-frequency transportation data, platform logs, and dispatch records to further examine the role of the policy under different disaster scenarios.

In addition, the policy variable in this paper characterizes whether a city entered the national pilot, and cannot distinguish the differences across cities in investment scale, degree of platform integration, and actual intensity of use. The average pilot effect is therefore closer to the overall impact of a package of policies than to the marginal return of a specific digital technology. If subsequent research can obtain project investment, system launch, departmental use, and emergency response logs, it can further identify the implementation differences between "entering the pilot" and "forming governance capability". Furthermore, smart city policies may generate spatial spillovers through regional platforms, knowledge diffusion, and resource flows, which can be tested with spatial DID in the future [38]; the sensitivity to the parallel trends assumption can also be further assessed using methods that allow limited pre-trend deviations [39].

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Appendix

Appendix A. Supplementary robustness figures and tables

Figure A1 compares the DID coefficients under different fixed effects specifications, and Figure A2 reports the standardized policy effects for the three disaster outcomes. The two figures are used to test whether the baseline conclusions depend on a particular model specification or a single outcome variable.

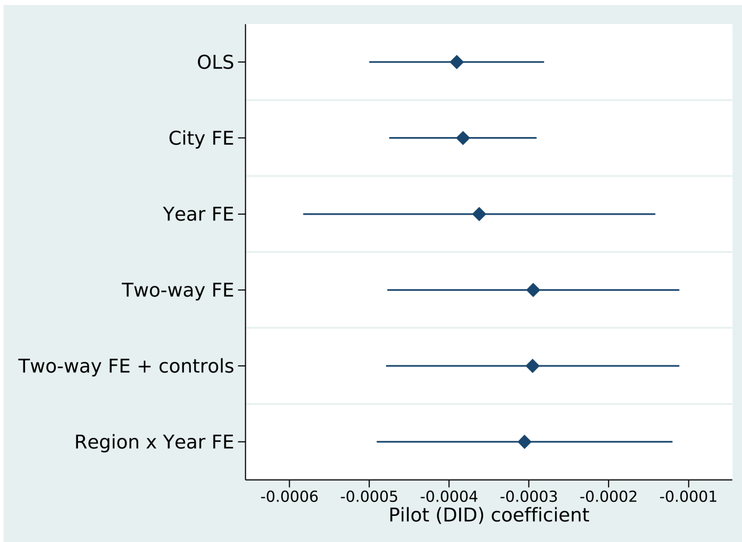

Figure A1. Coefficients of the smart city pilot under different fixed effects specifications

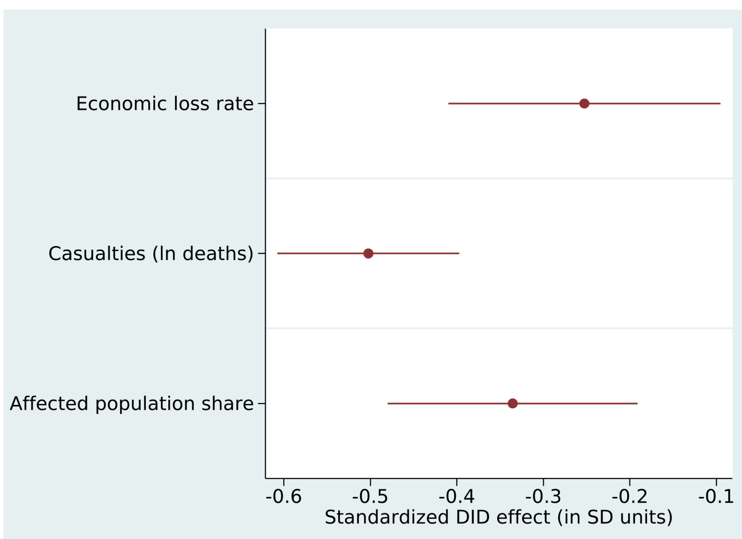

Figure A2. Standardized DID effects across different disaster outcomes

Figures A1 and A2 show that the point estimates under various specifications are negative, and the confidence intervals are consistent with the judgments in the regression tables in the main text.

Table A1. Dynamic treatment effects of the smart city pilot (base period: −1)

Relative policy period	Coefficient	Std. error	Significance
≤ −5	0.00008	(0.00016)	
−4	0.00011	(0.00013)	
−3	0.00007	(0.00011)	
−2	−0.00007	(0.00008)	
0 (current period)	−0.00027	(0.00008)	***
+1	−0.00030	(0.00010)	***
+2	−0.00025	(0.00012)	**
+3	−0.00027	(0.00013)	**
+4	−0.00023	(0.00014)	*
≥ +5	−0.00034	(0.00015)	**
Pre-policy joint test	$F(4,299) = 1.22$	$p = 0.30$	

Note: ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels respectively.

Table A2. Placebo test results

Test method	Estimate/statistic	Std. error	*p*-value	Conclusion
Random permutation (1,000 times)	True coefficient −0.000295; exceedances 0	—	< 0.001	True estimate lies in the extreme tail of the empirical distribution
In-time placebo (advanced 3 years)	−0.00004	(0.00009)	0.61	Spurious treatment effect insignificant

Table A3. Sun–Abraham interaction-weighted event study results

Relative policy period	IW treatment effect	Std. error	*p*-value
≤ −5	0.000007	(0.000199)	0.9730
−4	0.000045	(0.000173)	0.7963
−3	0.000014	(0.000149)	0.9241
−2	−0.000106	(0.000108)	0.3276
0 (current period)	−0.000250**	(0.000098)	0.0107
+1	−0.000361**	(0.000167)	0.0311
+2	−0.000298	(0.000193)	0.1222
+3	−0.000328*	(0.000177)	0.0642
+4	−0.000204	(0.000170)	0.2304
≥ +5	−0.000340**	(0.000166)	0.0401
Post-policy average	−0.00030**	(0.00014)	0.032

Note: ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels respectively.

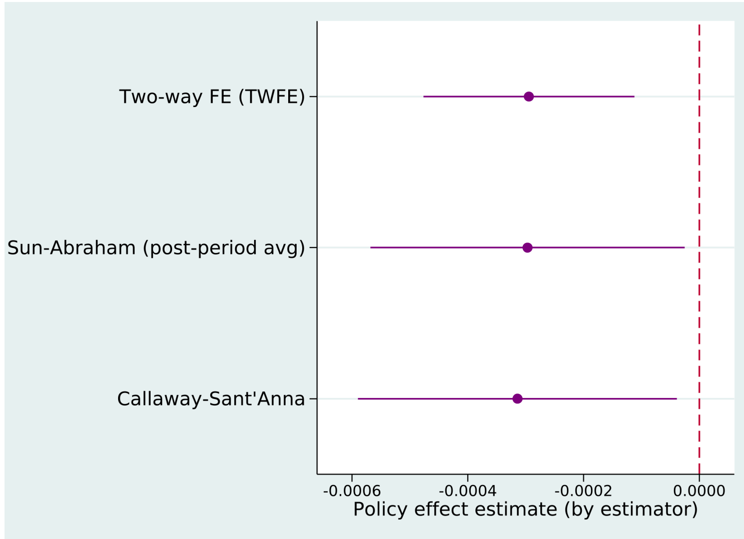

Figure A3. Comparison of policy effects across different staggered DID estimators

Figure A3 visually compares the TWFE, Callaway–Sant'Anna, and Sun–Abraham estimates; the point estimates of the three methods are close and consistent in direction.

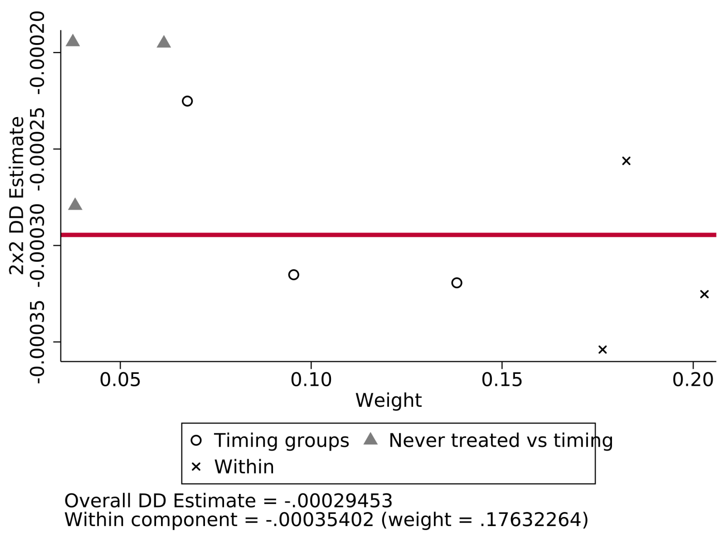

Figure A4. Goodman–Bacon 2 × 2 comparison decomposition

Figure A4 shows the weights and estimates of the various 2 × 2 comparisons in the Goodman–Bacon decomposition, and the horizontal line in the figure is the overall DID estimate.

Appendix B. Complete results of channel tests

Table B1. Effects of the smart city pilot on mechanism variables

Variable	Emergency resource allocation	Monitoring and early warning capability	Technological innovation level	Cross-departmental data sharing
DID	7.4383***	18.5645**	0.9495***	11.7023***
	(0.4540)	(7.9024)	(0.0371)	(0.3717)
Standard controls	Yes	Yes	Yes	Yes
Meteorological controls	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes
Region × year fixed effects	Yes	Yes	Yes	Yes
Observations	4,500	4,500	4,500	4,500

Note: ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels respectively.

Table B2. Policy effects after including mechanism variables

Variable	Adding emergency resources	Adding monitoring and early warning	Adding technological innovation	Adding data sharing
DID	−0.00027***	−0.00032***	−0.00028***	−0.00030***
	(0.00009)	(0.00009)	(0.00009)	(0.00009)
Corresponding channel variable	−0.000007**	−0.000000	−0.000046**	−0.000002
	(0.000004)	(0.000001)	(0.000023)	(0.000003)
Standard controls	Yes	Yes	Yes	Yes
Meteorological controls	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes
Region × year fixed effects	Yes	Yes	Yes	Yes
Observations	4,500	4,500	4,500	4,500

Note: ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels respectively.