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Digital Village Policy and Relative Agricultural Economic Performance in China: Threshold Effects on Sustainable Agricultural Transformation

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Abstract

Sustaining agricultural economic performance under climate and market disruptions has become a strategic priority for developing economies facing escalating climate risks and persistent rural development challenges. Using an unbalanced panel of 281 prefecture-level cities in China (2011–2023), this study examines the association between the Digital Village Pilot policy and relative agricultural economic performance (RAEP)—a city's agricultural growth measured against the national agricultural benchmark, which captures the resistance dimension (whether a city maintains its agricultural-economic position during disruptions) rather than the recovery, adaptability, ecological, or household-livelihood dimensions of the broader resilience concept—through difference-in-differences estimation, Hansen panel threshold regression, and a two-step channel analysis. The results indicate that the Pilot is associated with a statistically significant improvement in relative agricultural economic performance, an effect that remains broadly stable across specification checks. A threshold pattern emerges: the estimated policy association is negligible in city-year observations where digital infrastructure falls below an identified cutoff but rises substantially above it. Because a large share of cities falls below this threshold, the program's benefits remain unevenly distributed. Channel analysis reveals that the Pilot is associated with a marginally significant increase in digital financial inclusion and a significant reduction in agricultural agglomeration, with the latter reflecting a shift toward diversified rather than spatially concentrated agricultural activity, a pattern theoretically linked to greater shock resistance. These findings advance understanding of how digital rural policies affect relative agricultural economic performance and provide empirical evidence for identifying the enabling conditions under which digital transformation strengthens sustainable agriculture.

Keywords: digital village policy; relative agricultural economic performance; threshold effects; difference-in-differences; digital infrastructure; sustainable development goals; Hansen threshold model; China

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1. Introduction

Against the backdrop of accelerating climate change, persistent rural development challenges, and increasing pressure on food systems, building agricultural resilience has emerged as a central policy concern for agricultural nations worldwide [1,2]. This challenge intersects with multiple United Nations Sustainable Development Goals (SDGs), particularly SDG 2 (Zero Hunger), SDG 9 (Industry, Innovation and Infrastructure), and SDG 11 (Sustainable Cities and Communities), underscoring the need for evidence on how digital interventions can strengthen agricultural systems. At the international policy level, the European Union's Farm to Fork Strategy has set concrete targets for pesticide and fertilizer reduction by 2030 to promote sustainable food systems [3], while the Food and Agriculture Organization (FAO) emphasizes digital agriculture as a key pathway for building climate-resilient food production in developing countries [4]. These global frameworks highlight the urgency of integrating digital transformation with sustainable agricultural development.

In China, where agriculture supports nearly 1.4 billion people but faces compounding pressures from frequent extreme weather events, rural depopulation, and resource constraints, the imperative is particularly acute. Under the Rural Revitalization Strategy [5] and the dual carbon goals of reaching peak emissions by 2030 and carbon neutrality by 2060 [6], the Chinese government has positioned digital rural transformation as a core strategy for agricultural modernization [7]. The State Council's 2019 Digital Village Development Strategy Outline [8] and the follow-up Digital Village Development Action Plan (2022–2025) [9] established the institutional framework for embedding digital tools (from broadband networks and smart farming platforms to e-governance systems) into rural production and governance.

Central to this framework is the Digital Village Pilot policy. Implemented in 2020, it designated 99 prefecture-level cities as testbeds for integrated digital rural modernization, covering digital infrastructure rollout, e-governance systems, smart farming applications, and digital literacy campaigns. Because the policy creates a well-defined treatment group and a single implementation date, it lends itself to quasi-experimental evaluation of whether, and under what conditions, digital transformation improves agricultural outcomes.

A growing body of research documents the effects of digital rural investment on agricultural outcomes in China. At the farm and regional levels, digital village construction has been associated with gains in green production efficiency [10], land-use sustainability [11], farmland consolidation [12], and rural governance quality [13]. At a broader scale, the digital economy contributes to agricultural development resilience [14], food production capacity [15], and rural modernization [16]. Digital infrastructure development also drives rural revitalization [17] and supply-chain robustness [18].

Despite this progress, three gaps remain. First, existing studies have largely focused on individual agricultural performance indicators rather than on relative agricultural economic performance (RAEP), understood as a city's agricultural growth measured against the national benchmark [2,19]. Following the regional-resilience tradition [1], RAEP operationalizes the resistance dimension of resilience (relative economic performance during disruptions); it is deliberately not a comprehensive resilience index and does not capture recovery, adaptability, ecological resilience, or household livelihood resilience, which would require different data and methods. We therefore frame the outcome as a relative-performance indicator and reserve the broader term "resilience" for the motivating concept rather than the measured variable. Policymakers concerned with sustaining rural livelihoods through diverse shocks need evidence on relative agricultural robustness, not only on single-indicator productivity. Second, the possibility that the policy effect depends nonlinearly on digital infrastructure levels has received little empirical attention, even though identifying such threshold conditions would directly inform decisions about where and in what sequence to designate digital village pilots. Third, the specific channels through which digital village programs affect relative agricultural economic performance remain unclear, limiting the design guidance that empirical work can provide to practitioners.

This study adopts a quasi-experimental approach to address these gaps. Using an unbalanced panel of 281 cities over 2011–2023 (3345 city-year observations), we provide quasi-experimental evidence on the Pilot's association with relative agricultural economic performance. We embed Hansen's (1999) [20] panel threshold model, with the threshold variable constructed from pre-2020 (baseline) digital infrastructure to address endogeneity concerns, to identify the baseline digital infrastructure level at which the estimated policy effect differs across regimes, and apply a two-step channel analysis [21] to examine transmission through digital financial inclusion (DFI) and agricultural agglomeration as candidate mediators.

The contributions of this study are threefold. First, it extends the emerging literature on digital village policies by providing city-level quasi-experimental evidence on the Pilot's association with relative agricultural economic performance, complementing existing county- and province-level analyses. Second, it estimates specific cutoff values of baseline digital infrastructure at which the policy effect differs across regimes, using pre-2020 infrastructure to mitigate endogeneity of the threshold variable. While building on the broader nonlinear digital-economy literature, this approach applies Hansen's panel threshold framework to the specific context of relative agricultural economic performance. Third, it documents a suggestive pattern of effect attenuation in cities with high agricultural dependence (Chow test $p = 0.014$), indicating that the gains accrue more strongly to cities with a more diversified economic structure.

The remainder of this article is organized as follows. Section 2 reviews the literature and develops testable hypotheses (Figure 1 summarizes the theoretical framework). Section 3 describes the data sources and identification strategy. Section 4 presents the empirical results. Section 5 discusses the findings and their implications, and Section 6 concludes.

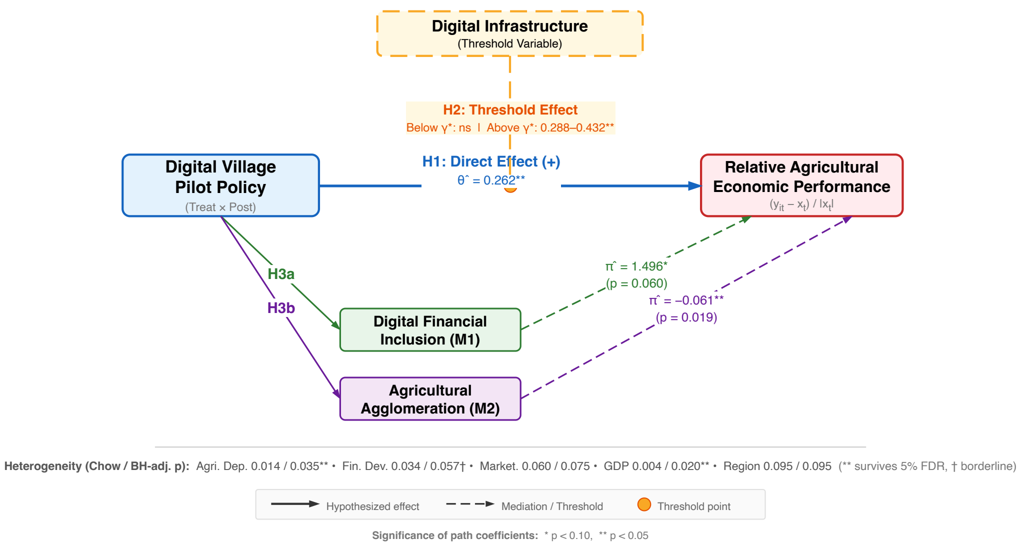

Figure 1. Theoretical frameworklinking the Digital Village Pilot to relative agricultural economic performance. The policy association may operate through a direct channel (Hypothesis 1), vary across digital infrastructure regimes (Hypothesis 2), and be descriptively associated with digital financial inclusion (Hypothesis 3a) and agricultural agglomeration restructuring (Hypothesis 3b). Key heterogeneity dimensions are shown below.

2. Theoretical Analysis and Research Hypotheses

2.1. Digital Village Policy and Relative Agricultural Economic Performance: The Main Effect

2.1.1. Digital Transformation of Agriculture

Digital technologies have substantially changed how food systems are organized, managed, and governed. In China, this shift is anchored in two landmark policy documents: the 2019 Digital Village Development Strategy Outline [8] and the 2022 Digital Village Development Action Plan [9]. Together, these frameworks envision a full digitalization of rural life, spanning broadband networks, smart farming platforms, e-governance systems, and digital financial services. The Digital Village Pilot, launched in 2020 across 99 prefecture-level cities, is the operational arm of this vision, providing a well-defined institutional intervention through which to evaluate the agricultural consequences of digital rural modernization.

The theoretical case for positive agricultural spillovers from digital village policies draws on multiple, complementary channels. At the information level, broadband and mobile networks reduce search costs and information asymmetries in agricultural input and output markets, enabling farmers to access real-time price data, weather forecasts, and technical guidance [12]. At the production level, precision-agriculture technologies such as soil sensors, drone-based monitoring, and variable-rate applicators allow input optimization that raises both yields and resource-use efficiency [22]. At the market-access level, e-commerce platforms connect farm producers directly to urban consumers, compressing distribution chains and raising farm-gate prices [17]. The empirical literature has begun to document these effects: digital village construction has been associated with gains in green production efficiency [10], reductions in agricultural carbon intensity [23–25], more sustainable cultivated-land use [11], and higher food production capacity [15]. At a broader scale, the digital economy correlates with agricultural modernization and inclusive rural financial development [26].

2.1.2. From Agricultural Productivity to Relative Agricultural Economic Performance

Existing studies have largely focused on individual agricultural performance metrics such as yields, efficiency, and emissions, rather than on agricultural resilience as a regional economic outcome. Originating in ecological systems theory [27] and subsequently adapted to regional economics [1,28], resilience is commonly understood as the ability of a local system to maintain or recover its core economic function relative to an aggregate benchmark. Applied to agriculture, this framework captures more than how much a region produces; it captures whether the region's agricultural economy performs better or worse than the national agricultural trend under droughts, price fluctuations, pandemics, and other disruptions [2,7].

Digital village policies may strengthen each resilience dimension through distinct mechanisms. Resistance is bolstered by early warning systems that provide advance notice of weather shocks and pest outbreaks, allowing preemptive protective action.Recovery is accelerated by digital credit and insurance platforms that deliver rapid post-disaster liquidity, and by e-commerce channels that restore market access when physical distribution is disrupted [29]. Adaptability is enhanced by data-driven advisory services that guide long-run crop diversification and by digital platforms that support the adoption of climate-smart agricultural practices [30]. Recent empirical work by Luo et al. [31] provides direct evidence that digitalization improves agricultural production resilience in China, while Xue et al. [14] show that the digital economy enhances agricultural development resilience through market-access and human-capital channels. Green finance, closely associated with digital infrastructure, has also been shown to strengthen agricultural economic resilience [32].

The studies cited above are framed in terms of agricultural resilience broadly; our test is narrower. Because our measure captures only the resistance facet—a city's relative agricultural economic performance against the national benchmark—we read this body of evidence as motivating, rather than directly validating, our hypothesis: the resistance mechanisms (early warning, liquidity, market-access continuity) bear most directly on the relative-performance outcome we observe, while the recovery and adaptability mechanisms motivate the broader concept but extend beyond what we measure. Given this multi-channel logic and the accumulating evidence, we expect the Digital Village Pilot, as a wide-ranging digital rural modernization program, to be associated with a measurable improvement in relative agricultural economic performance among treated cities. Based on the above analysis, we propose the following hypothesis:

Hypothesis 1. *The Digital Village Pilot is associated with a statistically significant increase in relative agricultural economic performance among treated cities.*

2.2. Digital Infrastructure as a Threshold Condition

2.2.1. Threshold Economics and Policy Effectiveness

A central insight from threshold economics is that the effectiveness of a policy intervention may depend nonlinearly on the level of a conditioning variable. Hansen [20] formalizes this intuition in a panel threshold regression framework, which allows the slope coefficient to shift discontinuously across regimes defined by an observable threshold, with the threshold identified endogenously and significance assessed through bootstrap inference. This framework has been widely applied in development economics to study contexts where the returns to policy depend on complementary enabling conditions, such as the effectiveness of education spending on institutional quality, or the impact of foreign direct investment on financial development.

In the digital village context, digital infrastructure is a natural candidate for the threshold variable. The logic is grounded in the layered architecture of digital ecosystems: application-layer services (smart farming apps, e-commerce platforms, digital advisory systems) depend on a functioning infrastructure layer (broadband networks, mobile coverage, cloud computing, digital payment systems). Policies that promote digital applications without adequate infrastructure are analogous to building software without hardware: the investment is unlikely to generate returns when the foundational enabling condition is weak.

2.2.2. Nonlinear Digital Economy Effects: Evidence

Several recent studies document nonlinear effects of digital-economy interventions on agricultural and rural outcomes. Xue et al. [14] find that the digital economy's impact on agricultural development resilience follows a nonlinear trajectory, with effects concentrated in regions that have already achieved a baseline level of digitalization. Peng and Dan [33] report that the digital economy's effect on urban-rural income inequality exhibits threshold dependence: the digital dividend materializes only beyond a certain digitalization level, while below it digitalization may actually widen inequality, a "digital divide" effect. Xiong et al. [34] demonstrate that the digital economy's influence on urban environmental efficiency varies with industrial agglomeration levels, and Zhu et al. [35] identify threshold dependence in digital technology's effect on agricultural economic resilience using a dynamic panel threshold model. Outside agriculture, Qian and Xi [36] and Li and Xu [37] document nonlinear digital-economy effects on green development efficiency, while Chen et al. [18] show that broadband infrastructure construction boosts industrial-chain resilience with heterogeneous effects across cities.

2.2.3. Why Agriculture Amplifies the Threshold Logic

The threshold argument carries particular force in agriculture for three reasons. First, precision agriculture requires a tightly integrated infrastructure stack: soil sensors transmit through cellular networks, analytics run on cloud platforms that depend on broadband, and e-commerce logistics presuppose digital payment systems. A break in any layer (inadequate mobile coverage, for instance) renders the entire stack non-functional. Second, agricultural production is spatially dispersed: unlike manufacturing, which concentrates in industrial parks where infrastructure provision is relatively straightforward, farming occurs across wide geographic areas where network externalities are harder to achieve. Third, the benefits of digital agricultural tools are often realized through network effects. A farmer's adoption of a digital market platform is valuable only if buyers, logistics providers, and payment systems are also connected. Below a critical mass of infrastructure penetration, these network effects fail to activate.

This layered dependency generates a clear prediction: the marginal return to application-layer digital village policies should be limited at low infrastructure levels and increase once the infrastructure layer crosses a critical level. Based on the above analysis, we propose the following hypothesis:

Hypothesis 2. *The magnitude of the policy effect depends on the level of digital infrastructure, crossing a threshold below which the effect is negligible and above which it is significant.*

2.3. Transmission Mechanism: Digital Financial Inclusion

2.3.1. How the Policy Promotes Digital Financial Inclusion

The Digital Village Pilot promotes digital financial inclusion through both direct institutional channels and indirect spillover effects. At the institutional level, pilot cities are required to develop e-governance platforms and digital service systems that integrate financial services (mobile banking, digital payments, and microcredit) into the rural governance infrastructure [8]. The policy's emphasis on digital literacy campaigns further accelerates the adoption of digital financial tools among rural households who previously lacked access to formal banking services [38]. At the infrastructure level, the broadband deployment and mobile network expansion mandated by the Pilot create the connectivity foundation on which digital financial services depend: mobile payment platforms require network coverage, and digital lending algorithms rely on data streams generated by connected devices [17]. The empirical literature documents positive associations between digital village construction and inclusive financial development [26], suggesting a plausible pathway from the policy to enhanced digital financial inclusion.

2.3.2. From Digital Financial Inclusion to Relative Agricultural Economic Performance

Digital financial inclusion may strengthen relative agricultural economic performance through three complementary pathways. At the credit access level, mobile banking and digital microcredit services relax liquidity constraints that limit agricultural investment in inputs, equipment, and technology adoption [38]. Farmers with access to digital credit can smooth consumption across agricultural seasons, invest in productivity-enhancing technologies, and respond more rapidly to market opportunities. At the risk management level, digital insurance platforms, including weather-index products and crop insurance distributed through mobile channels, allow farmers to hedge against production risks from droughts, floods, and pest outbreaks [26]. By reducing the financial consequences of adverse shocks, digital insurance enables farmers to maintain agricultural production during disruptions, directly contributing to the resistance dimension of resilience. At the market access level, digital payment infrastructure is a prerequisite for meaningful participation in e-commerce platforms that connect farm producers to urban consumers, compress distribution chains, and raise farm-gate prices [18]. If the Digital Village Pilot raises digital financial inclusion, and if enhanced financial inclusion improves agricultural resilience through the credit, insurance, and market-access channels described above, then digital financial inclusion should function as a transmission channel linking the policy to relative agricultural economic performance.

Based on the above analysis, we propose the following hypothesis:

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Hypothesis 3a. *Digital financial inclusion is a plausible transmission channel linking the policy to relative agricultural economic performance.*

2.4. Transmission Mechanism: Agricultural Agglomeration

2.4.1. Agglomeration Theory and Agricultural Systems

Agricultural agglomeration, the spatial concentration of production, processing, and related services, has long been recognized as a source of productivity gains through knowledge spillovers, specialized input suppliers, labor pooling, and scale economies in processing and logistics. Wang et al. [39] demonstrate that industrial agglomeration in agriculture raises green production efficiency, and Wang et al. [40] show that agricultural technological progress promotes rural revitalization partly through scale operations. Agglomeration can also strengthen resilience: diversified local agricultural ecosystems with dense input-output linkages may better absorb and recover from shocks than dispersed, isolated production units.

2.4.2. Digital Technologies and Spatial Reorganization

Digital village policies may reshape agricultural agglomeration through two countervailing forces. On the one hand, digital tools lower information costs and reduce geographic frictions in agricultural markets, potentially encouraging spatial concentration by enabling farmers in favorable locations to access distant buyers and specialized services more efficiently. E-commerce platforms, for instance, create incentives for geographic specialization by connecting concentrated production zones to national consumer markets [12]. On the other hand, digital technologies simultaneously enable *dispersion* by lowering the barriers to remote participation in agricultural value chains. A smallholder in a peripheral county can now sell specialty crops through e-commerce, access agronomic advice through mobile platforms, and obtain credit through digital finance, all without relocating to an agglomeration hub. This dispersion force may reduce measured agricultural agglomeration even as it expands economic opportunities and enhances resilience through activity diversification [26].

2.4.3. The Net Direction: Diversification over Concentration

Theoretically, the net direction is ambiguous ex ante. However, the specific context of China's digital village policy (which emphasizes digital livelihoods, e-commerce for rural households, and multi-dimensional rural modernization rather than single-commodity specialization) suggests that the dispersion force may dominate. If digital tools enable farmers to adopt a wider portfolio of crops and income-generating activities (e-commerce, agritourism, value-added processing) rather than concentrating on traditional commodity production, the policy would be associated with reduced agricultural agglomeration. This diversification, while reducing measured geographic concentration, may itself be resilience-enhancing: diversified agricultural systems are inherently less vulnerable to single-commodity price shocks and single-crop disease outbreaks [1]. Regardless of the direction of the net effect, if the Digital Village Pilot systematically affects agricultural agglomeration, and if agglomeration is itself associated with resilience, then agglomeration will mediate (or, in the language of mediation analysis, suppress) a portion of the total policy effect.

Based on the above analysis, we propose the following hypothesis:

Hypothesis 3b. *Agricultural agglomeration is a plausible transmission channel linking the policy to relative agricultural economic performance.*

Table 1 summarizes the four testable hypotheses and the econometric approach used to evaluate each.

Table 1. Summary of Research Hypotheses and Testing Approaches.

Label	Prediction	Method	Key Statistic
Hypothesis 1	Policy $\rightarrow$ Econ. resilience (+)	TWFE DID	$\hat{\theta}$, p-value
Hypothesis 2	Threshold in digital infra.	Panel threshold [20]	γ^*, bootstrap p
Hypothesis 3a	DFI channel	Two-step [21]	$\hat{\pi}$, p-value
Hypothesis 3b	Agglomeration channel	Two-step [21]	$\hat{\pi}$, p-value

Note: All models include city and year fixed effects, nine time-varying controls, and city-level clustered standard errors.

3. Materials and Methods

3.1. Data Sources

Our analysis draws on an unbalanced panel of 281 Chinese prefecture-level cities observed annually from 2011 through 2023, yielding 3345 city-year observations after imposing standard quality filters on the fully controlled specification. Relative agricultural economic performance is constructed from city-level primary-industry value added and national primary-industry value added, using the original city-level agricultural resilience dataset for 1990–2023 and retaining the raw, non-imputed resilience series for the 2011–2023 analysis window. Socioeconomic covariates come from the China City Statistical Yearbook, the China Statistical Yearbook, and provincial statistical yearbooks. Digital infrastructure data are obtained from the China Information Industry Statistical Yearbook and the Ministry of Industry and Information Technology. The Peking University Digital Financial Inclusion Index (DFI) is used to measure digital financial inclusion [38]. Environmental regulation indices are compiled from the China Environmental Statistical Yearbook, and the marketization index is sourced from the NERI Index of Marketization of China's Provinces. Agricultural agglomeration is calculated as a location quotient derived from employment statistics.

The treated subsample comprises 99 cities designated under the national Digital Village Pilot program launched in 2020; the remaining 182 cities form the control group. After integrating all data sources and imposing standard quality filters (dropping observations with missing values on core variables), the baseline estimation sample contains 3345 city-year observations from 281 cities. This sample is drawn from an initial universe of 300 prefecture-level cities with available data. The panel is unbalanced: not every city contributes observations in every year, reflecting differential data availability across controls. The estimation sample of 3345 represents the intersection of non-missing values across all variables required for the fully controlled specification. Sample sizes reported in each table reflect this intersection principle; minor variations across specifications arise when specific robustness checks require additional variables or alternative estimation windows that alter the set of non-missing observations.

3.2. Variable Construction

3.2.1. Dependent Variable

The outcome variable is relative agricultural economic performance (RAEP). Consistent with the regional-resilience tradition, which compares local economic performance against an aggregate benchmark [1,28], RAEP measures the gap between a city's agricultural growth and national agricultural growth in the same year. We adopt the precise label "relative agricultural economic performance" rather than "resilience" because the construct captures only the resistance dimension of the broader resilience concept—a region's relative economic standing during common shocks—and not its recovery, adaptability, ecological, or household-livelihood dimensions, each of which would require different data and methods. To verify that the construct is not an artifact of one particular output base, Section 4.8 reports two alternative relative-performance measures, built from gross agricultural output value and from physical grain output. Specifically, let Y_{it}^{agr} denote the first-industry value added of city i in year t, and let $Y_t^{nat,agr}$ denote national first-industry value added. The city-level agricultural growth rate is $y_{it} = (Y_{it}^{agr} - Y_{i,t-1}^{agr})/Y_{i,t-1}^{agr}$, and the national agricultural growth rate is $x_t = (Y_t^{nat,agr} - Y_{t-1}^{nat,agr})/Y_{t-1}^{nat,agr}$. RAEP is then defined as:

$$\mathrm{RAEP}_{it} = \frac{y_{it} - x_t}{|x_t|}. \tag{1}$$

This measure is positive when a city's agricultural economy grows faster than the national agricultural benchmark and negative when it underperforms the national benchmark. It captures relative agricultural economic performance, specifically the resistance dimension of resilience, rather than all dimensions of agricultural resilience, such as ecological resilience, household livelihood resilience, or separate recovery and adaptability components. This scope limitation should be borne in mind when interpreting the results: we study whether digital village policies help cities maintain their relative agricultural economic position, not whether they improve every facet of agricultural system resilience. Table 2 summarizes the construction. Across the estimation sample the index has a mean of -0.118 and a standard deviation of 1.211.

Table 2. Relative Agricultural Economic Performance Variable Construction.

Component	Definition	Interpretation
City growth y_{it}	Growth rate of city first-industry value added	Local agricultural economic performance
National growth x_t	Growth rate of national first-industry value added	Aggregate agricultural benchmark
RAEP	$(y_{it} - x_t)/|x_t|$	Relative over- or under-performance

Note: First-industry value added is used as the agricultural economic output measure. Higher values indicate stronger relative agricultural economic performance, i.e., city agricultural growth exceeding the national benchmark in the same year.

The growth-benchmark measure has two complementary strengths. First, it normalizes city agricultural growth by the national agricultural growth in the same year, which absorbs common national shocks (e.g., aggregate weather, macro conditions, or pandemic-era disruptions) and isolates the city's relative performance. Second, it is computed from official first-industry value added, a transparent and widely available accounting category, so the construction is reproducible. The measure also has clear limits. Because the denominator $|x_t|$ can be small in years when national agricultural growth is muted, the index is mechanically sensitive to small denominators, which is reflected in its wide observed range and motivates the winsorized robustness check in Section 4.7. The measure captures relative economic performance and does not directly observe drought resistance, post-shock recovery, crop diversification, household livelihood resilience, ecological resilience, or input-use sustainability. A multidimensional resilience index using ecological, social, and livelihood indicators would be a valuable complement, but is beyond the data available at the prefecture-city level for the 2011–2023 window.

3.2.2. Independent Variable

The treatment variable is the interaction $D_{it} \equiv Treat_i \times Post_t$, where $Treat_i$ takes the value one for the 99 pilot cities and zero for the control group, and $Post_t$ switches on from 2020 onward. Because all pilot cities receive treatment simultaneously in a single batch, the TWFE estimator does not suffer from the heterogeneous-treatment-weights problem documented in staggered adoption settings [41–43].

3.2.3. Threshold Variable

The threshold variable is a composite digital infrastructure index that aggregates broadband penetration, mobile internet coverage, and digital service accessibility at the city level. To address the endogeneity concern that the threshold variable may itself respond to the policy, the threshold indicator is constructed from the city-level pre-2020 (baseline) mean of the digital infrastructure index, ensuring that regime assignment is determined solely by pre-treatment infrastructure. In the estimation sample the index has a mean of 0.002 and a standard deviation of 0.001.

3.2.4. Mediation Variables

Two transmission channels are examined. The first is digital financial inclusion (M1), measured by the Peking University Digital Financial Inclusion Index, which proxies for the broader infrastructure-upgrade channel (because digital infrastructure itself serves as the threshold variable, we use DFI to avoid conceptual overlap). The second is agricultural agglomeration (M2), measured by the location quotient (LQ) of agricultural employment [44]. Specifically, $LQ_i = (E_i^{agr}/E_i^{total})/(E_{nat}^{agr}/E_{nat}^{total})$, where E_i^{agr} and E_i^{total} denote agricultural and total employment in city i, and the denominator uses national aggregates. The LQ averages 1.613 with a standard deviation of 1.012.

3.2.5. Control Variables

Nine time-varying covariates are included to absorb confounding variation: (1) log GDP per capita, capturing development level; (2) secondary industry share, reflecting industrial composition; (3) tertiary industry share, capturing service-sector depth; (4) financial development (total loans/GDP), proxying for credit market depth; (5) government intervention (fiscal expenditure/GDP); (6) education expenditure/GDP, measuring human-capital investment; (7) log of college student enrollment, proxying for human-capital stock; (8) urbanization rate; and (9) science expenditure/GDP, capturing innovation-input intensity.

3.3. Empirical Strategy

3.3.1. Baseline DID Model

The baseline specification follows the standard TWFE DID framework [41,45]:

$$\mathrm{RAEP}_{it} = \theta \cdot D_{it} + \mathrm{c}_{it}'\eta + \alpha_i + \tau_t + u_{it} \tag{2}$$

where $D_{it} = Treat_i \times Post_t$ is the DID treatment indicator; c_{it} is a vector of nine time-varying covariates; α_i and τ_t denote city and year fixed effects, respectively; and u_{it} is the idiosyncratic error. The target parameter θ measures the average association between the Digital Village Pilot and relative agricultural economic performance under the maintained parallel-trends assumption; it should be interpreted as a quasi-experimental estimate rather than a causal effect, given that pilot designation was not randomly assigned. Serial correlation within cities is accommodated by clustering standard errors at the city level [45].

The identifying assumption is that, conditional on city fixed effects, year fixed effects, and time-varying controls, treated and untreated cities would have followed parallel agricultural-economic-resilience trajectories in the absence of the Pilot. Pilot designation was not randomly assigned, and treated cities differed from control cities in several pre-treatment characteristics. City fixed effects absorb time-invariant readiness differences, while the event-study specification below tests for differential pre-policy outcome trends. Accordingly, the estimates should be interpreted as quasi-experimental conditional effects rather than as results from randomized assignment. To make the selection issue transparent, Table 3 reports pre-treatment balance for key variables.

Table 3. Pre-treatment Balance Between Pilot and Non-pilot Cities (2011–2019).

Variable	Treated Mean	Control Mean	Difference	p-Value
Relative agric. econ. performance	−0.167	−0.150	−0.017	0.755
Digital infrastructure	0.0018	0.0016	0.0001	0.014
Agglomeration (LQ)	1.457	1.672	−0.215	<0.001
ln(GDP per capita)	10.793	10.631	0.162	<0.001
Financial development	1.135	0.924	0.211	<0.001
Urbanization rate	0.563	0.539	0.024	<0.001

Note: Balance statistics are computed over pre-treatment observations. Differences are tested using two-sample unequal-variance t-tests. Several covariates differ significantly before treatment, which motivates the use of city fixed effects, time-varying controls, PSM-DID robustness checks, and event-study diagnostics.

3.3.2. Event Study Specification

Dynamic treatment effects and the validity of the parallel trends assumption are assessed through an event study:

$$\mathrm{RAEP}_{it} = \sum_{s \neq -1} \theta_s \cdot Treat_i \times \mathbb{1}(t - 2020 = s) + \mathrm{c}'_{it}\eta + \alpha_i + \tau_t + u_{it} \tag{3}$$

Here s counts years relative to the 2020 implementation, with $s = -1$ (2019) serving as the omitted reference category. Coefficients for $s < 0$ test whether treated and control cities followed similar resilience trajectories prior to the policy; those for $s \geq 0$ map the post-policy dynamics.

3.3.3. Hansen Threshold Model

Nonlinear threshold dependence is tested via the Hansen [20] panel threshold regression:

$$\mathrm{RAEP}_{it} = \theta_L \cdot D_{it} \cdot \mathbb{1}(q_i \leq \gamma^*) + \theta_H \cdot D_{it} \cdot \mathbb{1}(q_i > \gamma^*) + \mathrm{c}'_{it}\eta + \alpha_i + \tau_t + u_{it} \tag{4}$$

where q_i is the city-level pre-2020 (baseline) mean of the digital infrastructure index, γ^* is the estimated cutoff, $\mathbb{1}(\cdot)$ denotes the indicator function, and θ_L and θ_H capture the policy association in cities whose baseline digital infrastructure falls below or exceeds the threshold, respectively. The grid-search estimator locates γ^* by minimizing the sum of squared residuals, and a bootstrap with 500 replications assesses the statistical significance of the threshold. Constructing the threshold variable from pre-2020 values addresses the endogeneity concern that a time-varying threshold might itself respond to the Pilot: because q_i is fixed at its pre-treatment level, regime assignment is determined solely by baseline infrastructure and is exogenous to the post-2019 policy. The estimated θ_L and θ_H should accordingly be interpreted as a contrast between cities with predominantly low versus predominantly high baseline digital infrastructure, rather than as evidence that infrastructure is the sole determinant of treatment effectiveness.

3.3.4. Channel Analysis Strategy

To examine the transmission channels through which the Digital Village Pilot may affect relative agricultural economic performance, we follow the two-step procedure recommended by Jiang [21]. The rationale is that, in a quasi-experimental panel setting with endogenous mediators, the traditional Baron–Kenny causal-steps approach and Sobel test face well-known identification problems: the mediator is jointly determined with the outcome, and the sequential ignorability assumption is difficult to defend [46]. The two-step procedure sidesteps these issues by testing only the policy-to-mediator link statistically and establishing the mediator-to-outcome link through theoretical reasoning supported by existing literature.

Step 1 (Statistical test: policy → mediator). Each candidate mediator is regressed on the DID indicator plus controls and fixed effects:

$$Med_{it} = \pi \cdot D_{it} + c_{it}'\eta + \alpha_i + \tau_t + u_{it} \tag{5}$$

A significant $\hat{\pi}$ establishes that the policy is associated with a change in the mediator, satisfying the first necessary condition for a transmission channel.

Step 2 (Theoretical reasoning: mediator → outcome).The link from the mediator to relative agricultural economic performance is established through theoretical argumentation and existing empirical literature rather than through the regression of the outcome on both the treatment and the mediator. This avoids the endogeneity problem that arises when the mediator is included in the outcome regression, since the mediator may be correlated with unobserved determinants of resilience [21].

As a supplementary check, we also estimate the auxiliary regression:

$$\mathrm{RAEP}_{it} = \theta^* \cdot D_{it} + \kappa \cdot Med_{it} + c_{it}'\eta + \alpha_i + \tau_t + u_{it} \tag{6}$$

This regression is reported solely to document the bivariate association between the mediator and the outcome conditional on the treatment; we do not interpret $\hat{\kappa}$ as a causal effect, nor do we compute indirect effects or conduct Sobel tests. The distinction between statistical and theoretical identification of the two links is central to the channel analysis framework.

3.3.5. Robustness Strategy

Nine robustness exercises are conducted: (1) PSM-DID to address selection on observables [47,48]; (2) lagged treatment (one-year delay); (3) excluding the four directly administered municipalities; (4) placebo test using 500 randomly reshuffled treatment assignments; (5) winsorizing all variables at the 5th/95th percentiles; (6) a minimal-controls specification retaining only log GDP per capita and industrial structure; (7) a restricted estimation window (2013–2023); and (8) dropping the policy year (2020). Province-specific linear trends are not reported because, in a model with city and year fixed effects, province-specific trends are nearly collinear with the DID treatment indicator when all treated cities share a single implementation year, leaving too little independent variation for reliable estimation.

4. Results

4.1. Descriptive Statistics

Table 4 summarizes the main estimation variables. Relative agricultural economic performance averages −0.118 with a standard deviation of 1.211, spanning from −5.351 to 6.249. The interquartile range (P25 = −0.550 to P75 = 0.381) equals 0.931, so a one-unit change in the index exceeds one interquartile range, a substantively meaningful shift. The DID indicator averages 0.065, implying that 6.5% of city-year observations are simultaneously treated and post-policy. The digital infrastructure index averages 0.002 (SD = 0.001), while the agricultural agglomeration LQ averages 1.613 (SD = 1.012).

Table 4. Summary Statistics of Key Variables.

Variable	Obs.	Mean	Std. Dev.	Min	Max
Relative agric. econ. performance	3345	−0.118	1.211	−5.351	6.249
DID (*Treat* × *Post*)	3345	0.065	0.247	0	1
Digital infrastructure	3345	0.002	0.001	0	0.014
Agricultural agglomeration (LQ)	3345	1.613	1.012	0.070	5.267
Environmental regulation	3345	94.251	12.259	5.0	100.5
Marketization index	3345	8.308	1.622	3.359	11.494
ln(GDP per capita)	3345	10.785	0.563	8.773	12.456
Secondary industry share (%)	3345	45.027	10.911	10.68	82.05
Tertiary industry share (%)	3345	42.941	10.094	14.36	83.87
Financial development (Loans/GDP)	3345	1.074	0.625	0.132	7.450
Government intervention (Fiscal/GDP)	3345	0.201	0.101	0.044	0.916
Education expenditure/GDP	3345	0.035	0.018	0.008	0.149
ln(College students)	3345	10.643	1.332	5.220	14.214
Urbanization rate	3345	0.569	0.150	0.182	1.178
Science expenditure/GDP	3345	0.003	0.003	0.000	0.063

Note: Statistics are computed on the baseline estimation sample ($N = 3345$; 281 prefecture-level cities, 2011–2023), i.e., observations with non-missing values on all nine controls and the dependent variable. The panel is unbalanced. Pilot cities: $Treat = 1$ ($N = 99$); post-policy period: $Post = 1$ for 2020 onward.

4.2. Baseline DID Results

Table 5 displays DID estimates from Equation (2) under four progressively richer specifications. Column (1) includes only city and year fixed effects. Column (2) adds basic controls (GDP per capita and industrial structure). Column (3) incorporates all nine controls with city-level clustering. Column (4) re-estimates the full model with province-level clustering.

In every specification the treatment indicator carries a positive sign. Expressed in standardized units, the estimates range from 0.177 (14.6% of a standard deviation) in the no-controls model to 0.258 (21.3% of SD) with basic controls, settling at 0.262 (21.6% of SD) in the fully controlled specification. Our primary estimate, in Column (3), yields $\hat{\theta} = 0.262$ (SE = 0.118, p = 0.026). In percentile terms, the coefficient equals 28.1% of the interquartile range (0.262/0.931 = 28.1%), confirming that the effect is not merely statistically detectable but economically discernible.

Several complementary channels plausibly account for this positive effect. At the information level, broadband and mobile connectivity reduce search costs in agricultural markets, enabling farmers to respond more rapidly to price signals and weather alerts. At the production level, precision-agriculture tools (soil sensors, drone-based monitoring, variable-rate applicators) raise both yields and resource-use efficiency. At the market-access level, e-commerce platforms connect farm producers directly to urban consumers, compressing distribution chains and raising farm-gate prices. At the institutional level, digital governance platforms improve the delivery of agricultural extension services and disaster-response coordination. The subsequent channel analysis (Section 4.5) examines two candidate mediators, digital financial inclusion (DFI) and agricultural agglomeration restructuring, while the heterogeneity analysis (Section 4.6) identifies the city characteristics that amplify or attenuate the gains.

Under province-level clustering (Column 4), the point estimate is essentially unchanged at 0.262 (SE = 0.112, p = 0.019), suggesting that within-province correlation in implementation environments does not materially inflate the standard errors. Because the sample contains only 30 provinces, province-clustered standard errors should be interpreted cautiously as a sensitivity check rather than as a preferred specification [49]. That the estimate is stable in sign and magnitude across varying control sets (columns 1 and 3 differ by only 0.085 units despite the addition of nine covariates) is consistent with the treatment assignment being uncorrelated with the observable controls, though this does not rule out bias from unobservable confounders.

Table 5. Baseline Difference-in-Differences Estimates of Digital Village Policy.

	(1) No Controls	(2) Basic Controls	(3) Full, City Clust.	(4) Full, Prov. Clust.
DID (*Treat* × *Post*)	0.177 *	0.258 **	0.262 **	0.262 **
	(0.106)	(0.116)	(0.118)	(0.112)
Control variables				
ln(GDP per capita)		1.359 ***	1.128 ***	1.128 ***
		(0.196)	(0.218)	(0.253)
Secondary industry share		−0.148 ***	−0.151 ***	−0.151 ***
		(0.020)	(0.020)	(0.026)
Tertiary industry share		−0.183 ***	−0.185 ***	−0.185 ***
		(0.023)	(0.023)	(0.033)
Financial development			−0.168 **	−0.168 **
			(0.072)	(0.076)
Government intervention			−1.129	−1.129
			(1.032)	(1.384)
Education expenditure/GDP			−0.615	−0.615
			(7.563)	(8.233)
ln(College students)			0.071	0.071
			(0.119)	(0.131)
Urbanization rate			0.011	0.011
			(0.704)	(0.906)
Science expenditure/GDP			14.125	14.125
			(16.487)	(17.877)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Clustering	City	City	City	Province
N	3345			
R^2 (within)	0.004	0.077	0.088	0.088

Note: Parenthesized values are standard errors clustered at the city level (columns 1–3) or province level (column 4). Column (1) includes only city and year fixed effects. Column (2) adds ln(GDP per capita) and secondary/tertiary industry shares. Columns (3)–(4) add financial development, government intervention, education expenditure/GDP, ln(college students), urbanization rate, and science expenditure/GDP. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.3. Parallel Trends and Dynamic Effects

Figure 2 plots the event study coefficients from Equation (3); the underlying estimates appear in Table 6. The event window spans $t = -9$ to $t = +2$ relative to the 2020 implementation, using the full pre-treatment horizon available in the panel (2011–2019) and the post-treatment years through 2022. We deliberately use every available pre-treatment year, rather than a narrower symmetric window, so that the parallel-trends assessment is not selective. The window ends at $t = +2$ (2022) because 2023 is the sole realization of $t = +3$; in a model with year fixed effects, a single-year event-time bin is collinear with the year dummy and cannot be separately identified. All 2023 observations still enter the static DID specification (Table 5) and the robustness checks, so no year is dropped from the main estimates.

All eight pre-treatment coefficients are statistically indistinguishable from zero, and every pre-treatment confidence interval contains zero. This pattern provides direct support for the parallel-trends identifying assumption: there is no evidence of differential pre-trends between treated and control cities in any year from $t = -9$ through $t = -2$.

Table 6. Event Study Coefficients for the Digital Village Policy.

Period	Coefficient	SE	95% CI	p-Value
$t = -9$	0.187	0.159	[−0.126, 0.499]	0.240
$t = -8$	0.204	0.156	[−0.102, 0.511]	0.191
$t = -7$	0.173	0.157	[−0.136, 0.481]	0.271
$t = -6$	0.295	0.157	[−0.014, 0.604]	0.061
$t = -5$	0.211	0.228	[−0.238, 0.661]	0.355
$t = -4$	0.289	0.200	[−0.105, 0.683]	0.150
$t = -3$	−0.202	0.266	[−0.725, 0.322]	0.449
$t = -2$	−0.021	0.218	[−0.449, 0.408]	0.925
$t = -1$		Reference period		
$t = 0$ (2020)	0.293 **	0.133	[0.032, 0.554]	0.028
$t = +1$	0.416 **	0.193	[0.036, 0.796]	0.032
$t = +2$	0.317 **	0.152	[0.018, 0.616]	0.038
City FE + Year FE			Yes	
Controls			Full	
Clustering			City	
N			3345	

Note: Coefficients from Equation (3) with 95% confidence intervals. The reference period ($t = -1$) is normalized to zero. All nine controls included; city-level clustered standard errors. Significance: ** $p < 0.05$.

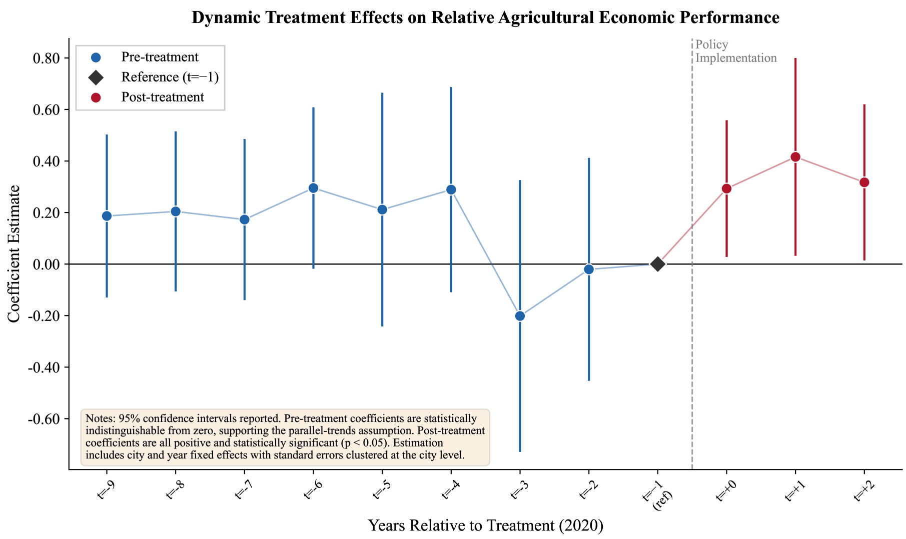


Figure 2. Dynamic treatment effects of the Digital Village Pilot on relative agricultural economic performance ($N = 3345$; 281 cities, 2011–2023). Point estimates and 95% confidence intervals are plotted relative to $t = -1$ (2019), the omitted reference period. City-level clustered standard errors. The dashed vertical line marks the 2020 implementation. All eight pre-treatment confidence intervals contain zero, consistent with the parallel trends assumption.

Post-treatment coefficients tell a different story. At implementation ($t = 0$), the estimated association is 0.293 (p = 0.028), equivalent to 24.2% of a standard deviation. The 95% confidence interval at $t = 0$ is [0.032, 0.554], bounded well away from zero. The association grows larger at $t = +1$ (coef = 0.416, p = 0.032, 34.4% of SD) and remains significant at $t = +2$ (coef = 0.317, p = 0.038, 26.2% of SD). The trajectory (rising from $t = 0$ to $t = +1$ before moderating at $t = +2$) is consistent with an initial ramp-up as pilot cities scale up digital infrastructure and a partial diffusion of gains in the second post-treatment year. The peak at $t = +1$ likely reflects the cumulative effect of front-loaded infrastructure investment and rapid adoption by digitally ready farmers. Importantly, even at $t = +2$ the estimate remains equivalent to more than one-fifth of a standard deviation, economically meaningful regardless of the trend. The pattern (flat pre-trend, sharp jump at $t = 0$, sustained positive post-treatment estimates) is inconsistent with a pre-existing differential trend and consistent with a genuine policy association.

4.4. Threshold Effects

Table 7 reports regime-specific DID estimates at three quartile cutoffs of the threshold variable. To address the endogeneity concern that the threshold variable (digital infrastructure) is itself a flow variable that may respond to the policy, we construct the threshold indicator from the city-level pre-2020 (baseline) mean of digital infrastructure. Cities are split into "below" and "above" groups at each quartile of the baseline distribution, and the DID coefficient is estimated freely within each regime.

Table 7. Threshold Effects: Baseline Digital Infrastructure as Regime Divider.

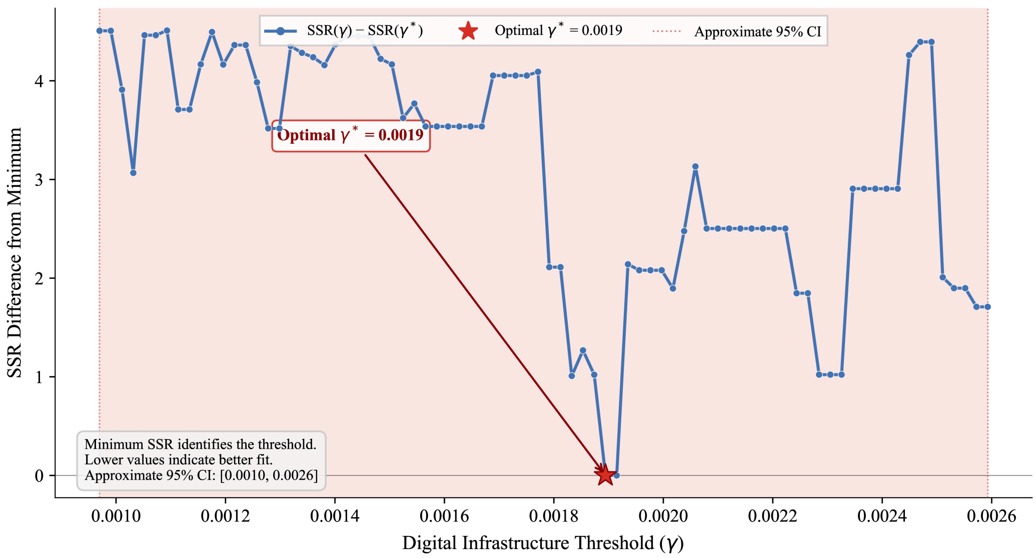
	Cutoff (Quartile of Pre-2020 Mean)		
	P25 (γ = 0.00118)	**P50 (γ = 0.00153)**	**P75 (γ = 0.00196)**
DID (below)	0.174	0.203	0.185
	(0.228)	(0.134)	(0.119)
p-value	0.445	0.130	0.120
DID (above)	0.288 **	0.315 **	0.432 **
	(0.123)	(0.161)	(0.215)
p-value	0.020	0.050	0.045
N		3345	

Note: The threshold variable is the city-level pre-2020 (baseline) mean of digital infrastructure, ensuring exogeneity to the post-2019 treatment. "Below" and "above" denote observations whose baseline digital infrastructure is, respectively, below or above the indicated quartile cutoff. Full controls included; city-level clustered standard errors in parentheses. Significance: ** $p < 0.05$.

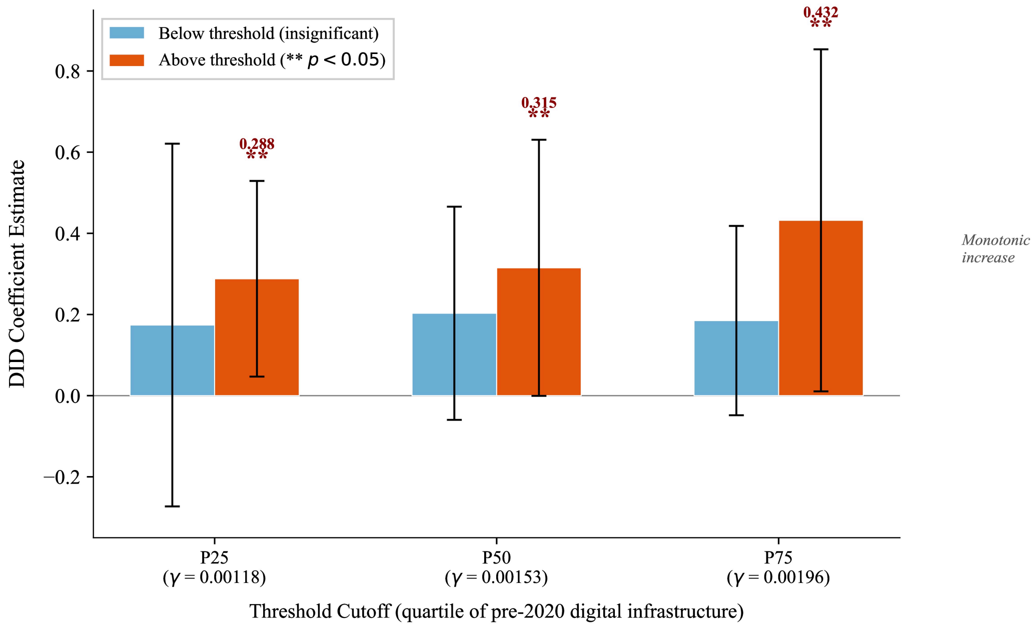
Figure 3 visualizes the threshold analysis: Panel (A) plots Hansen's threshold diagnostic—the sum-of-squared-residuals difference across candidate cutoff values of baseline digital infrastructure, with the optimal γ^* and its approximate 95% confidence band—and Panel (B) plots the regime-specific DID estimates at the three quartile cutoffs. Two patterns emerge. First, the policy effect is statistically significant only above each cutoff: at the P25 cutoff the above-threshold estimate is 0.288 (p = 0.020) while the below-threshold estimate is 0.174 (p = 0.445); at P75 the contrast is sharpest, 0.185 (p = 0.120) versus 0.432 (p = 0.045). Second, the above-threshold point estimate rises monotonically with the cutoff (0.288 $\rightarrow$ 0.315 $\rightarrow$ 0.432), implying that cities with the highest baseline infrastructure experience the largest gains. The below-threshold estimates, by contrast, are statistically indistinguishable from zero across all three cutoffs. This regime contrast supports Hypothesis 2 in a descriptive sense (infrastructure first, applications second), while the use of baseline (pre-2020) infrastructure mitigates the endogeneity concern that the threshold variable itself responds to the policy.

4.5. Channel Analysis

Table 8 presents the channel analysis following the two-step procedure of Jiang [21]. Step 1 tests whether the policy is statistically associated with each candidate mediator (Equation (5)). Step 2 establishes the mediator-to-outcome link through theoretical reasoning and existing literature, avoiding the endogeneity problems that arise from regressing the outcome on an endogenous mediator [21,46]. To keep the threshold variable (digital infrastructure) and the candidate mediators conceptually distinct, we examine digital financial inclusion (DFI, measured by the Peking University DFI index) and agricultural agglomeration as the two candidate mediators rather than digital infrastructure itself.



(**A**)



(**B**)

Figure 3. Panel (**A**): Hansen's [20] threshold diagnostic. The curve plots the SSR difference from its minimum across candidate threshold values of baseline digital infrastructure (γ). The red star marks the optimal γ^* that minimizes SSR; the pink band shows the approximate 95% confidence interval. Panel (**B**): Regime-specific DID estimates at three quartile cutoffs. Below-threshold estimates (blue) are statistically indistinguishable from zero at all cutoffs; above-threshold estimates (red) are significant at the 5% level and rise monotonically from 0.288 at P25 to 0.432 at P75. Error bars denote 95% confidence intervals. $N = 3345$; city-level clustered standard errors.

We stress at the outset that this analysis does *not* establish causal mediation. As a reviewer correctly observed, a statistically significant policy-to-mediator link does not by itself demonstrate that the mediator transmits the policy effect: the second link (mediator $\rightarrow$ outcome) is argued from theory and prior evidence, not identified econometrically, and the mediators are themselves endogenous and jointly determined with the outcome. Accordingly, we report only the policy-to-mediator associations as statistical evidence and describe the mediators as *plausible*, *suggestive* channels. We compute no indirect effects, Sobel statistics, or mediation shares, and none of the channel results should be read as a causal decomposition.

Table 8. Channel Analysis for Digital Village Policy (Two-Step Procedure).

Step 1: Policy → Mediator (Equation (5))		
	M1: Digital Financial Inclusion	**M2: Agricultural Agglomeration**
DID (*Treat* × *Post*)	1.496 *	−0.061 **
	(0.792)	(0.026)
p-value	0.060	0.019
City FE + Year FE	Yes	Yes
Controls	Full (excl. fin_dev)	Full
Clustering	City	City
N	3345	3345

Note: Step 1 reports the effect of the Digital Village Pilot on each candidate mediator from Equation (5), testing the first necessary condition for a transmission channel [21]. For M1 (DFI), the financial-development control is excluded to avoid overlap with the mediator. All other controls included; city-level clustered standard errors in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$.

Digital financial inclusion (M1). Step 1 establishes that the Pilot is associated with a marginally significant increase in digital financial inclusion ($\hat{\pi}$ = 1.496, p = 0.060; Table 8). Relative to the sample mean of the DFI index (approximately 218 in 2020), this represents roughly a 0.7% increase, a modest increment, though the DFI index is itself a composite that changes slowly.

Step 2 invokes theoretical reasoning. The existing literature provides substantial evidence that digital financial inclusion strengthens relative agricultural economic performance through multiple pathways. At the credit level, mobile banking and digital payment services relax liquidity constraints that limit agricultural investment [38]. At the risk-management level, digital insurance and weather-index products allow farmers to hedge against production risks [26]. At the market-access level, digital payment infrastructure enables participation in e-commerce platforms that connect farmers to urban markets. The significant policy-to-mediator link at the 10% level, combined with the well-established theoretical argument, supports Hypothesis 3a: digital financial inclusion is a plausible transmission channel linking the Digital Village Pilot to relative agricultural economic performance, though the evidence is suggestive rather than definitive.

Agricultural agglomeration (M2). Step 1 establishes that the Pilot is associated with a statistically significant decline in agricultural agglomeration ($\hat{\pi}$ = −0.061, p = 0.019), equivalent to a 3.8% reduction relative to the mean LQ of 1.613. The negative sign is notable: rather than concentrating agricultural activity, the Pilot appears to be associated with structural transformation toward a more diversified agricultural economy.

Step 2 relies on theoretical reasoning. The negative association between digital village policies and agricultural agglomeration is consistent with three complementary theoretical mechanisms. First, from the perspective of portfolio theory applied to regional economies, just as financial diversification reduces portfolio risk, agricultural diversification reduces regional vulnerability to single-commodity price shocks, single-crop disease outbreaks, and localized weather events [1]. The rural livelihoods literature documents that households with diversified income streams recover faster from adverse shocks than those reliant on a single agricultural commodity [50]. A decline in geographic agricultural concentration (lower LQ) may therefore signal a broadening of the economic base, representing a structural shift toward a more balanced portfolio of agricultural and agriculturally-linked activities rather than a weakening of agriculture. Second, digital platforms systematically lower the barriers to non-traditional agricultural activities. E-commerce enables direct-to-consumer marketing of specialty crops and processed products; digital advisory services support crop diversification and precision agriculture; rural tourism platforms open new revenue streams from agritourism and heritage products. These digitally-enabled activities expand the agricultural economy beyond traditional commodity production [51], reducing measured geographic concentration even as total agricultural value added may rise. Third, ecological resilience theory provides a parallel: biodiverse ecosystems are more resilient than monocultures because functional redundancy ensures that the loss of any single species does not collapse the system [27]. Analogously, an agricultural economy with a diversified portfolio of activities, crops, and market channels is less susceptible to systemic collapse than one organized around spatial concentration in a single commodity. The observed decline in agglomeration is thus consistent with a diversification-for-resilience channel: digital tools promote activity breadth rather than geographic concentration, and diversified agricultural systems are theorized to be more resilient [1]. This interpretation is reinforced by the heterogeneity finding (Section 4.6) that the Pilot's association with relative agricultural economic performance is concentrated in cities with low agricultural dependence, precisely the cities whose more diversified economic structure may be better positioned to absorb and benefit from digital transformation. This pattern supports Hypothesis 3b. These channel estimates capture within-city correlations and do not warrant a causal interpretation [46].

4.6. Heterogeneity Analysis

To examine whether the policy association varies systematically across city characteristics, we conduct subgroup analysis along five dimensions, testing for structural stability using Chow tests. Because five simultaneous comparisons inflate the risk of false positives, Table 9 also reports Benjamini–Hochberg (BH) adjusted p-values controlling the false-discovery rate [52–54], and we cross-check the dimensions we emphasize with formal full-sample $D_{it} \times$ High interaction terms. Only two dimensions—agricultural dependence and pre-2020 GDP growth—survive the BH correction at the 5% false-discovery rate; financial development is borderline, and the marketization and regional splits do not survive. We therefore treat the heterogeneity results as exploratory patterns rather than definitive moderating effects, and base our interpretation on the two dimensions that are statistically robust.

Agricultural dependence is the strongest moderator (Chow $p = 0.014$; BH-adjusted $p = 0.035$, surviving multiple-testing correction; formal interaction $p = 0.002$). In cities with below-median agricultural dependence, the DID estimate is 0.485 ($p = 0.003$), while in heavily agrarian cities the estimate is effectively zero (-0.004, $p = 0.981$). This pattern is consistent with the diversification interpretation: cities with a more diversified economic structure are better positioned to translate digital connectivity into measurable gains in relative agricultural economic performance, while heavily agrarian cities may lack the complementary non-farm sectors through which digital spillovers typically propagate. This result aligns with the threshold finding: cities with lower agricultural dependence tend to have more developed infrastructure and institutional capacity, which lets them capitalize on the Pilot designation.

Financial development shows a complementary pattern (Chow $p = 0.034$; BH-adjusted $p = 0.057$, borderline), with the policy effect concentrated in cities with above-median financial depth (DID = 0.370, $p = 0.014$) and absent in cities with thinner financial markets (DID = 0.028, $p = 0.858$). Because this dimension is only borderline after multiple-testing correction, we read it as suggestive: it is consistent with the literature on financial constraints as a binding bottleneck for technology adoption—digital tools may require complementary financial infrastructure (credit, insurance, payment systems) to translate into agricultural outcomes—but we do not treat it as established.

Table 9. Heterogeneity Analysis: Sub-Sample DID Estimates, Chow Tests, and Multiple-Testing Adjustment.

	DID	SE	N	Chow p	BH-Adj. p
Panel A: Agricultural dependence					
Low agricultural dep.	0.485 ***	(0.163)	1680	0.014	0.035 **
High agricultural dep.	−0.004	(0.153)	1665		
Panel B: Financial development					
Low financial dev.	0.028	(0.159)	1668	0.034	0.057 †
High financial dev.	0.370**	(0.149)	1665		
Panel C: Marketization					
Low marketization	0.378 **	(0.166)	1753	0.060	0.075
High marketization	0.086	(0.140)	1592		
Panel D: GDP growth (pre-2020)					
Low GDP growth	0.424 **	(0.192)	1678	0.004	0.020 **
High GDP growth	0.171	(0.118)	1667		
Panel E: Region (joint Chow $p = 0.095$)					
Eastern	0.334 *	(0.172)	1964	0.095	0.095
Central	0.278 *	(0.165)	683		
Western	0.456 *	(0.257)	698		

Note: Each row reports a separate TWFE regression on a subsample split at the city-level pre-2020 median (Panels A–D) or by geographic region (Panel E). All models include city and year fixed effects and the full set of nine controls. Standard errors clustered at the city level in parentheses. The Chow test evaluates the null hypothesis that the DID coefficient is equal across subsamples within each panel; the final column reports Benjamini–Hochberg (BH) adjusted p-values controlling the false-discovery rate across the five dimensions [52]. As a formal cross-check, augmenting the full-sample model with a D_{it} × High interaction term yields significant interactions for agricultural dependence ($\hat{\beta} = -0.580$, $p = 0.002$) and pre-2020 GDP growth ($\hat{\beta} = -0.589$, $p = 0.004$), but only a marginal interaction for marketization ($\hat{\beta} = -0.356$, $p = 0.060$). Stars on raw subsample DID estimates: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. On the BH column, ** denotes survival at the 5% false-discovery rate and † borderline; only agricultural dependence and pre-2020 GDP growth survive the correction.

Marketization. At face value the split suggests a larger association in below-median marketization cities (DID = 0.378, $p = 0.024$) than in highly marketized ones (DID = 0.086, $p = 0.539$). A reviewer rightly noted that a stronger effect in *less*-marketized cities seems to run against the intuition that better market institutions amplify policy benefits. Two points resolve the apparent tension. First, the pattern is not statistically robust: the Chow test is significant only at the 10% level (Chow $p = 0.060$), it does *not* survive the Benjamini–Hochberg correction (BH-adjusted $p = 0.075$), and the corresponding full-sample D_{it} × High-marketization interaction is likewise only marginal ($\hat{\beta} = -0.356$, $p = 0.060$). We therefore do not treat marketization as an established moderator. Second, to the extent the pattern is real, it need not contradict theory: in less-marketized cities the Pilot's digital platforms—e-commerce, mobile payments, and digital credit—can *substitute* for missing or thin market institutions, so that the marginal value of digitally mediated market access is largest precisely where formal markets are least developed [26]. This institutional-substitution (or "digital leapfrogging") reading is consistent with the inclusive-finance literature. Given the weak statistical support, however, we report this dimension only for completeness and draw no policy conclusion from it.

GDP growth (pre-2020 baseline) shows the most robust heterogeneity (Chow $p = 0.004$; BH-adjusted $p = 0.020$; formal interaction $p = 0.004$): the policy association is larger in slower-growing cities (DID = 0.424, $p = 0.029$) than in faster-growing ones (DID = 0.171, $p = 0.150$). This pattern suggests that the Pilot may be particularly relevant for cities that have been economically lagging, potentially because they face larger marginal returns to digital intervention.

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Regional heterogeneity (joint Chow $p = 0.095$) reveals that all three regions (eastern, central, and western) show positive and marginally significant DID estimates, with the western region displaying the largest point estimate (0.456, $p = 0.081$) despite lower average infrastructure. This is not necessarily inconsistent with the threshold finding: western pilot cities that receive the designation may be precisely those with relatively better endowments, and the targeted infrastructure investment may represent a proportionally larger shock in low-base cities. However, the western estimate is based on a smaller subsample ($N = 698$) and is significant only at the 10% level, so this interpretation should be viewed cautiously.

Collectively, the two dimensions that survive multiple-testing correction present a coherent pattern: the Digital Village Pilot's association with relative agricultural economic performance is strongest in cities that are economically diversified (low agricultural dependence) and were growing more slowly before the policy. Financial development points in the same direction but is only borderline, while the marketization and regional splits are not statistically robust and are reported for completeness. This robust core echoes the threshold finding that complementary endowments matter for policy effectiveness. Figure 4 displays the subsample DID estimates graphically.

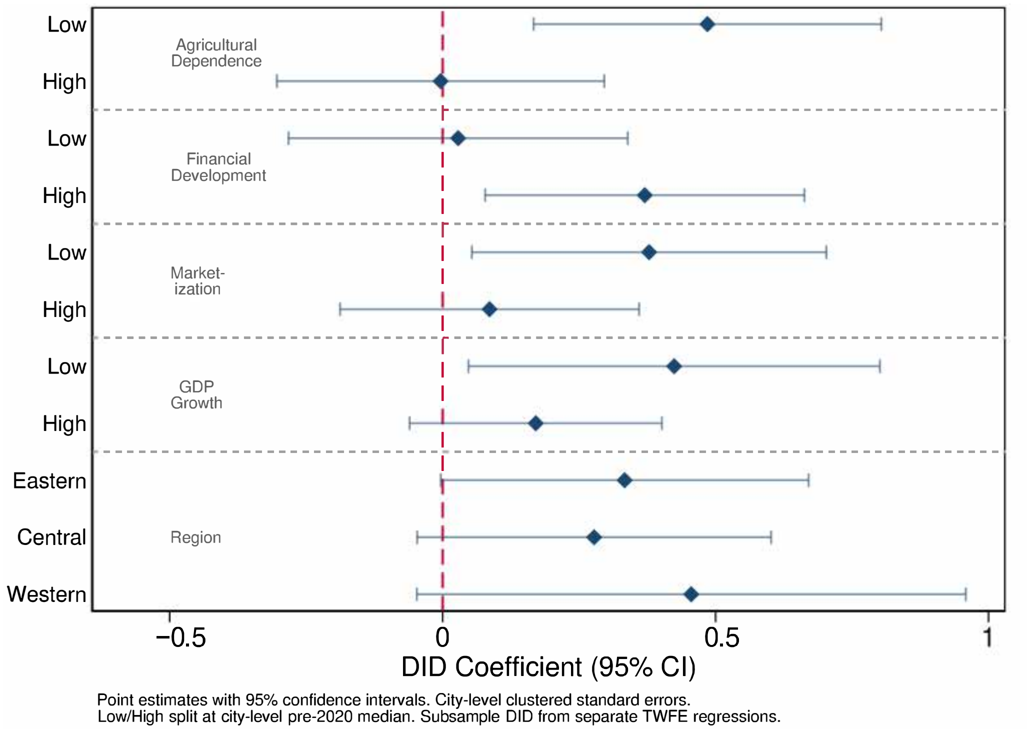

Figure 4. Subsample DID estimates with 95% confidence intervals across five heterogeneity dimensions. Low/High split at the city-level pre-2020 median. All regressions include city and year fixed effects and the full set of nine controls; city-level clustered standard errors. The dashed vertical line at zero denotes no association.

4.7. Robustness Checks

To probe the fragility of the baseline estimate, Table 10 summarizes nine robustness tests. The *sign* and statistical significance of the association are stable: all nine specifications remain significant at the 5% level, and the permutation placebo test rejects random assignment (p = 0.006). The *magnitude*, however, varies non-trivially across specifications—from 0.180 under winsorization to 0.375 under caliper-matched PSM-DID, roughly a twofold range around the 0.262 median. We therefore characterize the effect as robust in direction and significance but moderate and somewhat specification-dependent in size, rather than uniform.

Table 10. Robustness Tests for the Digital Village Policy Effect.

Specification	DID Coef.	SE	*p*-Value	*N*
Baseline (for reference)	0.262 **	(0.118)	0.026	3345
(1) PSM-DID (caliper 0.05)	0.375 **	(0.169)	0.029	1347
(2) Lagged treatment	0.215 **	(0.105)	0.042	3345
(3) Exclude municipalities	0.259 **	(0.115)	0.025	3297
(4) Placebo assignment test	0.262	—	0.006	3345
(5) Winsorized (5th/95th)	0.180 **	(0.077)	0.021	3330
(6) Minimal controls	0.259 **	(0.112)	0.021	3345
(7) Window 2013–2023	0.292 **	(0.123)	0.018	2785
(8) Exclude year 2020	0.318 **	(0.126)	0.012	3067
(9) Province-level cluster	0.262 **	(0.112)	0.019	3345
Range of significant estimates: 0.180–0.375; Median: 0.262				

Note: All models include city and year fixed effects. (1) Propensity score matching DID using 1:1 nearest-neighbor matching with caliper 0.05 on pre-treatment city-level covariates (57 treated matched to 57 controls; 114 matched cities); (2) one-year lagged treatment; (3) excluding four directly administered municipalities; (4) permutation test using 500 randomly reshuffled treatment assignments; the coefficient is the true estimate and the reported *p*-value is the upper-tail share of placebo coefficients exceeding it, so no conventional regression standard error is reported; (5) winsorizing at 5th/95th percentiles; (6) only ln(GDP per capita) and industrial structure controls; (7) restricted estimation window; (8) dropping policy year 2020; (9) standard errors clustered at the province level rather than the city level. Significance: ** $p < 0.05$.

The robustness tests broadly support the baseline result in sign and significance, while underscoring that its magnitude is specification-dependent: all nine specifications remain significant at the 5% level, including the permutation-based placebo test (p = 0.006), but the point estimates span roughly a factor of two (0.180–0.375). PSM-DID with caliper matching (row 1) yields $\hat{\theta} = 0.375$ (p = 0.029) on the matched subsample of 114 cities (1347 city-year observations). The PSM-DID estimate is somewhat larger than the baseline, reflecting the tighter common support imposed by caliper matching; the matched sample is smaller than the full panel because the 1:1 matching discards control cities without a close treated counterpart. Post-match balance diagnostics indicate that six of seven covariates achieve post-match standardized mean differences below 0.10, with the remaining covariate (agglomeration) at 0.11, marginally above the conventional threshold (see Appendix A).

The lagged treatment specification (row 2, $\hat{\theta} = 0.215$, p = 0.042) confirms that the effect is not driven solely by the implementation year, retaining significance at the 5% level despite the shorter effective post-treatment window (2021–2023). Excluding municipalities (row 3), restricting the window (row 7), omitting the policy year (row 8), and clustering at the province level (row 9) all leave the estimate significant at the 5% level, with point estimates ranging from 0.259 to 0.318.

The most conservative quantitative estimate comes from winsorization (row 5): $\hat{\theta} = 0.180$ (p = 0.021), which is 31% smaller than the baseline. The reduction reflects the trimming of outlier observations with extreme relative-performance values (likely those affected by severe droughts or floods), which indicates that part of the baseline estimate captures tail events rather than policy effects alone. Even this lower bound represents 12.1% of a standard deviation, which is not negligible. The significant estimates cluster in a band from 0.180 to 0.375, with a median of 0.262, suggesting that the main result is not driven by a single specification. The minimal-controls specification (row 6) retains significance at the 5% level ($\hat{\theta} = 0.259$, p = 0.021), confirming that the result is not an artifact of the particular control set.

The 500-permutation placebo (row 4) yields an upper-tail p = 0.006: only 0.6% of placebo coefficients exceed the true estimate, indicating that the observed DID coefficient is highly unlikely to arise from a random spatial assignment of pilot status (see Figure 5).

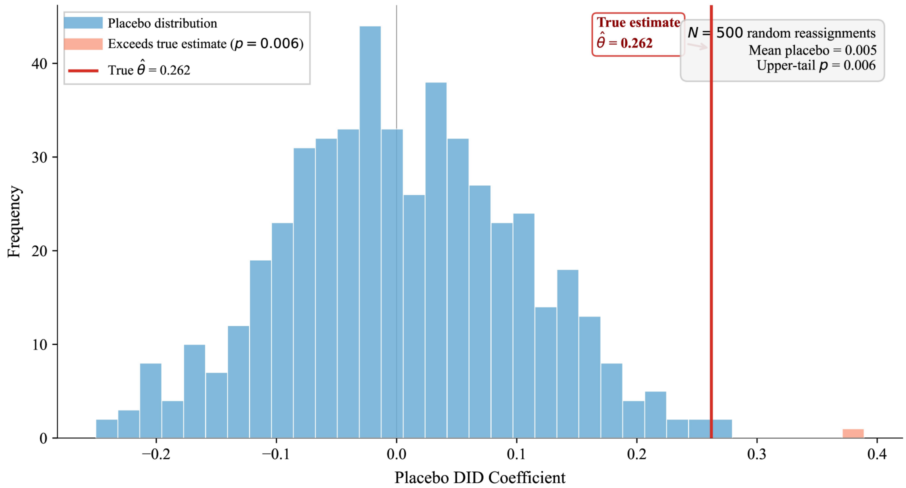

Figure 5. Permutation test with 500 random treatment reassignments ($N = 3345$). The histogram displays the distribution of placebo DID coefficients obtained by randomly reshuffling treatment assignment across 281 cities; the red vertical line marks the true estimate ($\hat{\theta} = 0.262$). The empirical upper-tail p-value of 0.006 indicates that only 0.6% of placebo estimates exceed the true coefficient, confirming that the observed effect is highly unlikely to arise from a random spatial assignment of pilot status.

Because the policy year (2020) coincides with the COVID-19 pandemic, differential pandemic-related agricultural disruptions could confound the estimate. Reassuringly, the specification that drops 2020 (row 8) yields $\hat{\theta} = 0.318$ ($p = 0.012$), suggesting that the result is not driven by pandemic-era anomalies in the implementation year alone. We note, however, that the post-2020 period was also affected by COVID-19 disruptions in China, and if pilot and non-pilot cities were differentially exposed to these disruptions, some confounding may remain.

4.8. Alternative Relative-Performance Measures

A natural concern, raised in review, is whether the headline result is an artifact of one particular output base—first-industry value added—rather than a feature of relative agricultural economic performance more generally. To address this, and to give empirical content to the relabeled construct, we re-estimate the baseline DID (Equation (2)) using two alternative relative-performance outcomes built from different agricultural output measures, each benchmarked against its national aggregate in the same way as the baseline (Table 11).

Table 11. Alternative Relative-Performance Outcomes.

Outcome (Relative-Performance Form)	DID Coef.	SE	p-Value	N
Baseline: first-industry value added	0.262 **	(0.118)	0.026	3345
(A) Gross agricultural output value	0.713 **	(0.354)	0.045	3024
(B) Physical grain output (tonnes)	0.008	(0.006)	0.210	3011

Note: Each row re-estimates Equation (2) with the same nine controls, city and year fixed effects, and city-clustered standard errors, replacing only the dependent variable. (A) uses the relative-performance form $(y_{it} - x_t)/|x_t|$ on gross agricultural output value; (B) uses the relative form $y_{it} - x_t$ on physical grain output (the level-normalized ratio is undefined because national grain growth is near zero in several years). Both alternative outcomes are winsorized at the 1st/99th percentiles to limit small-base outliers. Coefficient *magnitudes* are not directly comparable across rows because the normalizing denominators differ; the comparison of interest is sign and significance. Significance: ** $p < 0.05$.

The first replaces first-industry value added with *gross agricultural output value* (the gross output of farming, forestry, animal husbandry, and fishery). The policy association remains positive and significant ($\hat{\theta} = 0.713$, $p = 0.045$), confirming that the finding is not specific to the value-added accounting category. The second uses *physical grain output* in tonnes, a price-robust, food-security–oriented measure. Here the association is positive but not statistically significant ($\hat{\theta} = 0.008$, $p = 0.21$).

We read this contrast as substantively informative rather than as a failure of robustness. The Pilot moves the *economic* dimension of agricultural performance—value added and gross output value—but not raw physical grain tonnage. This is precisely what the diversification channel (Section 4.5) predicts: digital tools shift activity toward higher-value, diversified outputs (e-commerce, specialty crops, agritourism, processing) rather than expanding staple-grain volume. It is also why we label the outcome relative agricultural *economic* performance rather than agricultural resilience writ large. Together, the two measures bound the construct: the economic-performance result is robust to the choice of output base, while the absence of a physical-grain effect cautions against interpreting it as a gain in physical food production.

5. Discussion

5.1. Interpreting the Main Effect

Our analysis delivers quasi-experimental evidence on the Digital Village Pilot's association with relative agricultural economic performance. The baseline DID estimate (Equation (2)) of 21.6% of a standard deviation, stable across most specification checks (median estimate 0.262), positions the Digital Village Pilot as a moderately effective agricultural policy intervention under the maintained identifying assumptions, comparable in magnitude to documented effects of broadband rollout on rural economic outcomes in other developing-country settings [55].

The event study dynamics add a temporal dimension that speaks to the policy's implementation logic. The sharp jump at implementation followed by a declining (though not statistically distinguishable) trajectory through $t = +2$ is consistent with two non-exclusive interpretations: (i) an initial "announcement effect" as pilot cities front-load investments to meet designation targets, followed by slower consolidation; or (ii) early adopters capturing gains quickly while late adopters within treated cities take longer to benefit. Disambiguating these interpretations requires longer post-treatment panels and within-city micro-data.

5.2. Threshold: The Signature Finding

The threshold result from Equation (4) is the most policy-relevant finding. The near-zero association below the cutoff and the substantial gain above it (nearly one-quarter of a standard deviation) reframe the policy challenge from "how to design better digital applications" to "how to close the infrastructure gap first." This resonates with the broader literature on technology adoption thresholds: just as Peng and Dan [33] find that the digital dividend for rural incomes materializes only beyond a digitalization threshold, our results suggest that relative agricultural economic performance gains are concentrated where a stronger infrastructure base is present.

The threshold finding also interacts with the heterogeneity results in an important way. Agricultural dependence, the strongest moderator (Chow $p = 0.014$), is directionally consistent with the threshold logic: cities with lower agricultural dependence (and typically better infrastructure endowments) show a substantially larger policy association, which mirrors the above-threshold advantage identified in Section 4.4. The financial-development dimension points in the same direction—the policy association is concentrated in cities with above-median financial depth—although it is only borderline after multiple-testing correction (BH-adjusted $p = 0.057$), so we read it as corroborative rather than decisive:

credit, insurance, and payment systems are plausible complementary enablers alongside digital infrastructure.

More broadly, the pattern of significance only where complementary endowments are present (recurring across digital infrastructure, financial development, and economic diversification) suggests a "stacking" logic: the Digital Village Pilot appears to depend on more than digital infrastructure, also requiring a constellation of enabling conditions (financial depth, diversified economic structure, institutional readiness) to translate into relative agricultural economic performance gains. This finding connects to the capabilities literature on technology adoption [55] and suggests that single-dimensional interventions may be insufficient in the absence of complementary investments.

5.3. Channels: Digital Financial Inclusion and Agricultural Diversification

The channel analysis (Section 4.5) identifies two transmission channels using the two-step procedure of Jiang [21]: *digital financial inclusion* and *agricultural diversification*.

Digital financial inclusion is a plausible channel because the Pilot is associated with a marginally significant increase in the DFI index ($\hat{\pi} = 1.496$, $p = 0.060$; Table 8), and the theoretical case linking digital financial services to agricultural resilience is well established in the literature (Section 2.3). The Pilot's promotion of broadband connectivity and digital platforms creates the infrastructure layer on which digital financial services (mobile banking, digital credit, and online insurance) depend. Infrastructure appears to function primarily as an enabling condition (the policy association is substantially larger when infrastructure exceeds γ^*; Section 4.4), while DFI captures the application-layer financial channel through which connectivity translates into gains in relative agricultural economic performance. This interpretation is consistent with the threshold finding: infrastructure is a prerequisite for digital financial services to reach farmers, but the policy association may also reflect other channels (governance, market access, behavioral change) beyond financial inclusion.

The agglomeration channel reveals a diversification pattern with important theoretical implications. The Pilot is associated with a 3.8% decline in geographic agricultural concentration ($p = 0.019$), and the theoretical literature links such diversification to greater resilience through three mechanisms: portfolio risk reduction across multiple crops and activities [1,50], digitally-enabled expansion into non-traditional agricultural value chains, and enhanced ecological-style functional redundancy analogous to biodiverse ecosystems [27]. A plausible interpretation is that digital tools promote activity *diversification* rather than geographic *concentration*: farmers adopt e-commerce, agritourism, and value-added processing alongside traditional crops, lowering measured agglomeration even as diversification itself is resilience-enhancing. This is reinforced by the heterogeneity finding that the Pilot's association with relative agricultural economic performance is strongest in cities with low agricultural dependence, cities whose diversified economic structure may be better positioned to absorb digital transformation gains. This pattern is consistent with the Pilot's emphasis on multi-dimensional rural modernization rather than single-commodity specialization.

5.4. Policy Implications: The Sequencing Lesson

The results point to a conditional policy implication for digital rural transformation globally: *sequence infrastructure before applications*. This pattern is not necessarily specific to China. Rural development programs that deploy digital tools in areas without foundational connectivity may face the same structural problem identified here.

Priority 1: Close the infrastructure gap. A slight majority of Chinese cities fall below the digital infrastructure threshold. For these cities, expanding digital village designation without parallel infrastructure investment may yield limited relative agricultural economic performance gains. Broadband deployment, mobile network coverage, and digital platform construction should precede or accompany pilot designation. This finding is directly relevant to rural development practitioners debating whether to prioritize hardware (connectivity) or software (applications) in digital rural programs.

Priority 2: Target regions with lower baseline infrastructure but adequate complementary conditions. Cities with lower pre-2020 GDP growth show the largest policy association (DID = 0.424, $p = 0.029$), suggesting that lagging regions may benefit disproportionately from digital village designation, provided that complementary financial and institutional infrastructure is in place. Expanding the Pilot to these regions, with the requisite infrastructure investment, would align with China's regional convergence objectives and echoes lessons from rural electrification literature on the disproportionate returns to infrastructure in lagging areas.

Priority 3: Coordinate with financial sector development. The (borderline) near-zero estimate in cities with low financial development (coef = 0.028, statistically indistinguishable from zero) is suggestive that digital village programs may not realize their full benefits in a financial vacuum. Digital financial services, agricultural insurance, and tailored credit products should be bundled with digital village rollout. This has implications for the design of rural development packages in other developing countries, where thin rural financial markets are common.

Priority 4: Embrace rural economic diversification. The suppression pattern suggests that policymakers should not expect increased agricultural concentration as the only route to resilience. Instead, digital village policies may be compatible with more diversified rural economies, which are often more resilient than specialized ones. This aligns with the rural livelihoods literature's emphasis on income diversification as a core resilience strategy [1].

5.5. Limitations and Future Research

Six limitations deserve emphasis. First, the causal identification rests on conditional parallel trends, not random assignment. Although the event study (Section 4.3) finds no differential pre-trends and city fixed effects absorb time-invariant confounders, unobserved time-varying factors could still bias the estimate. The pre-treatment balance tests (Table 3) document significant differences that motivate time-varying controls and the PSM-DID check, but these address only selection on observables. The direction of residual bias is ambiguous. Future work employing instrumental-variable strategies exploiting geographic or political discontinuities in pilot designation could strengthen causal inference.

Second, the channel analysis does not establish causal mediation. The two-step procedure of Jiang [21] tests the policy-to-mediator link statistically and supports the mediator-to-outcome link theoretically, thereby avoiding the post-treatment mediator endogeneity problem [46]. However, this approach does not quantify indirect effects, and the mediators are endogenous post-treatment variables [56]. We accordingly characterize the channels as *plausible and suggestive* rather than causally identified, and compute no mediation shares. Future research employing instrumental variables for mediators or formal causal mediation analysis with sensitivity checks could decompose transmission pathways.

Third, the post-treatment window (2020–2023) is short, providing only three point estimates. We cannot determine whether the partial moderation at $t = +2$ reflects genuine attenuation, transitory volatility, or a plateau. The technology-adoption literature suggests that full benefits of digital interventions may materialize over several years [55], while policy diffusion to control cities could narrow treatment-control contrasts. The reported effect (0.262) should therefore be interpreted as a short-run association, and extending the analysis to 2025+ would help distinguish transitory from permanent gains.

Fourth, RAEP captures the *resistance* dimension of regional economic resilience (relative economic standing during shocks) and is an economic measure based on first-industry value added. It does not capture recovery, adaptability, ecological resilience (soil health, water quality), or household livelihood resilience—each of which would require different data and methods. The validation exercise in Section 4.8 confirms this scope: the association is significant for value-based outcomes but not for physical grain output. Future work could construct a multidimensional index incorporating ecological, physical-production, and household-livelihood indicators. Recent quasi-experimental evidence that another Chinese place-based pilot policy—the Zero-Waste City program—enhances urban ecological resilience [57] indicates that digital village designation could likewise be evaluated against ecological-resilience outcomes.

Fifth, generalizability beyond China warrants caution. China's distinctive institutional environment—strong central coordination, substantial fiscal capacity, smallholder-dominated agriculture (average farm < 1 ha), and a relatively high 2020 digital infrastructure baseline—differs from many developing countries. The threshold effects documented here may manifest at higher infrastructure levels where connectivity is sparse, and the quantitative estimates should not be mechanically extrapolated. Cross-country comparative studies would help identify which findings generalize.

Sixth, the threshold variable is fixed at its pre-2020 baseline to ensure exogeneity [20], but this prevents detection of within-city regime shifts as infrastructure improves post-treatment. The threshold estimates capture pre-existing infrastructure conditions, not dynamic threshold-crossing effects. Future work employing dynamic panel threshold models or exogenous infrastructure shocks could address this trade-off.

Collectively, these limitations bound what the study can claim: the estimates are quasi-experimental rather than strictly causal, the channels suggestive rather than causally decomposed, the effects short-run, the outcome economic rather than multidimensional, and the findings context-specific to China. Future research through longer panels, experimental designs, multidimensional resilience indices, and cross-country replication will be essential for strengthening the evidence base.

6. Conclusions

Applying a TWFE difference-in-differences design to an unbalanced panel of 281 Chinese prefecture-level cities (2011–2023), this study evaluates whether, and under what conditions, the Digital Village Pilot policy is associated with higher relative agricultural economic performance. The Pilot is associated with a 21.6% standard-deviation increase in relative agricultural economic performance, an estimate that is broadly stable across specification checks (significant estimates range from 0.180 to 0.375) and emerges with no evidence of pre-existing differential trends. More importantly, this average association is conditional on infrastructure: Hansen threshold regression estimates a negligible policy association in cities below a critical digital infrastructure cutoff, and a substantial association (ranging from 0.288 to 0.432 depending on the cutoff quartile) in cities above it. Since many Chinese cities fall below the cutoff, the program's benefits are not yet broadly accessible. Channel analysis following Jiang [21] adds a further nuance: the Pilot is associated with marginally higher digital financial inclusion ($\hat{\pi} = 1.496$, $p = 0.060$) and significantly lower agricultural agglomeration (-3.8%), with the latter pointing to diversification of agricultural activities rather than geographic concentration as a plausible resilience-enhancing channel supported by the rural livelihoods literature.

The central policy implication is one of *sequencing*: foundational digital infrastructure should be in place before application-layer policies can reliably deliver relative agricultural economic performance gains. For the majority of cities below the identified threshold, the priority investment is broadband deployment, mobile network coverage, and digital platform construction rather than additional application-layer designation. Policymakers should conduct infrastructure readiness assessments before expanding the Pilot program, and bundle digital village rollout with coordinated financial-sector development, given the suggestive near-zero estimate in cities with low financial depth.

This study makes several contributions to the literature. It provides quasi-experimental evidence on the Digital Village Pilot's association with city-level relative agricultural economic performance, identifies threshold conditions in digital rural policy effectiveness, and examines suggestive transmission channels that have not been fully documented in this context. By showing that digital infrastructure is closely associated with policy effectiveness, the findings are relevant to the broader policy agenda covered by SDG 9 (building resilient infrastructure) and SDG 2 (ending hunger through sustainable agriculture), though we emphasize that our outcome measure captures relative agricultural economic performance rather than comprehensive agricultural system resilience. The evidence offers practical policy references for developing countries advancing digital rural transformation strategies. As the digital transformation of agriculture progresses, extending the post-treatment window and employing formal causal mediation methods, together with a multidimensional resilience measure spanning recovery, adaptability, ecological, and livelihood dimensions, will be important next steps for strengthening the evidence base.

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

Appendix A. Covariate Balance for the PSM-DID Specification

Table A1 reports the standardized mean differences (SMDs) between treated and control cities for the seven covariates used in the propensity-score matching underlying the PSM-DID robustness check (row 1 of Table 10), before and after 1:1 nearest-neighbor caliper matching. After matching, six of the seven covariates exhibit absolute SMDs below the conventional 0.10 threshold; only agglomeration remains marginally above it (0.110), confirming that caliper matching substantially improves covariate balance.

Table A1. Standardized mean differences before and after propensity-score matching.

Covariate	Pre-Match SMD	Post-Match SMD
ln(GDP per capita)	−0.316	0.091
Secondary industry share	0.015	0.023
Tertiary industry share	−0.225	0.092
Financial development	−0.337	0.049
Urbanization rate	−0.168	0.044
Digital infrastructure	−0.236	−0.045
Agglomeration (LQ)	0.205	−0.110

Note: Entries are standardized mean differences (SMDs) between treated and control cities, computed as the difference in means divided by the pooled standard deviation. "Pre-Match" uses the full sample; "Post-Match" uses the 1:1 nearest-neighbor caliper-matched sample. An absolute SMD below 0.10 is conventionally regarded as adequate balance.

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