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Digital–Green Policy Synergy and Urban Energy Intensity: Evidence from Chinese Cities

Mingjian Xie [1,†], Yang Guo [2,†], Zhibin Tao [3,*,†], Shenjian Li [3], Yu Zhou [4] and Donghao Duan [5]

[1] Faculty of Humanities and Social Sciences, Macao Polytechnic University, Macao 999078, China; p2512881@mpu.edu.mo
[2] School of Economics and Management, Harbin Engineering University, Harbin 150001, China; 13861396887@hrbeu.edu.cn
[3] Department of Accountancy and Finance, University of Otago, Dunedin 9054, New Zealand; shenjian.li@postgrad.otago.ac.nz
[4] School of Public Finance and Taxation, Zhongnan University of Economics and Law, Wuhan 430073, China; zhouyuu@stu.zuel.edu.cn
[5] School of Economics and Trade, Hunan University, Changsha 410079, China; dhduan@hnu.edu.cn
* Correspondence: taozhibin7@outlook.com
† These authors contributed equally to this work.

Abstract

Digitalization can either reduce energy use or generate rebound effects, depending on how digital development is coordinated with environmental and energy objectives. This study examines the relationship between digital–green policy synergy and urban energy intensity using a balanced panel of 282 Chinese cities from 2007 to 2022. We construct a nonlinear digital–green policy synergy index (QSI) from policy attention measures and estimate city and year fixed effects models with complementary robustness and inference procedures. A one-unit increase in QSI is associated with a 0.0537-log-point, or approximately 5.2%, decline in energy intensity. The association is stronger in cities with higher energy intensity, lower initial spatial inequality, and stronger initial digital policy capacity. An accounting decomposition indicates that the estimated relationship is primarily associated with lower values of the archived energy use measure rather than GDP expansion. QSI is also associated with lower nighttime light spatial inequality. The findings therefore identify a robust policy-relevant conditional relationship, rather than a definitive causal effect, and highlight the need to align digital initiatives with energy and environmental objectives.

Keywords: digital–green policy synergy; energy intensity; nonlinear governance index; government attention; Chinese cities

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1. Introduction

Digitalization is increasingly embedded in the infrastructure, production networks, and public administration systems that shape urban energy demand. Digital investment can reduce energy use through monitoring, optimization, and improved allocation, but it can also increase demand through digital infrastructure, data processing, and scale expansion. This ambiguity makes the policy alignment between digital development, energy management, and environmental governance more consequential than digitalization considered in isolation. Recent city-level evidence documents both energy-saving channels and energy demand rebounds, depending on infrastructure, industrial structure, environmental regulation, and local implementation capacity [1–6]. Official assessments likewise emphasize the simultaneous importance of digital transformation and environmentally

sustainable energy systems [7–9]. The policy question is how local priorities direct digital capacity toward energy-saving uses while containing energy-intensive expansion.

A growing body of literature links digital development to energy efficiency, energy intensity, carbon performance, and green productivity, but the estimated direction and magnitude vary substantially across settings and empirical designs. Recent studies generally find that digital development can improve resource allocation, technology adoption, industrial upgrading, or carbon efficiency, but the magnitude and even the sign of the effect vary across specifications and locations [10–20]. This mixed evidence suggests that digitalization should be studied as an energy policy instrument whose consequences depend on governance objectives, implementation capacity, and local economic conditions.

This study focuses on whether the alignment of digital policy and green governance priorities is associated with urban energy intensity. Local governments allocate limited administrative attention across growth, digitalization, environmental protection, and energy security objectives. Digital policy priorities may complement green governance when digital systems support industrial energy monitoring, environmental enforcement, and resource allocation. They may instead compete with these objectives when investment attraction, infrastructure expansion, or output targets dominate the local agenda. This perspective shifts the empirical focus from the amount of digital development to the alignment of policy priorities governing its use [21–30].

To characterize this alignment, we construct a nonlinear digital–green policy synergy index (QSI) from city-level policy attention measures and a context-sensitive alignment term. The index combines city digital policy intensity and green development attention with province-level digital attention, city energy attention, and a phase term that represents whether multiple policy objectives move in a reinforcing or offsetting configuration. The index is a measurement device rather than a claim that local governance operates as a physical quantum system; its value depends on transparent construction, construct validity, and empirical performance [31–33].

This study makes three contributions. First, it shifts the analysis from digitalization in isolation to the alignment of digital and green policy priorities in urban energy governance. Second, it develops a reconstructible nonlinear index of policy attention and evaluates whether it contains information beyond the additive levels of its constituent policy attention measures and a conventional joint attention benchmark. Third, it distinguishes the robustness of the estimated levels association from causal identification and decomposes the energy intensity result into associations with the archived energy use series and GDP [34–38].

The preferred fixed effects coefficient is -0.0537. Province-by-year effects and alternative inference support a negative conditional association, while city trends, first differences, and lead placebos limit causal interpretation. We therefore treat the result as an association that requires independent measurement and identification.

2. Literature Review and Conceptual Framework

2.1. Digitalization, Energy Demand, and Rebound Risks

Prior research suggests that digital development can affect urban energy outcomes through efficiency gains, industrial restructuring, information improvements, and, conversely, rebound effects generated by infrastructure and expanded economic activity. Digitalization has been connected to urban energy efficiency, carbon intensity, carbon emission efficiency, green total factor productivity, and green innovation. At the city level, Ma et al. [13] report a nonlinear relationship between the digital economy and total factor energy efficiency, while Wang, Wang et al. [15] document heterogeneous urban energy efficiency effects. Lyu et al. [10]; Lian et al. [11]; Liu, Pang, and Liu [12]; Guo et al. [16];

Huang and Chen [17]; and Wang, Zheng, and Yang [14] link digital development to green total factor productivity through innovation, industrial upgrading, regulation, agglomeration, and allocation channels. Bai et al. [39] further show that digitalization and green innovation evolve jointly across urban agglomerations. Xia et al. [19] find improved carbon emission efficiency in a 269-city sample, and Wu et al. [20] emphasize both decarbonization and distributional environmental implications.

Digital infrastructure provides a particularly useful contrast because the literature contains both positive and negative estimates. Tang and Yang [1] find that digital infrastructure can raise several urban carbon emission measures through higher energy inputs, and Zhu and Lan [40] document a carbon rebound associated with the digital economy. By contrast, Zhang et al. [2], Peng et al. [3], Xiao et al. [4], Yu and Hu [5], and Liao and Liu [6] identify emission reduction or efficiency-enhancing effects under different policy designs and samples. This divergence motivates an explicit governance perspective: the environmental effect of digitalization depends on what digital capacity is used for and how it is coordinated with other policy objectives.

2.2. Policy Coordination and Local Government Attention

Government attention has become measurable through policy text, government work reports, and administrative documents. Man et al. [21], Tu et al. [22], and Du et al. [23] show that environmental attention is associated with greener regional outcomes, while Li et al. [41] document broader links between government attention and coordinated Sustainable Development Goal progress. At the innovation margin, government attention is associated with renewable energy innovation and green technology innovation [29,30,42,43]. Firm-level evidence similarly indicates that locally measured environmental attention can improve productivity or green innovation [28].

Digital government further illustrates the complementarity between information capacity and environmental governance. Hao et al. [25], Cao et al. [44], and Zhang et al. [45] find that digital government initiatives can promote green innovation through information, resource allocation, or business environment channels. Chen and Xing [46] connect the digital economy to high-quality development through green innovation. Most directly related to the present study, Liu, Li et al. [24] document synergistic effects between the digital economy and government governance in urban low-carbon transition.

Policy synergy is not limited to the digital domain. Text mining studies show that the configuration and coordination of pollution control and carbon policies matter for environmental outcomes. Zhu and Wang [47] map the evolution of pollution carbon policy synergy, Zhang et al. [48] analyze latent policy synergy across more than twelve thousand city air pollution policies, and Li et al. [49] show that environmental forcing and policy coordination can jointly shape pollution and carbon governance. Li and Lu [26] further show that digital government policy can strengthen combined pollution reduction, carbon reduction, and green expansion. These studies support an empirical approach in which policy combinations, rather than isolated indicators, are the object of measurement.

2.3. A Nonlinear Measure of Digital–Green Policy Alignment

Conventional additive indices treat policy dimensions as independent and monotonic contributors. The QSI instead permits the joint measure to vary with a context-sensitive alignment term, thereby representing reinforcing and offsetting policy configurations. The quantum-inspired terminology is analogical only: it is used to motivate a nonlinear representation of context dependence, not to assert that social institutions are quantum systems [31]. The empirical contribution of the measure must therefore be assessed through transparent construction, comparison with conventional indices, and robustness to alterna-

tive specifications. Recent work has also discussed quantum-like probability updating and applications to social science decision problems [32,33]. The present application uses this language only as a nonlinear measurement device. QSI is not a physical quantum variable and should be evaluated by construct validity, robustness, and predictive content rather than by physical interpretation. The joint attention benchmark in Section 4.1 increases only when digital and green attention are jointly high in the same year. Agreement between the two measures is an empirical question. A distinct QSI coefficient can show different conditional information but cannot establish that its nonlinearity measures coordination more accurately.

2.4. Hypotheses

H1. *The nonlinear configuration of digital and green policy attention measures is associated with urban energy intensity. A negative coefficient would be consistent with energy-saving governance, but the sign alone would not identify policy coordination or its causal effect.*

H2. *The QSI association should be stronger where local administrative and digital capacity allow for policy alignment to be converted into implementation. The analysis therefore tests heterogeneity by initial digital policy capacity, initial GDP, initial spatial inequality, regional location, and locational policy intensity.*

H3. *Governance synergy may reduce energy intensity by affecting the spatial distribution of activity and by lowering the archived energy use measure. Because energy attention and province digital attention enter the QSI construction, they are not treated as independent mediators; doing so would mechanically reuse components of the treatment variable.*

3. Data, Variables, and Empirical Strategy

3.1. Data Sources and Sample Construction

The unit of observation is a city in a calendar year. The estimation sample contains 282 city labels observed annually from 2007 through 2022, giving a balanced panel of 4512 city–year observations (282 $\times$ 16). The four province-level municipalities are included; their exclusion leaves 4448 observations (278 $\times$ 16). Table A1 lists every analysis–city label, the internal panel identifier used in the replication files, and the standardized province label. The numeric city id is an internal replication identifier rather than an official GB/T administrative code. The panel combines the archived energy use measure and GDP, city-level policy attention indicators, province-level digital policy attention, and city-level nighttime light inequality measures.

Exponentiating the 2022 log-GDP observations reproduces the official current price GDP totals for Beijing (41,610.882 versus 41,610.9), Shanghai (44,652.787 versus 44,652.80), Tianjin (16,311.294 versus 16,311.34), and Chongqing (29,129.043 versus 29,129.03), all in RMB 100 million, to rounding [50–53]. This verifies the recorded GDP scale but not the original download source for the full panel. By contrast, the archived Beijing 2022 energy use level, exp(ln energy use) = 4267.0 in archive units, does not equal the officially reported 68.969 million tonnes of standard coal-equivalent total energy consumption [54]. As an external validation of scale, exponentiating the 2022 log-GDP observations for the four province-level municipalities reproduces official current price GDP figures to within 0.05% (Table A2), confirming the recorded scale of the GDP series even though the upstream download source for the full panel could not be independently verified.

3.1.1. Duplicate Records and the Final Sample

The source panel contained 4527 rows. Five city–year combinations appeared four times each: Chongzuo in 2011 and 2016, Liuzhou in 2011, Baise in 2016, and Guigang in 2016 (Table A6). Within these groups, GDP, the archived energy use outcome, and nighttime light indicators were identical, whereas text-derived policy indicators differed. Collapsing each four-row group to its arithmetic city–year mean converts 20 records into five, removes 15 excess rows, and leaves 4512 unique city–year observations.

3.1.2. Geographic and Temporal Matching

City-level variables are indexed by the archive's internal city id and calendar year, while province-level digital attention is indexed conceptually by province and year. Table A1 fixes the exact city and province labels used in the reported regressions. Outside the source-level mean fill observations described in Section 3.1.3, province digital attention is constant within province–year cells, as expected for a province-level measure.

The label-cleaning procedure recovered intermittent missing province labels using each city's modal nonmissing province entry. Nine cities had no usable province label in the source panel and were assigned to their known provinces in the cleaned analysis file: Lüliang, Xinzhou, Jinzhong, Yuncheng, Changzhi, and Yangquan to Shanxi; Yan'an and Yulin to Shaanxi; and Shizuishan to Ningxia.

3.1.3. Missing Values and Model-Specific Samples

Table 1 reports 4512 nonmissing observations for each listed baseline analysis variable, and an audit of the cleaned numerical columns finds no NA values or common sentinel codes such as −9999 for those variables. For the nine cities named above, green attention is fixed at 4.420233, energy attention at 2.671954, province digital attention at 2.156515, and the phase at 0 in all 16 years (144 city–year observations); these constants equal the corresponding 4527-row source panel means. Province digital attention is also set to 2.156515 for every city in 2021 and 2022, and this exact value appears in 696 cleaned observations in total. These patterns are treated as upstream mean filling rather than as observed zero attention. The baseline retains the archived values, while Table A3 reports a sensitivity estimate excluding the nine fully mean filled cities; the pre-2020 specification in Table 2 also excludes the universally mean filled 2021–2022 province attention years.

Table 1. Descriptive statistics.

Variable	N	Mean	SD	Min	Median	Max
Log energy intensity	4512	−2.573	0.750	−5.138	−2.544	1.429
QSI	4512	1.128	0.790	0.040	1.057	6.885
Digital policy intensity	4512	1.493	1.121	0.000	1.386	4.710
Green attention	4512	4.421	0.630	0.000	4.511	5.820
Energy attention	4512	2.673	0.569	0.000	2.708	4.564
Province digital attention	4512	2.155	0.899	0.000	2.157	4.094
Log GDP	4512	7.245	1.010	4.151	7.183	10.707
Locational policy intensity	4512	0.003	0.002	0.000	0.003	0.012
Night light Gini	4512	0.640	0.232	0.039	0.675	0.987
Night light Theil	4512	0.308	0.134	0.009	0.298	0.761
Night light Atkinson	4512	0.523	0.277	0.005	0.543	0.980
Spatial inequality PC1	4512	0.000	1.625	−3.858	0.163	3.517
Log energy use	4512	4.672	1.257	0.143	4.705	8.359

Note. The cleaned sample contains 4512 city–year observations. Spatial inequality PC1 is the first principal component of the standardized nighttime light Gini, Theil, and Atkinson measures.

Table 2. Pearson correlations among key variables.

Variable	1	2	3	4	5	6	7	8
Energy intensity	1.000							
QSI	0.017	1.000						
Digital policy	0.289	0.019	1.000					
Green attention	0.078	−0.609	0.262	1.000				
Energy attention	0.034	−0.418	0.053	0.759	1.000			
Log GDP	−0.001	0.012	0.496	0.186	0.075	1.000		
Loc. policy	−0.004	−0.114	0.245	0.122	0.007	0.247	1.000	
Spatial inequality	−0.250	−0.038	−0.297	−0.070	−0.054	−0.575	−0.113	1.000

Note. Lower triangle shown. The bivariate correlation between QSI and log energy intensity is small; the main identifying variation comes from within-city changes after fixed effects and controls.

The fixed constants are inherited from the archived panel, not a theoretically preferred imputation estimator. Retaining them in the baseline preserves the exact supplied QSI construction and its published sample; it does not make mean substitution preferable to complete case analysis or multiple imputation. The missingness is concentrated in nine entire city histories and, for provincial digital attention, every city in 2021–2022. Without upstream text or external auxiliary information, a multiple-imputation model would require untestable assumptions about the missing data mechanism and would risk manufacturing within-city or province year variation where none was observed. Excluding the affected city histories or years changes the estimation population but provides transparent sensitivity bounds; their joint exclusion is reported in Table A4. The filled values are not interpreted as observed policy attention.

The model-specific sample sizes are consistent with the 16-year balanced panel. One-year lags, one-year leads, and first differences use 4230 observations (282 × 15), and two-year lags or leads use 3948 (282 × 14). Restricting the sample to 2007–2019 gives 3666 observations (282 × 13); excluding the four municipalities gives 4448 (278 × 16). The 1% winsorization sensitivity is fully specified: ln energy intensity, QSI, ln GDP, and locational-policy intensity are winsorized jointly over the pooled panel at the 1st and 99th percentiles using replacement; the preferred baseline is not winsorized.

3.2. Variable Definitions and Measurement

3.2.1. Archived Energy Use Measure, GDP, and Energy Intensity

Let E_{it} denote the archived energy use measure and Y_{it} the GDP series for city i in year t. The dependent variable is $\ln(EI_{it}) = \ln(E_{it}) - \ln(Y_{it})$, so $EI_{it} = E_{it}/Y_{it}$ and higher values indicate more of the archived energy use measure per unit of output. All underlying E_{it} and Y_{it} values are positive because the supplied log variables are finite throughout the estimation panel. Cross-checks against the four municipality-level 2022 statistical releases show that Y_{it} is recorded in current price RMB 100 million; no GDP deflator or constant price transformation appears in the replication archive. By contrast, the upstream physical coverage and unit of E_{it} are not retained, and the Beijing check above rules out simply relabeling the archive series as official total energy consumption. Accordingly, the dependent variable is interpreted as an archive-scale log energy intensity measure rather than assigned an unsupported physical unit. The variable labelled log energy efficiency is exactly the negative of log energy intensity and is therefore an inverse transformation of the same ratio, not an independent efficiency measure. Because nominal GDP enters the denominator, measured intensity can fall when local output prices rise without a commensurate increase in real output. A city-specific GDP deflator would be required for a constant-price intensity series.

A fixed multiplicative conversion of energy or currency units changes the level of the log ratio by an additive constant, but a change in price basis, energy concept, geographic coverage, or source methodology can change time variation. The empirical coefficients below therefore describe associations in the archived log measure; they are not converted into tonnes of standard coal per unit of GDP. This distinction is especially important for the 2016–2017 series discontinuity documented below. Year fixed effects remove price movements common to all cities, but they do not remove city-specific differences in inflation or changes in industrial output prices. Consequently, intertemporal changes in this ratio cannot be interpreted unconditionally as changes in physical energy use per unit of real output.

3.2.2. Policy Measures and QSI

Let d_{it}, g_{it}, e_{it}, and p_{it} denote the archived digital policy, green development, energy attention, and province-level digital attention variables, respectively. They are text-derived policy attention scores and should not be interpreted as expenditures, implemented investment, or measured digital output. The submission archive does not contain the original keyword dictionaries, document corpus, tokenization or regular expression rules, stop word treatment, denominator, overlap rule, or document-level aggregation code, so those upstream text extraction details cannot be reconstructed without invention. The empirical analysis therefore uses the four recorded series on their archived scales. The nonlinear QSI construction itself can, however, be reconstructed exactly from the 4527 source records. For $x \in \{d, g, e, p\}$, the standardized score uses the source panel mean μ_x and sample standard deviation s_x. The source moments (mean, SD) are d: (1.492390, 1.120805); g: (4.420233, 0.632383); e: (2.671954, 0.571731); and p: (2.156515, 0.898316). Equation (1) uses the absolute standardized digital and green amplitudes, while the phase is measured in radians as shown below. The four recorded inputs represent distinct roles: city digital policy intensity captures the local digital agenda; city green development attention captures the environmental objective; city energy attention describes the salience of energy management; and province digital attention supplies the broader digital policy context. This mapping motivates the choice of inputs but does not validate their upstream extraction. The missing dictionaries, corpus inventory, scoring denominator, treatment of overlapping phrases, and document-to-city-year aggregation prevent replication from the original policy texts. The component-level analysis therefore begins at the archived numerical measures.

$$z^x = \frac{x-\mu_x}{s_x}; D = \left|z^d\right|; G = |z^g|; \phi = \pi tanh\left(\frac{z^p - z^e}{2}\right) \quad (1)$$

$$QSI = \sqrt{D^2 + G^2 + 2DGcos(\phi)} \quad (2)$$

With these standardized-amplitude inputs, the stored record-level QSI is reproduced to rounding by Equation (2), and the stored alternative index is $QSI^{alt}_{it} = |z_{it}^{d} + z_{it}^{d}|$. Reconstruction error is below 2×10^{-6} for the source records, consistent with six-decimal storage. For the five duplicated city–year cells, the cleaned panel averages the four record-level QSI values, phase values, and components directly; consequently, Equation (2) is an exact source record construction but need not hold after substituting the separately averaged components in those five cleaned cells. This scale distinction resolves the apparent inconsistency between the raw attention means in Table 1 and the lower mean of QSI. Holding D_{it} and G_{it} fixed, a larger cosine term raises QSI, but across observations the index combines standardized policy attention magnitudes with phase alignment and should not be interpreted as a pure alignment angle. Absolute standardization makes a score equally large for the same positive or negative deviation from the source mean. The phase can

reinforce or offset the two nonnegative amplitudes. For example, Yongzhou in 2011 has green attention 0, QSI 6.885399, and a within-year green attention rank of 0.014; Guiyang in 2009 ranks 0.984 in digital and 0.911 in green attention, yet its QSI is 0.088222. Thus, a large QSI identifies a large nonlinear configuration of deviations and phase, not necessarily simultaneous high digital and green attention. Comparisons of policy synergy require the separate signed components and the benchmark below.

The parsimonious controls are log GDP and locationally oriented industrial policy intensity. Its mean of 0.003 is therefore not interpreted as a percentage or a word share. Nighttime light inequality is excluded from the preferred control set because it is examined as a potential downstream outcome. A separate construct validity model adds digital policy, green attention, energy attention, and province digital attention to evaluate whether QSI contains information beyond the additive levels of its constituent attention measures. The conventional benchmark is the geometric mean of the within-year percentile ranks of city digital policy and green development attention. It lies between zero and one and is high only when both ranks are high. Models use the same sample, controls, fixed effects, and city-clustered inference as the preferred QSI specification.

3.2.3. Nighttime Light Inequality

The three recorded nighttime light inequality measures are the Gini, Theil, and Atkinson indices. They describe dispersion in the measured spatial distribution of nighttime lights and are dimensionless; they are not direct measures of household income inequality. The supplied archive contains the city–year indices but not the underlying raster, boundary files, pixel mask, weighting scheme, treatment of zero or negative radiance, the exact Theil variant, or the Atkinson inequality aversion parameter. These upstream choices therefore cannot be reconstructed, and the paper does not infer them from the index names. The spatial inequality PC1 used in the mechanism analysis is fully reproducible from the supplied panel: Gini, Theil, and Atkinson are standardized over all 4512 observations using sample means (0.640408, 0.307745, 0.523210) and sample standard deviations (0.231798, 0.134237, 0.277404). The first-component loadings are 0.60295, 0.53302, and 0.59358, respectively; the sign is oriented so that higher scores indicate greater inequality. PC1 explains 87.98% of the joint variance and is not re-standardized afterward, which is why its sample standard deviation is 1.625 rather than 1.

Long-run nighttime light comparisons [55] require explicit sensor harmonization. As public benchmarks, the Earth Observation Group's Annual VNL V2 series begins in 2012, reports average radiance in nW cm^{-2} sr^{-1} on a 15 arc second EPSG:4326 grid, and has subsequent V2.x releases [56,57]. The cross-sensor NPP-VIIRS-like product of Chen et al. [58] covers 2000–2018 at 15 arc seconds. Neither reference establishes which raster product generated the supplied 2007–2022 inequality indices. Because the submission archive does not include year-specific raster filenames, product versions, retrieval dates, or cross-sensor processing code, the nighttime light source provenance and harmonization method remain a reproducibility limitation rather than being filled with an unverified product name. Table 1 reports the archived analysis variables and Table 2 their Pearson correlations.

Figures 1 and 2 provide complementary descriptive evidence. Figure 1 displays annual standardized means of QSI and log energy intensity, while Figure 2 reports the distribution of QSI across city–year observations. These figures are descriptive and do not identify the conditional relationship estimated below. The visible 2016–2017 discontinuity was audited directly in the supplied panel: the cross-city mean change in log energy use is +0.738 (median +0.615), compared with +0.070 in log GDP, so the corresponding mean change in log energy intensity is +0.668; 97.5% of cities show an increase in the archived

energy use measure between those two years. Because upstream source version and unit records are unavailable, this audit cannot distinguish a genuine energy change from a measurement or splice break. Table A3 therefore also reports a sensitivity estimate that drops 2016 and 2017, and the series break is treated as a data limitation rather than assigned a speculative cause. Figure 1 now marks the 2016–2017 discontinuity with a shaded vertical band. The official national energy consumption benchmark for those years, and separate pre- and post-break estimates, appear in Tables A4 and A5. A national physical energy aggregate cannot substitute for a city–year energy measure in the regression.

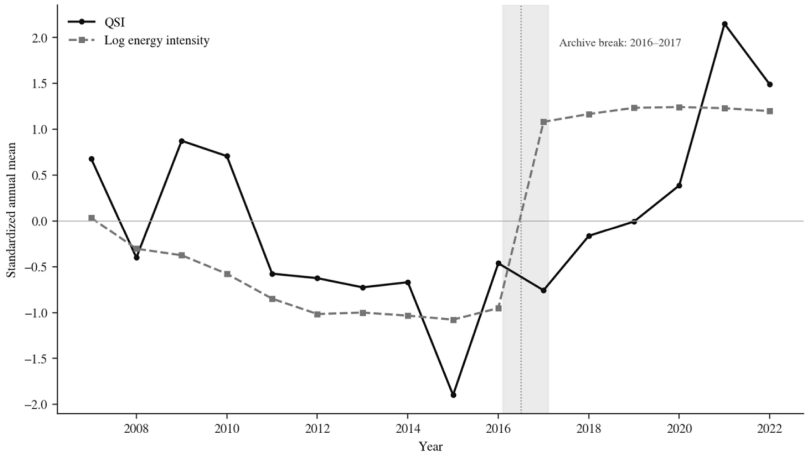


Figure 1. Standardized annual means of the digital–green policy synergy index and log energy intensity, 2007–2022. The shaded interval marks the 2016–2017 archive discontinuity.

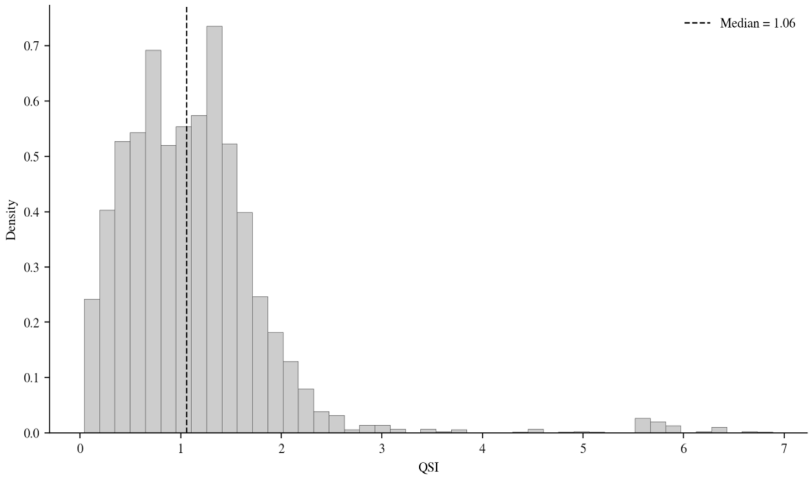


Figure 2. Distribution of the digital–green policy synergy index.

3.3. Econometric Specification

The preferred model relates log energy intensity to QSI, log GDP, and locational policy intensity while absorbing city and year fixed effects. City fixed effects remove time-invariant differences in industrial structure, geography, and baseline governance capacity; year fixed effects remove common macroeconomic and national policy shocks. Standard errors are clustered by city to allow arbitrary within-city serial correlation and heteroskedasticity.

$$ln(EI) = \beta QSI + \gamma^{\top} X + \mu_i + \lambda_t + \varepsilon \quad (3)$$

Several extensions test whether the result depends on a particular covariance estimator or low-dimensional specification. Province-by-year fixed effects absorb shocks

common to all cities in the same province–year. Driscoll–Kraay covariance allows for cross-sectional dependence in addition to serial correlation [34]. Wild cluster bootstrap inference is used because cluster-robust asymptotics can be sensitive to leverage and finite-cluster features [35,37,38]. A block permutation reassigns complete city QSI histories across cities, preserving the temporal structure of the treatment while breaking its city match.

Panel double/debiased machine learning is used as a nonlinear robustness check. City and year fixed effects are first removed from the outcome, treatment, and controls; nuisance functions for the residualized outcome and QSI are then estimated with gradient-boosted trees under five-fold cross-fitting grouped by city. The orthogonalized second-stage coefficient is estimated with city-clustered covariance, following the logic of double/debiased machine learning [36].

Distributional heterogeneity is analyzed with recentered influence function regressions at the 10th, 25th, 50th, 75th, and 90th unconditional quantiles of energy intensity [59]. Subgroup heterogeneity is tested with interaction terms rather than separate-regression significance comparisons. Initial city characteristics are measured over 2007–2010 to reduce post-treatment sorting, and Benjamini–Hochberg adjusted q-values are reported for the family of five interaction tests [60].

4. Empirical Results

4.1. Baseline Estimates

Table 3 reports estimates of the conditional relationship between QSI and log energy intensity. Column (1) includes city and year fixed effects. Column (2), the preferred specification, additionally controls for log GDP and locational policy intensity. Column (3) adds the individual policy attention components as a construct validity exercise. Column (4) replaces year fixed effects with province-by-year fixed effects while retaining city fixed effects. Table 4 compares QSI with a conventional joint attention index in otherwise identical models.

Table 3. Baseline fixed effects estimates.

Variable	(1)	(2)	(3)	(4)
QSI	−0.0557 ***	−0.0537 ***	−0.0693 ***	−0.0579 ***
	(0.0122)	(0.0120)	(0.0169)	(0.0110)
Digital policy			0.0186	
			(0.0138)	
Green attention			−0.0742 **	
			(0.0324)	
Energy attention			0.0649 **	
			(0.0275)	
Province digital attention			0.0213	
			(0.0182)	
Log GDP		−0.4677 ***	−0.4632 ***	−0.3344 ***
		(0.0877)	(0.0874)	(0.1190)
Locational policy intensity		5.9714	5.5193	3.4406
		(7.5607)	(7.5006)	(7.4348)
City fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No
Province × year fixed effects	No	No	No	Yes
Observations	4512	4512	4512	4512

Note. *** $p < 0.01$, ** $p < 0.05$. Standard errors clustered by city are in parentheses. Column (2) is the preferred parsimonious TWFE model. Column (4) absorbs province-by-year shocks.

Table 4. Comparison with conventional joint attention.

Vairable	(1) QSI	(2) Joint Attention	(3) Both
QSI	−0.0537 ***		−0.0538 ***
	(0.0120)		(0.0122)
Joint attention		0.0240	−0.0016
		(0.0446)	(0.0449)
Log GDP	−0.4677 ***	−0.4712 ***	−0.4676 ***
	(0.0877)	(0.0893)	(0.0877)
Locational policy	5.9714	9.5790	5.9815
	(7.5607)	(7.5459)	(7.5930)
City and year fixed effects	Yes	Yes	Yes
Observations	4512	4512	4512

Note. Joint attention is the geometric mean of the within-year percentile ranks of digital policy and green development attention. Each model uses the same 4512 observations, log GDP, locational policy intensity, city and year fixed effects, and city-clustered standard errors. *** $p < 0.01$. The index is not an independent validation of policy implementation.

In the preferred specification, the QSI coefficient is −0.0537 (SE = 0.0120; $p < 0.001$). Conditional on the included controls and fixed effects, a one-unit increase in QSI is associated with approximately 5.2% lower energy intensity. Because a one-unit change is large relative to the empirical scale of QSI, a one-standard-deviation increase in QSI (0.790) corresponds to a 0.0424-log-point change, or approximately 4.2% lower energy intensity, under the same specification. These are conditional associations rather than causal elasticities.

The estimate is similar after absorbing province-by-year shocks (−0.0579; SE = 0.0110). When component policy attention measures are entered jointly, the QSI coefficient remains negative (−0.0693; SE = 0.0169). This result is consistent with the index containing information beyond an additive specification, although it does not independently validate the causal interpretation of QSI. The benchmark results in Table 4 show that the QSI and joint attention index have a Pearson correlation of −0.121 and a Spearman correlation of −0.043. The joint attention estimate is 0.0240 (SE = 0.0446; $p = 0.591$), while QSI remains −0.0538 (SE = 0.0122) when both measures enter together. These results establish different conditional signals, not the superior construct validity of QSI.

The raw correlation between QSI and energy intensity is close to zero. Figure 3 therefore displays the conditional partial relationship after residualizing the outcome and QSI with respect to the specified fixed effects and controls.

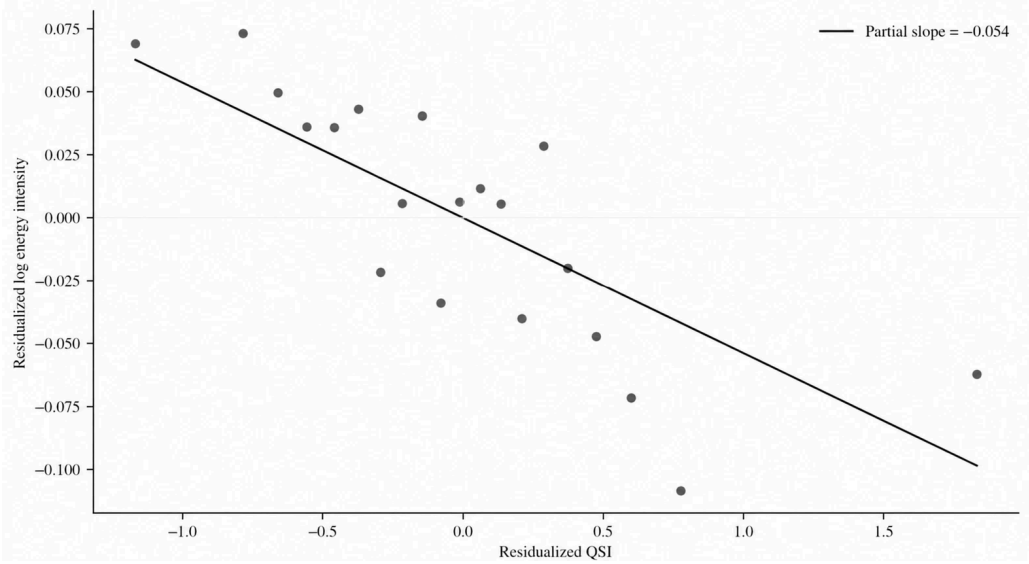


Figure 3. Conditional partial relationship between QSI and log energy intensity.

Figure 3 visualizes the conditional association estimated in Table 3; it is not a temporal identification or falsification test. The next section therefore evaluates sensitivity to inference assumptions, panel trends, imputed attention inputs, and the 2016–2017 measurement discontinuity.

4.2. Robustness and Inference

Table 5 broadens the robustness analysis beyond coefficient sign repetition. Lagged QSI remains negative at one and two years. The 1% winsorization result is based on pooled 1st/99th percentile replacement of log energy intensity, QSI, log GDP, and locational policy intensity and is nearly unchanged. The result also persists after excluding municipalities and limiting the sample to 2007–2019; the latter restriction removes the universally mean filled 2021–2022 province–digital attention values. The alternative QSI construction is negative and significant, and the province-by-year fixed effects model remains highly precise. In an additional data quality sensitivity reported in Table A3, excluding the nine cities whose green, energy, and province–digital attention series are fully mean-filled yields a QSI coefficient of −0.0543 (SE = 0.0119; N = 4368). Dropping the two years bracketing the 2016–2017 series discontinuity yields −0.0578 (SE = 0.0130; N = 3948). Neither exercise identifies the upstream measurement process, but neither suggests that these documented archive features alone generate the preferred levels association. Figure 4 plots the estimates and confidence intervals for the principal robustness checks.

Table 5. Robustness, alternative inference, and diagnostic specifications.

Specification	QSI Estimate	N	Inference
Preferred TWFE	−0.0537 *** (0.0120)	4512	City-clustered
Province × year FE	−0.0579 *** (0.0110)	4512	City-clustered
Lagged QSI, t − 1	−0.0608 *** (0.0133)	4230	City-clustered
Lagged QSI, t − 2	−0.0634 *** (0.0136)	3948	City-clustered
1% winsorization	−0.0555 *** (0.0120)	4512	City-clustered
Exclude municipalities	−0.0506 *** (0.0121)	4448	City-clustered
Pre-2020 sample	−0.0373 *** (0.0099)	3666	City-clustered
Alternative QSI	−0.0357 *** (0.0094)	4512	City-clustered
Panel DML	−0.0756 *** (0.0175)	4512	5-fold city cross-fit
Driscoll–Kraay SE	−0.0537 *** (0.0182)	4512	$p = 0.0098$
Wild cluster bootstrap	−0.0537 *** (0.0120)	4512	bootstrap $p = 0.002$
City-specific linear trends	0.0089 (0.0081)	4512	$p = 0.274$
First differences	0.0009 (0.0040)	4230	$p = 0.822$
Lead placebo, t + 1	−0.0460 *** (0.0117)	4230	$p = 0.0001$
Lead placebo, t + 2	−0.0366 *** (0.0104)	3948	$p = 0.0005$

Note. *** $p < 0.01$. The Driscoll–Kraay row changes the covariance estimator, not the point estimate. The wild bootstrap row reports the bootstrap p-value. The final four rows are diagnostics and are intentionally retained even though they weaken a strict causal interpretation.

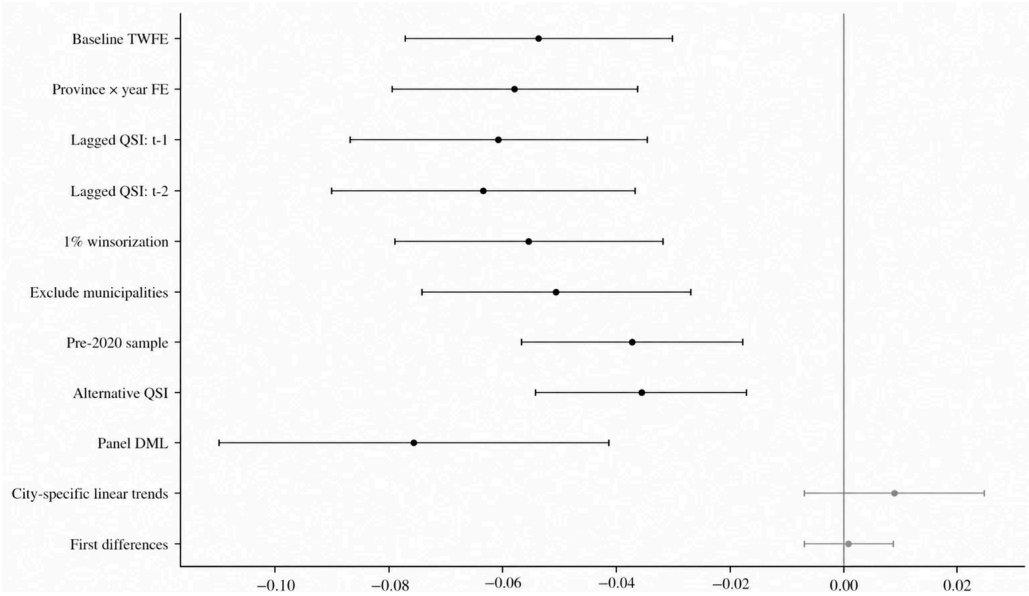


Figure 4. Coefficient stability across robustness and diagnostic specifications.

The Driscoll–Kraay standard error is 0.0182, yielding $p = 0.0098$. The wild city cluster bootstrap p-value is 0.002. A city history permutation test with 999 block permutations produces a two-sided p-value of 0.0010; the placebo distribution is centered near zero while the observed coefficient lies in its tail. Leave-one-province-out estimates are negative in every case, ranging from −0.0576 to −0.0481, and all remain significant at the 5% level.

Not all diagnostics reinforce a causal reading. Adding city-specific linear trends changes the QSI coefficient to 0.0089 ($p = 0.274$), and the first-difference estimate is 0.0009 ($p = 0.822$). Moreover, one- and two-period leads of QSI are significantly negative. This pattern is compatible with slow-moving omitted trajectories, anticipation, or policy attention responding to persistent energy conditions. The main result should therefore be interpreted as a robust within-city association, not as a fully isolated structural effect.

The structural break audit materially qualifies the pooled finding (Table A4). On non-overlapping samples, the QSI coefficient is −0.0065 (SE = 0.0075; $p = 0.386$) in 2007–2015 and +0.0118 (SE = 0.0122; $p = 0.334$) in 2017–2022. Neither is distinguishable from zero; under province-by-year effects, the respective estimates are −0.0072 (SE = 0.0080) and +0.0007 (SE = 0.0096). In contrast, replacing year effects with East/non-East-by-year effects on the pooled panel gives −0.0537 (SE = 0.0120), and province-by-year effects give −0.0579 (SE = 0.0110). Excluding both the nine mean-filled city histories and the universally filled 2021–2022 years gives −0.0466 (SE = 0.0109; N = 3822). Thus, the pooled association survives regional time controls and input exclusions, but the absence of detectable within-regime effects means it cannot be characterized as stable across measurement regimes. These diagnostics are not evidence of a physical energy-saving effect.

5. Heterogeneity Analysis

Heterogeneity is evaluated with formal QSI-by-group interactions. The subgroup indicators are predetermined from 2007–2010 city averages where applicable, and the family of five interaction tests is adjusted using the Benjamini–Hochberg false discovery rate procedure. This avoids the common but invalid practice of declaring groups different because one subgroup coefficient is significant and another is not. Table 6 reports the interaction estimates and Figure 5 plots the corresponding conditional coefficients.

Table 6. Heterogeneous QSI associations with formal interaction tests.

Dimension	Group 0	Effect 0	Group 1	Effect 1	Difference	BH q
Region	Non-east	−0.0492 ***	East	−0.0677 ***	−0.0185	0.5121
		(0.0138)		(0.0244)	(0.0281)	
Initial spatial inequality	High	−0.0174	Low	−0.1110 ***	−0.0936 ***	0.0021
		(0.0141)		(0.0221)	(0.0262)	
Initial locational policy intensity	Low	−0.0450 ***	High	−0.0671 ***	−0.0221	0.4851
		(0.0142)		(0.0213)	(0.0255)	
Initial GDP	Low	−0.0362 **	High	−0.0802 ***	−0.0441 *	0.1464
		(0.0145)		(0.0213)	(0.0257)	
Initial digital policy intensity	Low	−0.0286 **	High	−0.0980 ***	−0.0695 ***	0.0169
		(0.0142)		(0.0213)	(0.0255)	

Note. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All models include city and year fixed effects, log GDP, and locational policy intensity, with city-clustered standard errors. The difference column is the interaction coefficient. BH q is the Benjamini–Hochberg adjusted p-value across the five interaction tests.

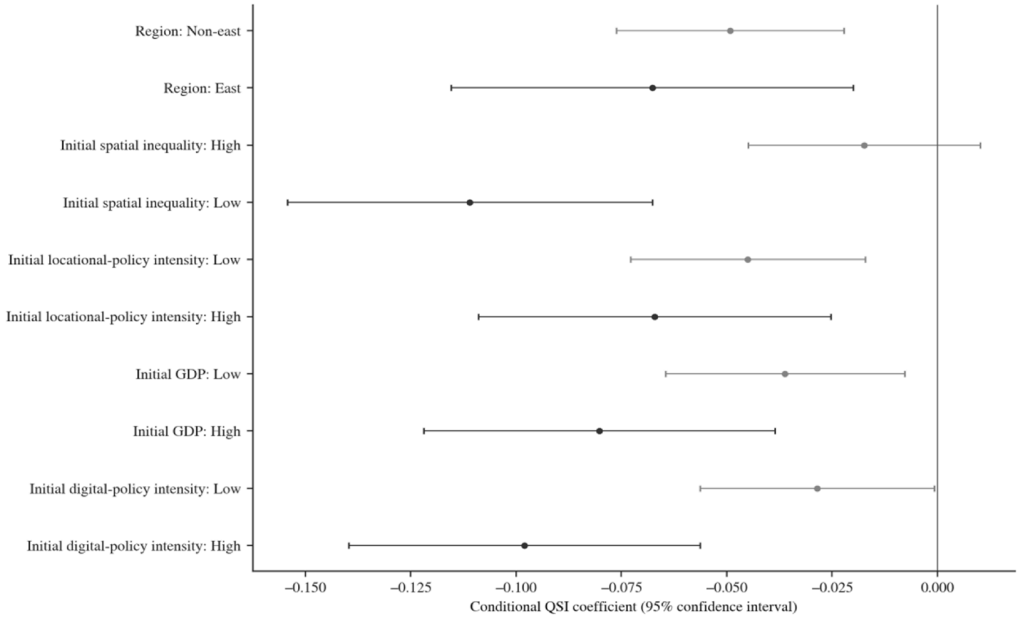


Figure 5. Conditional QSI coefficients across pre-specified city groups.

The strongest and most statistically distinct heterogeneity appears in initial spatial inequality and initial digital policy capacity. In cities with low initial spatial inequality, the QSI effect is −0.1110, compared with −0.0174 in high-inequality cities; the difference is −0.0936 (p = 0.0004, BH q = 0.0021). In cities with high initial digital policy intensity, the effect is −0.0980, compared with −0.0286 in low initial digital cities; the difference is −0.0695 (p = 0.0068, BH q = 0.0170). These differences are consistent with variation in implementation capacity but cannot establish that policy coordination caused the estimated changes in the archived intensity ratio.

Eastern and non-eastern cities do not differ significantly in the formal interaction (p = 0.512). The locational policy interaction is also imprecise (p = 0.388). The initial GDP difference is marginal before adjustment (p = 0.088) and does not survive the five-test correction (BH q = 0.146). The stronger evidence concerns initial digital policy capacity and spatial inequality.

6. Mechanism-Consistent Evidence and Outcome Decomposition

6.1. Spatial Inequality

Because energy attention and province digital attention are inputs to QSI, using them as mediators would mechanically regress a constructed index on its own components. The revised mechanism analysis therefore focuses on a variable external to the QSI formula: nighttime light spatial inequality. Table 7 reports the mechanism-consistent tests.

Table 7. Mechanism-consistent tests and accounting decomposition.

Outcome	Term	Estimate
Spatial inequality PC1	QSI	−0.0439 ***
		(0.0121)
Log energy intensity	Spatial inequality PC1 | QSI	−0.0078
		(0.0194)
Log energy intensity	QSI | spatial inequality PC1	−0.0541 ***
		(0.0120)
Log energy use measure	QSI	−0.0527 ***
		(0.0123)
Log GDP	QSI	0.0019
		(0.0059)
Log energy intensity	QSI	−0.0547 ***
		(0.0124)

Note. *** $p < 0.01$. All models absorb city and year fixed effects and cluster by city. The decomposition uses a common control set so that the QSI coefficient for log energy intensity equals the coefficient for log energy use minus the coefficient for log GDP.

The QSI coefficient in the spatial inequality equation is −0.0439 (SE = 0.0121, $p < 0.001$), indicating that a higher QSI is associated with a more even intra-city distribution of nighttime light activity. However, once QSI is included in the energy intensity equation, the spatial inequality PC1 coefficient is −0.0078 (SE = 0.0194, $p = 0.688$). Individual Gini, Theil, and Atkinson specifications yield the same qualitative conclusion: QSI predicts lower inequality, but the conditional mediator coefficient is not statistically distinguishable from zero. The evidence therefore shows an association with lower measured spatial inequality but does not establish mediation from QSI through inequality to energy intensity.

6.2. Energy Use Decomposition

The identity log energy intensity = log energy use − log GDP permits a transparent accounting decomposition on the archived scale. With city and year fixed effects and locational policy intensity as a common control, QSI is associated with a 0.0527-log-point reduction in the archived log energy use measure, while its association with log GDP is 0.0019. With identical observations, controls, and weights, the unrounded coefficients satisfy the accounting identity; the reported energy intensity coefficient is −0.0547. The decomposition therefore shows that the estimated ratio change is reflected primarily in the numerator series rather than GDP expansion. Because the physical unit and scope of the archived energy use measure are not documented, this is an accounting result within the supplied data and should not be relabeled as a causal reduction in official total energy consumption. Figure 6 displays this identity-preserving decomposition.

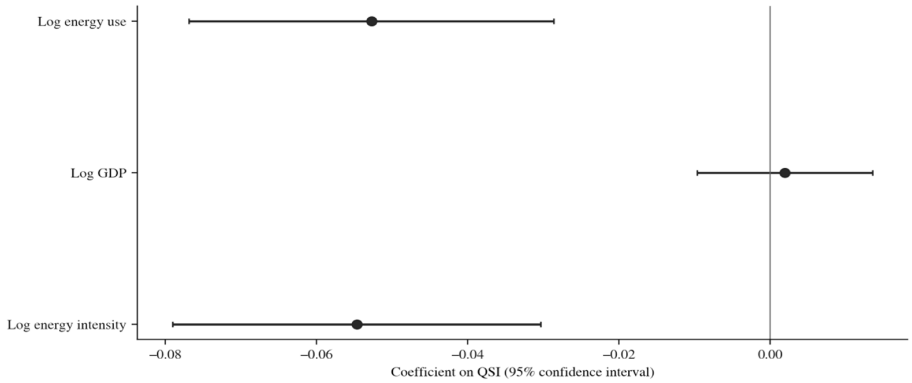


Figure 6. Accounting decomposition of the QSI association with energy intensity.

7. Further Analysis

7.1. Unconditional Distributional Effects

Mean regressions can conceal distributional differences. Recentered influence function regressions estimate the association of QSI with unconditional quantiles of the energy intensity distribution while retaining the fixed-effects structure. The 10th-quantile estimate is small and imprecise, whereas the coefficients become negative and significant from the 25th quantile onward. The largest point estimate occurs at the 75th quantile. Table 8 gives the numerical estimates and Figure 7 summarizes the coefficient profile.

Table 8. RIF unconditional quantile regressions.

Quantile	Outcome Quantile	QSI Estimate
0.10	−3.510	−0.0085
		(0.0323)
0.25	−3.047	−0.0549 **
		(0.0224)
0.50	−2.544	−0.0550 ***
		(0.0168)
0.75	−2.125	−0.0766 ***
		(0.0161)
0.90	−1.753	−0.0715 ***
		(0.0272)

Note. *** $p < 0.01$, ** $p < 0.05$. Recentered influence function regressions with city and year fixed effects, log GDP, locational policy intensity, and city-clustered standard errors.

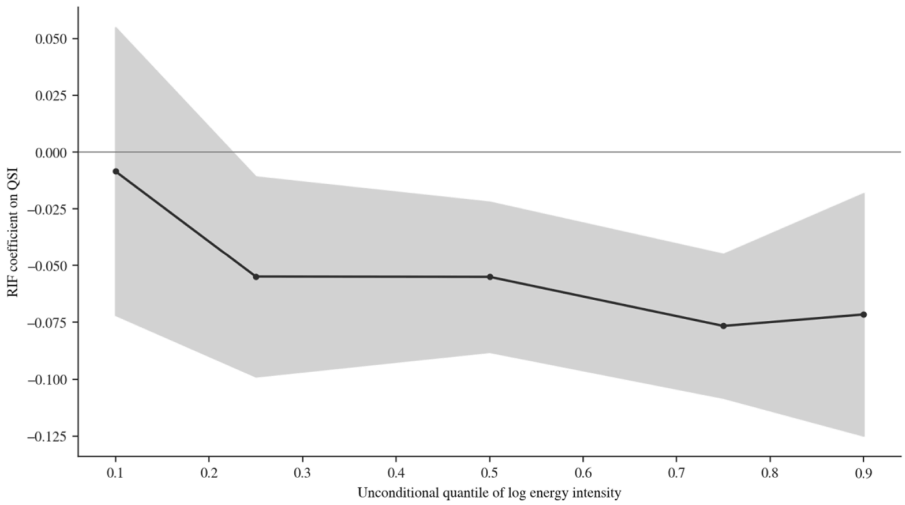


Figure 7. QSI coefficients across unconditional energy intensity quantiles.

At the 75th quantile, the QSI coefficient is −0.0766 (SE = 0.0161); at the 90th it is −0.0715 (SE = 0.0272). The 10th-quantile estimate is imprecise. This pattern is descriptive and does not identify heterogeneous causal effects.

7.2. Nonlinearity and Influence Diagnostics

A quadratic TWFE model yields a negative linear QSI term (−0.0704, $p < 0.001$) and a positive but statistically insignificant squared term (0.0075, $p = 0.159$). The data therefore do not provide strong evidence for a stable U-shaped or inverted-U relationship over the observed support. Leave-one-province-out analysis shows that the baseline coefficient remains negative regardless of which province is excluded, reducing concern that a single province drives the main result.

A same-province peer QSI instrumental variable sensitivity check is reported in Appendix A rather than used for identification. Its first stage is strong in a statistical sense, but the exclusion restriction is not credible because peer city governance can affect local energy intensity through provincial coordination, policy diffusion, and other spillovers. Accordingly, the paper does not treat this exercise as causal evidence.

8. Discussion

The benchmark sharpens this qualification. Because QSI assigns large amplitudes to deviations below as well as above the mean, its association with energy intensity cannot be read as evidence that simultaneous digital and green attention itself lowers energy use. The conventional joint attention coefficient is imprecise. Coordination remains a plausible interpretation to test with independently coded policy texts and measures of implementation.

The findings are compatible with policy coordination shaping how digital capacity is deployed, as discussed by Lian et al. [11]; Liu, Li et al. [24]; Zhu and Wang [47]; and Zhang et al. [48]. Plausible institutional, technological, and economic routes include shared monitoring information and enforcement, targeting digital tools to industrial energy management, and reallocating activity away from energy-intensive uses. The current regressions do not separately identify these processes or show that simultaneously high digital and green attention caused the energy intensity association.

Initial digital policy capacity and spatial balance show the clearest formal interactions. These differences may reflect implementation conditions for digital monitoring and public services [3,5,25,44,45]. They do not show that either initial condition caused the QSI association.

The accounting identity shows that the negative intensity coefficient is concentrated in the archived energy use numerator rather than in GDP expansion. This pattern can be compared with the mixed digital infrastructure evidence [1,2,4,6]. Because the numerator has no verified physical unit, it cannot establish a quantified change in official energy consumption or a causal mechanism.

City-specific trends and first differences remove the statistical precision of the QSI coefficient, while future QSI predicts current intensity. Persistent city trajectories, anticipatory adjustment, or omitted factors may explain part of the levels association. The archived energy use series lacks verified city-level physical coverage and units; nighttime lights measure spatial activity, so they cannot serve as an independent energy outcome. The 2016–2017 series discontinuity and nominal GDP denominator further limit intertemporal interpretation. A credible causal design and an independently documented physical energy series remain priorities for future work.

A future identification design could exploit staggered, officially dated city or provincial mandates that explicitly combine digital monitoring with energy or environmental

enforcement, rather than treating generic broadband investment as an instrument for policy synergy. With contemporaneously archived adoption notices, independently coded implementation intensity, and harmonized physical energy and constant price output data, an event study using not-yet-treated comparison cities and cohort-specific group–time effects could examine differential pre-trends and anticipation, which the present significant QSI leads do not satisfy. A pre-specified post-2017 measurement window, city-specific trend diagnostics, neighboring city spillover tests, and placebo outcomes would address the source break and remaining time-varying confounding. Where assignment follows a documented eligibility threshold, a local discontinuity or difference-in-discontinuities design could provide a stronger alternative, subject to tests of manipulation and continuity. These are proposed research strategies, not identification achieved in this paper.

The new period-stratified evidence sharpens the identification problem: the pooled coefficient may rely on cross-regime covariance between QSI and an energy numerator whose measurement changes in 2016–2017, rather than on a replicable association within a homogeneous measurement period. Official national total energy consumption rose from 4.36 to 4.49 billion tonnes of standard coal equivalent between 2016 and 2017, approximately 2.9%, whereas the equally weighted archived city mean log energy use increase is 0.738. These differently scoped quantities cannot be equated, but the stark contrast is a diagnostic reason to obtain harmonized city-level physical energy accounts before interpreting pooled estimates (Table A5).

9. Policy Implications

Local evaluations can connect digital programs to explicit energy and environmental objectives. Industrial monitoring, process optimization, and enforcement are plausible applications, but energy-intensive digital expansion could offset savings. The estimated QSI association alone cannot determine the net effect of any particular program.

Implementation capacity may condition the observed association. Support for information systems, technical training, and transparent monitoring can help cities test whether digital initiatives reduce measured energy use. Evidence on government attention and green outcomes suggests that administrative and financial capacity matter [21,23,27,29,41]. These recommendations remain conditional on further validation of the energy series.

Program evaluations should report independently documented physical energy use alongside constant-price energy intensity ratios. That pairing distinguishes a lower numerator from a change driven by nominal GDP. The present archive lacks the units and city-specific deflators needed for that comparison.

10. Conclusions

This study examines how a nonlinear configuration of digital, green, energy, and province-level policy attention is associated with recorded urban energy intensity in 282 Chinese cities from 2007 to 2022. QSI is a measure of standardized deviations and phase alignment. It does not measure simultaneous high digital and green attention monotonically.

The preferred fixed effects coefficient is -0.0537, or approximately 5.2% lower recorded intensity for a one-unit QSI difference. Several levels specifications and inference procedures retain a negative association. The conventional joint attention index is imprecise; QSI remains negative when both enter together. This comparison indicates different conditional signals, not superior measurement validity. City trends, first differences, and lead placebos limit a causal reading. However, estimates fitted separately for 2007–2015 and 2017–2022 are small and statistically imprecise, so the pooled coefficient is not established as a stable within-regime association.

The nighttime light inequality measure falls as QSI rises, but its conditional coefficient does not support statistical mediation. An identity-preserving decomposition places most of the intensity association in the archived energy use numerator, not GDP. Since the physical unit and scope of that numerator are undocumented, this is an accounting result in the archived series.

The missing policy text corpus and extraction rules, uncertain energy use provenance, nominal GDP denominator, and 2016–2017 discontinuity constrain interpretation. The analyses establish a conditional relationship in the archived panel. Future studies need independently coded policy documents, verified physical energy series, city-specific constant-price GDP, and credible exogenous variation before treating the index as a validated measure of coordination or its coefficient as causal. Independent national energy statistics corroborate the need to investigate the break but are not an external city panel substitute.

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Data Availability Statement: The GDP scale was checked against official 2022 municipal statistical bulletins [50–53]. The original policy text corpus, energy use source, and nighttime light raster provenance can request from the authors.

Conflicts of Interest: The authors declare no conflicts of interest.

Appendix A

Data Audit and Additional Diagnostics

Table A6 documents the source panel duplicate correction. Table A3 collects data quality, falsification, influence, and exploratory IV diagnostics; Figures A1–A3 visualize the permutation, leave-one-province-out, and lead lag evidence. Table A1 lists the 282 final analysis city labels and internal panel identifiers, and Table A2 provides an external scale check for the 2022 GDP observations in the four province-level municipalities. Table A4 reports break regime, region-by-year, and mean fill sensitivity checks; Table A5 contrasts the archived 2016–2017 break with the independently published national physical energy series. Table A7 documents the available variable codebook.

Table A1. Final analysis city list and internal panel identifiers.

ID	City	Province	ID	City	Province	ID	City	Province
1	Qitaihe	Heilongjiang	95	Changde	Hunan	189	Baicheng	Jilin
2	Sanya	Hainan	96	Pingliang	Gansu	190	Baishan	Jilin
3	Sanming	Fujian	97	Pingdingshan	Henan	191	Baiyin	Gansu
4	Sanmenxia	Henan	98	Guangyuan	Sichuan	192	Baise	Guangxi
5	Shanghai	Shanghai	99	Guang'an	Sichuan	193	Yiyang	Hunan
6	Shangrao	Jiangxi	100	Guangzhou	Guangdong	194	Yancheng	Jiangsu
7	Dongguan	Guangdong	101	Qingyang	Gansu	195	Panjin	Liaoning
8	Dongying	Shandong	102	Langfang	Hebei	196	Meishan	Sichuan
9	Zhongwei	Ningxia	103	Yan'an	Shaanxi	197	Shizuishan	Ningxia
10	Zhongshan	Guangdong	104	Kaifeng	Henan	198	Shijiazhuang	Hebei
11	Linfen	Shanxi	105	Zhangjiakou	Hebei	199	Fuzhou	Fujian
12	Linyi	Shandong	106	Zhangjiajie	Hunan	200	Qinhuangdao	Hebei

Table A1. *Cont.*

ID	City	Province	ID	City	Province	ID	City	Province
13	Lincang	Yunnan	107	Zhangye	Gansu	201	Shaoxing	Zhejiang
14	Dandong	Liaoning	108	Xuzhou	Jiangsu	202	Mianyang	Sichuan
15	Lishui	Zhejiang	109	Dezhou	Shandong	203	Liaocheng	Shandong
16	Lijiang	Yunnan	110	Deyang	Sichuan	204	Zhaoqing	Guangdong
17	Ulanqab	Inner Mongolia	111	Xinzhou	Shanxi	205	Zigong	Sichuan
18	Wuhai	Inner Mongolia	112	Huaihua	Hunan	206	Zhoushan	Zhejiang
19	Urumqi	Xinjiang	113	Huizhou	Guangdong	207	Wuhu	Anhui
20	Leshan	Sichuan	114	Chengdu	Sichuan	208	Suzhou	Jiangsu
21	Jiujiang	Jiangxi	115	Yangzhou	Jiangsu	209	Maoming	Guangdong
22	Yunfu	Guangdong	116	Chengde	Hebei	210	Jingzhou	Hubei
23	Bozhou	Anhui	117	Fuzhou	Jiangxi	211	Jingmen	Hubei
24	Yichun	Heilongjiang	118	Fushun	Liaoning	212	Putian	Fujian
25	Foshan	Guangdong	119	Jieyang	Guangdong	213	Heze	Shandong
26	Jiamusi	Heilongjiang	120	Panzhihua	Sichuan	214	Pingxiang	Jiangxi
27	Baoding	Hebei	121	Xinxiang	Henan	215	Yingkou	Liaoning
28	Baoshan	Yunnan	122	Xinyu	Jiangxi	216	Huludao	Liaoning
29	Xinyang	Henan	123	Wuxi	Jiangsu	217	Bengbu	Anhui
30	Karamay	Xinjiang	124	Rizhao	Shandong	218	Hengshui	Hebei
31	Lu'an	Anhui	125	Kunming	Yunnan	219	Hengyang	Hunan
32	Liupanshui	Guizhou	126	Zhaotong	Yunnan	220	Quzhou	Zhejiang
33	Lanzhou	Gansu	127	Jinzhong	Shanxi	221	Xiangyang	Hubei
34	Neijiang	Sichuan	128	Jincheng	Shanxi	222	Xining	Qinghai
35	Baotou	Inner Mongolia	129	Pu'er	Yunnan	223	Xi'an	Shaanxi
36	Beijing	Beijing	130	Jingdezhen	Jiangxi	224	Xuchang	Henan
37	Beihai	Guangxi	131	Qujing	Yunnan	225	Guigang	Guangxi
38	Shiyan	Hubei	132	Shuozhou	Shanxi	226	Guiyang	Guizhou
39	Nanjing	Jiangsu	133	Chaoyang	Liaoning	227	Hezhou	Guangxi
40	Nanchong	Sichuan	134	Benxi	Liaoning	228	Ziyang	Sichuan
41	Nanning	Guangxi	135	Laibin	Guangxi	229	Ganzhou	Jiangxi
42	Nanping	Fujian	136	Hangzhou	Zhejiang	230	Chifeng	Inner Mongolia
43	Nanchang	Jiangxi	137	Songyuan	Jilin	231	Liaoyuan	Jilin
44	Nantong	Jiangsu	138	Zaozhuang	Shandong	232	Liaoyang	Liaoning
45	Nanyang	Henan	139	Liuzhou	Guangxi	233	Dazhou	Sichuan
46	Xiamen	Fujian	140	Zhuzhou	Hunan	234	Yuncheng	Shanxi
47	Shuangyashan	Heilongjiang	141	Guilin	Guangxi	235	Lianyungang	Jiangsu
48	Taizhou	Zhejiang	142	Meizhou	Guangdong	236	Tonghua	Jilin
49	Hefei	Anhui	143	Wuzhou	Guangxi	237	Tongliao	Inner Mongolia
50	Ji'an	Jiangxi	144	Yulin	Shaanxi	238	Suining	Sichuan
51	Jilin	Jilin	145	Wuwei	Gansu	239	Zunyi	Guizhou
52	Lüliang	Shanxi	146	Wuhan	Hubei	240	Xingtai	Hebei
53	Wuzhong	Ningxia	147	Yongzhou	Hunan	241	Handan	Hebei
54	Zhoukou	Henan	148	Hanzhong	Shaanxi	242	Shaoyang	Hunan
55	Hulunbuir	Inner Mongolia	149	Shantou	Guangdong	243	Zhengzhou	Henan
56	Hohhot	Inner Mongolia	150	Shanwei	Guangdong	244	Chenzhou	Hunan
57	Xianning	Hubei	151	Jiangmen	Guangdong	245	Ordos	Inner Mongolia
58	Xianyang	Shaanxi	152	Chizhou	Anhui	246	Ezhou	Hubei
59	Harbin	Heilongjiang	153	Shenyang	Liaoning	247	Jiuquan	Gansu
60	Tangshan	Hebei	154	Cangzhou	Hebei	248	Chongqing	Chongqing
61	Shangqiu	Henan	155	Hechi	Guangxi	249	Jinhua	Zhejiang
62	Shangluo	Shaanxi	156	Heyuan	Guangdong	250	Jinchang	Gansu
63	Jiaxing	Zhejiang	157	Quanzhou	Fujian	251	Tieling	Liaoning
64	Jiayuguan	Gansu	158	Tai'an	Shandong	252	Tongchuan	Shaanxi
65	Siping	Jilin	159	Taizhou	Jiangsu	253	Tongling	Anhui
66	Guyuan	Ningxia	160	Luzhou	Sichuan	254	Yinchuan	Ningxia
67	Datong	Shanxi	161	Luoyang	Henan	255	Jinzhou	Liaoning
68	Daqing	Heilongjiang	162	Jinan	Shandong	256	Zhenjiang	Jiangsu
69	Dalian	Liaoning	163	Jining	Shandong	257	Changchun	Jilin
70	Tianshui	Gansu	164	Haikou	Hainan	258	Changsha	Hunan
71	Tianjin	Tianjin	165	Zibo	Shandong	259	Changzhi	Shanxi
72	Taiyuan	Shanxi	166	Huaibei	Anhui	260	Fuxin	Liaoning
73	Weihai	Shandong	167	Huainan	Anhui	261	Fuyang	Anhui
74	Loudi	Hunan	168	Huai'an	Jiangsu	262	Fangchenggang	Guangxi
75	Xiaogan	Hubei	169	Shenzhen	Guangdong	263	Yangjiang	Guangdong
76	Ningde	Fujian	170	Qingyuan	Guangdong	264	Yangquan	Shanxi
77	Ningbo	Zhejiang	171	Wenzhou	Zhejiang	265	Longnan	Gansu
78	Anqing	Anhui	172	Weinan	Shaanxi	266	Suizhou	Hubei
79	Ankang	Shaanxi	173	Huzhou	Zhejiang	267	Ya'an	Sichuan
80	Anyang	Henan	174	Xiangtan	Hunan	268	Qingdao	Shandong

Table A1. *Cont.*

ID	City	Province	ID	City	Province	ID	City	Province
81	Anshun	Guizhou	175	Zhanjiang	Guangdong	269	Anshan	Liaoning
82	Dingxi	Gansu	176	Chuzhou	Anhui	270	Shaoguan	Guangdong
83	Yibin	Sichuan	177	Binzhou	Shandong	271	Ma'anshan	Anhui
84	Yichang	Hubei	178	Luohe	Henan	272	Zhumadian	Henan
85	Yichun	Jiangxi	179	Zhangzhou	Fujian	273	Jixi	Heilongjiang
86	Baoji	Shaanxi	180	Weifang	Shandong	274	Hebi	Henan
87	Xuancheng	Anhui	181	Chaozhou	Guangdong	275	Hegang	Heilongjiang
88	Suzhou	Anhui	182	Puyang	Henan	276	Yingtan	Jiangxi
89	Suqian	Jiangsu	183	Yantai	Shandong	277	Huanggang	Hubei
90	Yueyang	Hunan	184	Jiaozuo	Henan	278	Huangshan	Anhui
91	Chongzuo	Guangxi	185	Mudanjiang	Heilongjiang	279	Huangshi	Hubei
92	Bazhong	Sichuan	186	Yulin	Guangxi	280	Heihe	Heilongjiang
93	Bayannur	Inner Mongolia	187	Yuxi	Yunnan	281	Qiqihar	Heilongjiang
94	Changzhou	Jiangsu	188	Zhuhai	Guangdong	282	Longyan	Fujian

Note. City and province names have been standardized in English; internal panel identifiers are retained. City id is an internal replication identifier, not an official administrative code. The submission archive does not contain the initial candidate-city list, official-code crosswalk, exclusion log, or boundary vintage.

Table A2. External validation of the 2022 GDP scale.

City	Archive Exp(ln GDP)	Official GDP	Difference	Official Source
Beijing	41,610.882	41,610.90	−0.018	Beijing statistical bulletin
Shanghai	44,652.787	44,652.80	−0.013	Shanghai statistical bulletin
Tianjin	16,311.294	16,311.34	−0.046	Tianjin statistical bulletin
Chongqing	29,129.043	29,129.03	+0.013	Chongqing statistical bulletin

Note. Units are RMB 100 million. The near-exact matches establish the scale of the archived 2022 GDP values; they do not identify the upstream download source for the full 2007–2022 panel.

Table A3. Influence and falsification diagnostics.

Diagnostic	Estimate/Range	Inference
City history permutation	Observed = −0.0537	$p = 0.0010$
Leave-one-province-out	Coefficient range	−0.0576 to −0.0481
Lead QSI, t+1	−0.0460	$p = 0.0001$
Lead QSI, t+2	−0.0366	$p = 0.0005$
City-specific trends	0.0089	$p = 0.274$
First differences	0.0009	$p = 0.822$
Exploratory peer QSI IV	0.0306 (0.0919)	$p = 0.739$; first-stage F ≈ 31.3
Exclude nine fully mean filled cities	−0.0543 (0.0119)	$p < 0.001$; N = 4368
Drop 2016 and 2017	−0.0578 (0.0130)	$p < 0.001$; N = 3948

Note. The IV is not used as a preferred causal design because same-province peer QSI may affect local energy intensity through provincial spillovers, violating the exclusion restriction. The two data quality sensitivity rows use the preferred TWFE control set and city-clustered standard errors.

Table A4. Structural break and missing attention sensitivity estimates.

Specification	QSI Coef. (SE)	N	Time Controls
Full sample, 2007–2022	−0.0537 (0.0120)	4512	Year
Pre-break, 2007–2015	−0.0065 (0.0075)	2538	Year
Post-break, 2017–2022	+0.0118 (0.0122)	1692	Year
Pre-break, province-by-year	−0.0072 (0.0080)	2538	Province × year
Post-break, province-by-year	+0.0007 (0.0096)	1692	Province × year
Full, East/non-East-by-year	−0.0537 (0.0120)	4512	Region × year

Table A4. *Cont.*

Specification	QSI Coef. (SE)	N	Time Controls
Full, province-by-year	−0.0579 (0.0110)	4512	Province × year
Exclude 2016 and 2017	−0.0578 (0.0130)	3948	Year
Exclude nine cities and 2021–2022	−0.0466 (0.0109)	3822	Year
Post-break, 2017–2020	+0.0009 (0.0095)	1128	Year

Note. All specifications include city fixed effects, log GDP, and locational policy intensity; standard errors are clustered by city. Region × year means East/non-East group-specific year effects, and province × year is a finer time control partition. Each split is estimated independently; 2016 is omitted from the pre-/post-break comparison. The pooled coefficient remains precise, but both split-period coefficients are insignificant. Complete case is a selection sensitivity, not a reconstruction of the missing original policy texts.

Table A5. Audit of the 2016–2017 archive discontinuity against official energy totals.

Series/Statistic	2016–2017 Change	Data Scope
Archive: mean city log energy use	+0.738 log points	282 cities; unverified unit
Archive: mean city log GDP	+0.070 log points	282 cities; nominal GDP
Archive: mean city log intensity	+0.668 log points	282 cities; archived ratio
Archive: positive log energy changes	97.5% of cities	282 city histories
Official national physical energy total	4.36 → 4.49 billion tce (+2.9%)	China total; NBS [61,62]

Note. Archived entries are unweighted averages of city-level log differences (or their share), not a national sum. The official 2016 and 2017 totals are preliminary national physical energy figures released by the National Bureau of Statistics [61,62]; they differ in spatial scope and measurement concept from the unidentified archive numerator. Their divergence flags a comparability concern and is not a substitute for an independent 282-city panel regression.

Table A6. Source panel duplicate audit.

City	Year	Raw Rows	Treatment
Chongzuo	2011	4	Collapsed to city–year mean
Chongzuo	2016	4	Collapsed to city–year mean
Liuzhou	2011	4	Collapsed to city–year mean
Baise	2016	4	Collapsed to city–year mean
Guigang	2016	4	Collapsed to city–year mean

Note. The five duplicated city–year cells account for the difference between 4527 raw rows and the 4512-observation balanced panel.

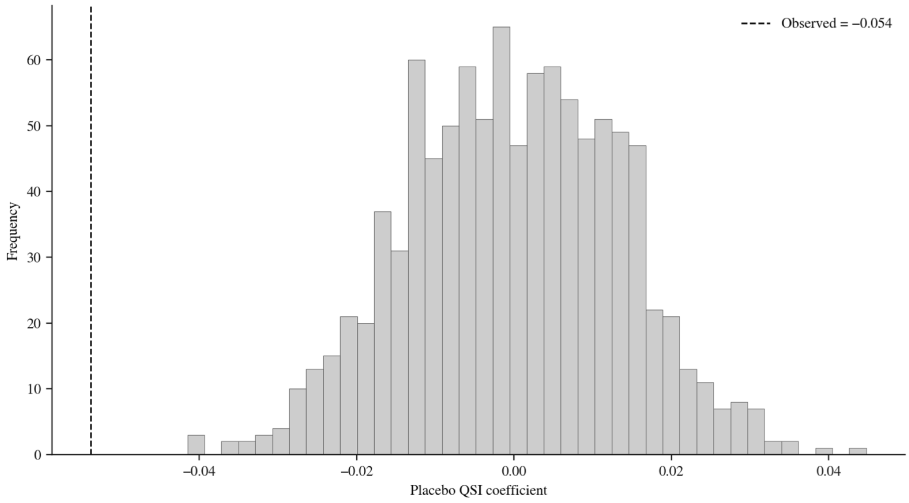


Figure A1. City history permutation placebo distribution.

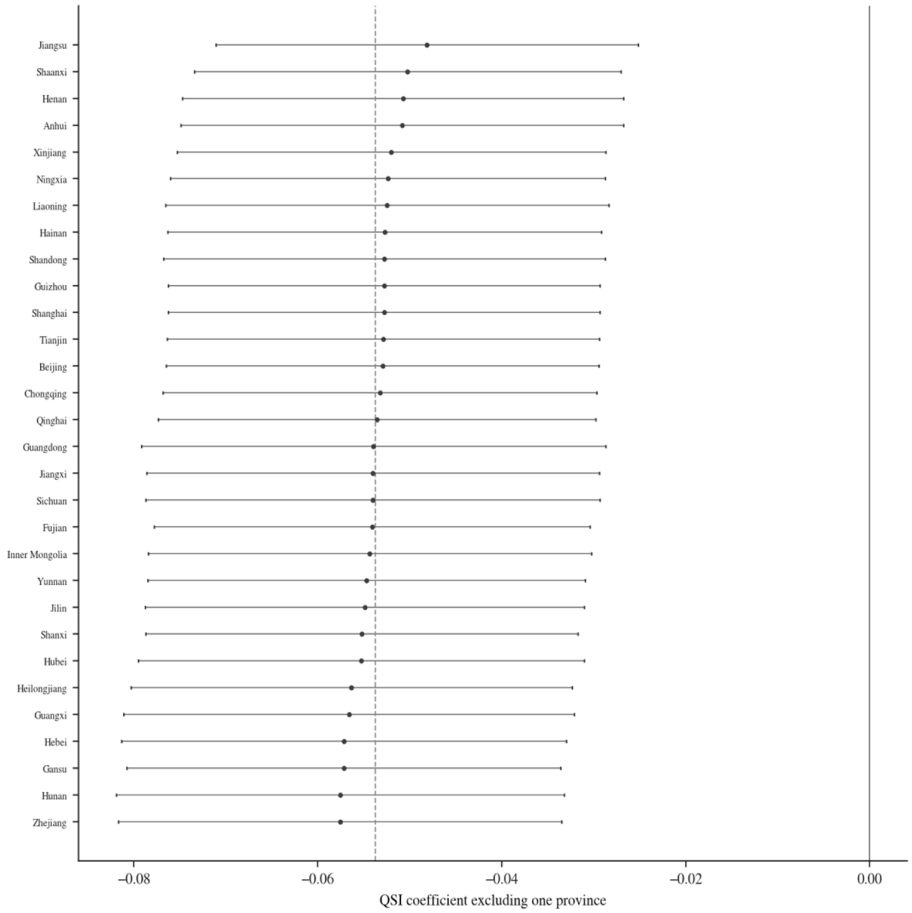


Figure A2. Leave-one-province-out influence estimates.

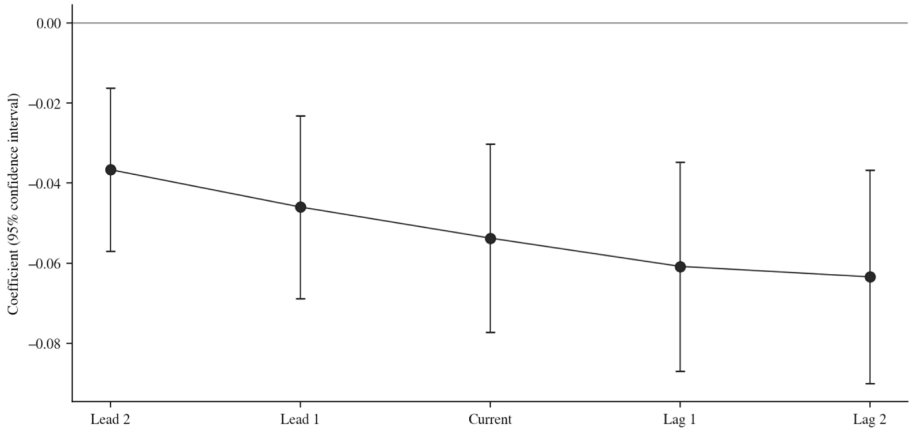


Figure A3. Lead, contemporaneous, and lagged QSI coefficients.

Table A7. Variables and archived numerical transformations.

Variable	Definition	Origin
city id	Numeric identifier for prefecture-level city	Derived after cleaning
city	Standardized city name	China Urban Statistical Yearbook
province	Standardized province; intermittent missing labels recovered by city mode; nine cities manually assigned to their known provinces	Cleaned from China Urban Statistical Yearbook

Table A7. *Cont.*

Variable	Definition	Origin
year	Calendar year, 2007–2022	China Urban Statistical Yearbook
digital policy	Digital economy policy intensity from local policy text	China Urban Statistical Yearbook
green attention	Green development attention from local government text	China Urban Statistical Yearbook
energy attention	Energy policy attention from local government text; a constituent of the QSI phase term, so not used as a mediator	China Urban Statistical Yearbook
prov digital attention	Province-level digital policy attention; a constituent of the QSI phase term	China Urban Statistical Yearbook
quantum phase	Phase term used in the quantum-inspired QSI construction	China Urban Statistical Yearbook
qsi	Quantum-inspired governance synergy index; higher values indicate stronger digital–green policy synergy	China Urban Statistical Yearbook
qsi alt	Alternative QSI construction used for robustness	China Urban Statistical Yearbook
peer qsi	Mean QSI of other cities in the same province-year, recomputed on the cleaned panel	Derived
ln energy intensity	Log energy use per unit GDP; higher values indicate worse energy efficiency	China Urban Statistical Yearbook
ln energy efficiency	Exactly the negative of ln energy intensity in the China Urban Statistical Yearbook; retained for audit but not treated as an independent outcome	China Urban Statistical Yearbook
nightlight gini	Nighttime light Gini coefficient	China Urban Statistical Yearbook
nightlight theil	Nighttime light Theil index	China Urban Statistical Yearbook
nightlight atkinson	Nighttime light Atkinson index	China Urban Statistical Yearbook
loc policy	Locationally oriented industrial-policy intensity	China Urban Statistical Yearbook
ln gdp	Log GDP	China Urban Statistical Yearbook
ln energy use	Log total energy use; together with ln gdp satisfies ln energy intensity = ln energy use − ln gdp	China Urban Statistical Yearbook
spatial ineq pc1	First principal component of standardized Gini, Theil, and Atkinson measures; explains 88.0% of their variance	Derived
east	Eastern region indicator	Derived from standard province classification
low initial ineq	1 if the city's 2007–2010 average spatial-inequality PC1 is at or below the city median	Derived, predetermined subgroup
high initial loc	1 if the city's 2007–2010 average locational-policy intensity exceeds the city median	Derived, predetermined subgroup
high initial gdp	1 if the city's 2007–2010 average log GDP exceeds the city median	Derived, predetermined subgroup
high initial digital	1 if the city's 2007–2010 average digital policy intensity exceeds the city median	Derived, predetermined subgroup

Appendix B. Exact QSI Reconstruction Boundary

Source panel standardization moments (mean; sample SD) are digital policy (1.492390; 1.120805), green attention (4.420233; 0.632383), energy attention (2.671954; 0.571731), and prov digital attention (2.156515; 0.898316). Let z x = (x − μ x)/s x; D = |z d|, G = |z g|, φ = π·tanh[(z p − z e)/2] and QSI = $\sqrt{(D^2 + G^2 + 2DG\cos\varphi)}$. This reconstructs source-record numerical QSI to rounding; for the five duplicated city-years the stored cleaned QSI is the mean of source-record QSI, not necessarily QSI recomputed on averaged component inputs.

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