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The Economic Empowerment Effect of Intangible Cultural Heritage: Causal Identification Based on Multi-Period DID and Historical Instrumental Variable

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Abstract

As the strategic role of cultural capital in regional development becomes increasingly prominent, evaluating the macroeconomic impact of National Intangible Cultural Heritage (ICH) policies necessitates rigorous causal identification. Based on a panel dataset of 286 Chinese prefectural-level cities from 2000 to 2019, this paper investigates the causal effect of the national ICH policy on urban per capita GDP utilizing multi-period Difference-in-Differences (DID) and Instrumental Variable (IV-2SLS) approaches. Our baseline estimates indicate that standard DID results are statistically insignificant due to a structural "negative selection bias": cities confronting economic bottlenecks exhibit stronger endogenous incentives to seek ICH status for revitalization. To mitigate this endogeneity, we construct an instrumental variable by interacting a historical Confucian cultural endowment index (from the Ming and Qing dynasties) with a linear time trend. By controlling for modern secondary school enrollment to account for the human capital transmission channel—and providing supportive evidence for the plausibility of our identification strategy via Conley's plausibly exogenous bounds and Hansen *J* overidentification tests—we find a robust positive policy effect: each additional national ICH item significantly increases a city's per capita GDP by approximately 1.6%. Moreover, mechanism analysis based on absolute sectoral employment empirically refutes the conventional "hollow tourism" stereotype, revealing a pronounced secondary-sector employment expansion wherein each additional national ICH item increases secondary industry employment by 11.0%, more than quadrupling the 2.5% expansion in tertiary employment. Finally, full-sample interaction heterogeneity analyses reveal a pronounced "catch-up effect": consistent with diminishing marginal returns, cities with relatively lagging initial economic and industrial foundations reap substantially larger marginal growth dividends (4.6% and 4.4%, respectively) than advanced regions. This study provides empirical evidence that cultural heritage, when properly leveraged, acts as an effective catalyst for real-economy structural transformation and regional convergence rather than a mere tourism asset.

Keywords: intangible cultural heritage; economic growth; structural transformation; Difference-in-Differences; instrumental variable; historical cultural endowment

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1. Introduction

Sustaining robust economic development remains the central pillar for nations seeking to transition from factor-driven expansion to innovation-driven, high-quality growth [1].

In this macroeconomic transition, traditional historical endowments are increasingly recognized not merely as relics requiring continuous government subsidies, but as vital forms of cultural capital capable of generating profound economic externalities [2]. As the world's largest developing economy, China has actively sought new drivers to upgrade its regional economic structures. Against this backdrop, the official launch of the National Intangible Cultural Heritage (ICH) list in 2006 marked a pivotal strategic shift. It systematically transformed highly dispersed local cultural traditions into officially branded, nationally recognized strategic assets, providing a profound quasi-natural experiment to evaluate the macroeconomic implications of cultural preservation.

Theoretically, how does the official recognition of ICH translate into regional gross domestic product (GDP) growth? Conventional wisdom heavily presumes a "ticket economy" stereotype, arguing that cultural heritage drives the economy almost exclusively by stimulating local tourism and the tertiary sector [3]. However, this perspective overlooks the broader industrial impact of ICH. The official ICH branding significantly reduces information asymmetry and grants a substantial premium to traditional products and local characteristic industries. Rather than merely boosting hollow tourism, ICH acts as a catalyst for secondary-sector employment expansion—driving labor reallocation and industrial upgrading in sectors such as traditional craftsmanship, artisanal brewing, sustainable agriculture, and heritage processing. Through these channels, intangible cultural endowments directly anchor the real economy and stimulate macroeconomic growth.

Despite these compelling theoretical mechanisms, estimating the causal effect of cultural policies on urban GDP is plagued by profound empirical difficulties, primarily driven by self-selection endogeneity. The designation of national ICH is not purely random. Local governments facing severe economic bottlenecks, resource depletion, or traditional industry decline often possess much stronger incentives to aggressively apply for national cultural titles as an alternative survival strategy. This inherent "negative selection bias" structurally downward-biases standard empirical estimators (such as classic two-way fixed effects models), inherently masking the economic dividends of cultural heritage and often leading to underestimated or insignificant results.

To investigate the plausible economic impact of the ICH policy and address these endogeneity concerns, this paper constructs a comprehensive panel dataset of 286 Chinese prefectural-level cities from 2000 to 2019 and employs a progressively rigorous empirical strategy. We begin by estimating a baseline multi-period Difference-in-Differences (DID) model using a binary policy shock dummy to evaluate the extensive margin of the ICH designation [4]. Recognizing that cities granted multiple ICH items over the years receive substantially more cultural and fiscal resources than those with a single item, we then advance to a continuous DID model. By utilizing the cumulative count of national ICH items, this specification captures the intensive margin of the policy and preserves the crucial variation in policy intensity. Subsequently, to verify the fundamental parallel trend assumption and examine dynamic effects, we conduct a rigorous event-study analysis. To explicitly mitigate the potential endpoint binning bias often associated with heterogeneous treatment effects in staggered DID models [5], we restrict our estimation sample to a symmetrical event window of five years before and after the policy intervention. Furthermore, recognizing that restricting the event window does not fully resolve the negative-weighting bias inherent in traditional Two-Way Fixed Effects (TWFE) under staggered adoption, we further validate our dynamic trends using the cutting-edge heterogeneity-robust DID estimator proposed in [6]. This advanced approach utilizes never-treated cities as a clean control group, rigorously confirming the validity of our parallel trends assumption without TWFE contamination.

Crucially, because both the binary and continuous DID estimates remain susceptible to the aforementioned negative selection bias, we introduce an Instrumental Variable (IV-2SLS) approach into our TWFE DID framework. We construct an instrument by interacting a cross-sectional historical Confucian culture index—comprising the historical counts of academies and Confucius temples during the Ming and Qing dynasties—with a linear time trend. To plausibly satisfy the exclusion restriction, we explicitly control for the natural logarithm of the number of modern secondary school students.

We specifically employ the scale of secondary school students, rather than primary or tertiary students, as the proxy for contemporary human capital due to methodological considerations. In the Chinese urban context, primary education is universally compulsory, thereby lacking sufficient regional variance to capture human capital disparities. Conversely, college students exhibit extreme spatial mobility, reflecting a city's higher education infrastructure rather than the endogenous human capital of its resident labor force. Secondary school enrollment, heavily tethered to local household registration (Hukou) and reflecting intense educational sorting mechanisms, serves as a reliable and spatially sticky indicator of a region's genuine educational investment. This critical step helps account for the alternative transmission channel where historical culture might affect current economic growth via modern human capital accumulation [7], ensuring our historical IV plausibly identifies contemporary economic growth predominantly through the cultural heritage mechanism.

Furthermore, recognizing that historical endowments might still influence contemporary economies through other unobserved persistent channels—such as institutional quality, social capital, or long-term development trajectories—we conduct a comprehensive analysis of exclusion restriction sensitivity and control variable validity in subsequent sections. By employing Conley et al.'s plausibly exogenous IV bounds [8], Hansen *J* overidentification tests [9], and controls for pre-existing dynamic trends, we provide supportive evidence that our estimates remain robust even if the strict exclusion restriction is partially relaxed.

This paper makes three primary contributions to the existing literature, which has traditionally treated cultural heritage either as a pure tourism asset or overlooked the endogenous administrative motives behind heritage applications.

First, it maps a meticulous methodological progression from a baseline multi-period DID model to an IV-2SLS framework, effectively mitigating the endogeneity concern in evaluating macroscopic cultural policies. After correcting for the aforementioned negative selection bias utilizing the historical distribution of Confucian academies and temples, we reveal that each additional national ICH item robustly increases a city's per capita GDP by approximately 1.6%. The credibility of our findings is further reinforced through a comprehensive suite of robustness and sensitivity checks—including 1–99% winsorization, subsample estimations excluding provincial capitals and sub-provincial cities, alternative treatment transformations, pre-determined baseline economic and educational trend controls, anticipation tests, population scale adjustments, Hansen *J* overidentification tests, and the plausibly exogenous IV sensitivity framework of Conley et al. [8].

Second, this study fundamentally shifts the research focus from micro-level tourism consumption to macro-level structural transformation. By examining absolute sectoral employment levels rather than endogenous employment shares, we empirically refute the conventional "hollow tourism" stereotype. Instead, we identify a pronounced secondary-sector employment expansion, demonstrating that while the ICH policy modestly increases tertiary employment by 2.5%, it expands absolute employment in the secondary industry by 11.0%. This provides a fresh, evidence-based perspective on how traditional heritage can anchor real-economy manufacturing and defend against premature deindustrialization.

Finally, through unified full-sample interaction IV-2SLS heterogeneity analyses, we uncover crucial boundary conditions and a compelling "catch-up effect" across initial eco-

nomic and industrial endowments. Governed by the law of diminishing marginal returns, official ICH branding functions as a scarce comparative advantage for less-developed cities: regions with below-median initial per capita GDP or lagging industrial foundations experience pronounced marginal growth dividends (4.6% and 4.4%, respectively), whereas advanced cities exhibit much smaller incremental gains. Rather than exacerbating regional inequalities (the "Matthew Effect"), cultural heritage revitalization serves as an inclusive engine that narrows regional disparities when paired with real-economy industrialization.

The remainder of this paper is organized as follows: Section 2 reviews the relevant literature, identifies the research gaps, and delineates the theoretical mechanisms and hypotheses. Section 3 details the data sources, sample selection criteria, and variable constructions. Section 4 outlines the empirical strategy, highlighting the multi-period DID framework and the historical instrumental variable identification. Section 5 presents the baseline estimations and discusses the core IV-2SLS results. Section 6 provides a series of robustness checks. Section 7 delves into the mechanism analysis, exploring labor reallocation and secondary-sector employment expansion. Section 8 conducts dual-perspective heterogeneity analyses based on initial economic and industrial endowments. Finally, Section 9 concludes the study and discusses relevant policy implications.

2. Literature Review and Theoretical Mechanisms

In this section, we review the related literature, identify the research gap, and delineate the theoretical mechanisms and hypotheses.

2.1. Literature Review

2.1.1. Cultural Capital and Macroeconomic Structural Transformation

The macroeconomic foundation of this paper is rooted in classic growth theories regarding the transition from factor-driven expansion to innovation-driven development [1]. Within this transition, cultural endowments are increasingly recognized not merely as relics requiring subsidies, but as vital forms of "cultural capital" capable of generating profound economic externalities [2,10].

Traditionally, the economic attributes of cultural heritage have been evaluated predominantly through the lens of the "ticket economy." A large strand of literature assumes that cultural consumption and heritage commercialization drive urban development almost exclusively by stimulating local tourism and the tertiary sector [11–13]. While these studies provide valuable insights into tourism-led growth, they largely overlook the broader industrial impact of cultural heritage. Our study bridges this gap by engaging with theories of industrial upgrading and labor reallocation, arguing that official heritage branding reduces information asymmetry and grants a substantial premium to traditional characteristic industries. This theoretically supports a secondary-sector employment expansion mechanism over the conventional tourism-driven growth model.

2.1.2. Policy Evaluation and Negative Selection Bias

Evaluating the macroeconomic impact of spatial and cultural policies has garnered significant research interest. Recent empirical evidence has explored the economic, social, and environmental impacts of China's designation of historical and cultural cities [14]. However, evaluating National ICH introduces severe endogeneity issues rooted in local government incentives and fiscal decentralization.

The literature on Chinese local government competition highlights that regional officials often exhibit strategic behaviors and home bias when pursuing national resources [15]. Consequently, regions facing resource depletion or economic bottlenecks have much stronger incentives to aggressively apply for national cultural titles as an alternative survival strategy.

This structural "negative selection bias" inherently downward-biases standard estimation models, masking the economic dividends of cultural preservation.

2.1.3. Advanced Econometrics and Historical Identification

To rigorously evaluate the ICH policy, this study relies on recent advancements in causal inference. Foundational works on multi-period DID and cluster-robust standard errors have long established the framework for policy evaluation [4]. However, recent cutting-edge econometric literature has highlighted the vulnerabilities of traditional TWFE estimators, particularly regarding heterogeneous treatment effects and endpoint binning bias in staggered rollout designs [5,16]. By adhering to recent standards in revisiting event-study designs [17] and employing the heterogeneity-robust staggered DID estimator of [6], we mitigate potential TWFE contamination and ensure reliable dynamic estimation.

To overcome the aforementioned endogeneity trap, our identification strategy is anchored in the literature on historical persistence [18]. Top-tier literature demonstrates the long-term economic persistence of historical institutions and culture, such as the profound impact of Confucian academies and the civil examination system on regional development [7]. To plausibly satisfy the exclusion restriction of our historical Instrumental Variable (IV-2SLS), we draw upon classic debates distinguishing the effects of deep-rooted institutions from human capital [19]. Specifically, alongside controlling for modern secondary school education to account for the human capital transmission channel, we incorporate the plausibly exogenous IV framework of Conley et al. [8] and Hansen *J* overidentification tests [9] to systematically evaluate the sensitivity and validity of our historical instrument.

2.1.4. Identification of the Research Gap

Recent literature has increasingly recognized the macroeconomic significance of cultural heritage. For instance, Zeng and Wang [3] demonstrated the positive role of tangible World Cultural Heritage (WCH) sites in boosting urban economic growth utilizing a standard DID framework. However, unlike internationally recognized WCH, National ICH is deeply intertwined with local governments' strategic policy applications. Existing studies predominantly rely on standard DID estimators, which are highly susceptible to the aforementioned "negative selection bias," structurally downward-biasing the policy effects.

Building upon this inquiry, our study diverges from the existing literature by targeting the macroeconomic dividends of the ICH policy through an IV-2SLS strategy. To achieve credible causal identification, our IV relies on the historical distribution of Confucian academies and temples. A critical threat to the exclusion restriction of this historical IV is the persistence of human capital: regions with strong historical educational institutions often exhibit higher modern educational attainment, which is a well-established direct driver of contemporary economic growth [7,19]. To address this, we explicitly control for the natural logarithm of the number of secondary school students. By partialling out the confounding effects of modern human capital accumulation, we plausibly shut down this alternative transmission pathway, ensuring that our historical IV reliably captures contemporary economic growth predominantly through the cultural heritage mechanism. Furthermore, the plausibility of this specification and our identification strategy is supported by additional sensitivity checks as noted above.

2.2. Theoretical Mechanisms and Research Hypotheses

Building upon the existing literature on cultural economics and structural transformation, we further delineate the theoretical mechanisms through which ICH empowers urban economic development. First, from the perspective of the brand premium effect, ICH endowments embody profound historical, artistic, and cultural values that can be

commercialized to mitigate product differentiation hurdles. In traditional manufacturing and characteristic industries, official ICH designation acts as a credible quality signal that alleviates asymmetric information between producers and consumers. This signaling mechanism enables local enterprises to command higher value-added margins, expand operational revenues, and generate robust macroeconomic momentum for urban per capita GDP growth.

Second, from the perspective of transaction cost reduction, the embedding of ICH within regional industrial clusters fosters shared cultural norms, trust, and localized social networks [20]. These informal institutions significantly lower search, contracting, and monitoring costs among supply chain participants. Consequently, instead of merely stimulating low-end tourism and service sectors (the tertiary industry), this reduction in transaction frictions directs policy rents and labor resources toward scale-intensive real economy sectors, particularly characteristic manufacturing (the secondary industry). Furthermore, in accordance with the law of diminishing marginal returns, the magnitude of these cultural dividends is contingent upon a city's initial economic and industrial endowments. While heavily advanced cities already possess saturated manufacturing ecosystems and diverse growth engines, less-developed cities with lagging industrial foundations can leverage official ICH branding as a scarce comparative advantage, thereby unlocking much stronger marginal catalytic effects. Based on these theoretical insights, we propose the following three research hypotheses:

H1. *The designation and revitalization of intangible cultural heritage exert a significant positive effect on urban economic growth, thereby robustly boosting urban per capita GDP.*

H2. *Rather than driving a "tourism-only" tertiary expansion, the ICH policy fosters real-economy restructuring by channeling labor resources into characteristic manufacturing and expanding absolute employment in the secondary industry.*

H3. *The economic empowerment effect of ICH exhibits a catch-up dynamic; cities with relatively lagging initial economic or industrial foundations experience more pronounced marginal growth dividends compared to advanced regions.*

3. Data, Variables, and Descriptive Evidence

3.1. Sample Selection and Data Sources

This study utilizes a fully balanced panel dataset of 286 prefectural-level cities in China from 2000 to 2019 ($N = 5720$). Using the official administrative divisions of mainland China at the end of 2019 as our territorial reference, the initial universe comprises all 337 prefecture-level and above administrative units (4 direct-controlled municipalities and 333 prefecture-level divisions). To ensure institutional and economic comparability, we apply a two-step filtering procedure: first, we exclude the 4 direct-controlled municipalities (Beijing, Shanghai, Tianjin, and Chongqing) and 40 non-standard prefecture-level administrative divisions (comprising 30 autonomous prefectures, 7 prefectures, and 3 leagues), leaving 293 standard prefectural-level cities. Second, we exclude 7 prefectural-level cities (including 6 cities in the Tibet Autonomous Region and Sansha City in Hainan Province) due to a severe lack of historical macroeconomic statistics across the sample period. This systematic procedure yields our final sample of 286 prefectural-level cities.

The core variables are constructed from three authoritative sources: First, the data for the independent variable—the cumulative number of National Intangible Cultural Heritage (ICH) items—are manually collected and aggregated from the official China Intangible Cultural Heritage website. According to the State Council, the first four batches of national

ICH lists were published in 2006, 2008, 2011, and 2014, respectively. Second, the city-level macroeconomic and social development data—including the dependent variable (per capita GDP), the control variable (the number of secondary school students, proxying for modern human capital), and the employment populations across the primary, secondary, and tertiary industries—are extracted from the corresponding years of the *China City Statistical Yearbook*. Third, the historical endowment data used to construct the instrumental variable (i.e., the historical count of academies and Confucius temples) are obtained from the Confucian Culture Database (CFCN). Using the prefecture-level cities as of the end of 2019 as our baseline, we aggregate the data for any prior prefecture-level city mergers (such as Laiwu and Chaohu) into the corresponding post-merger prefectures. For the 286 retained cities, a small number of sporadic missing values in the *China City Statistical Yearbook* are imputed using linear interpolation. Consequently, the final dataset forms a strictly balanced panel of 5720 city-year observations (286 cities × 20 years) with no missing values.

As outlined in the introduction, we parsimoniously employ the natural logarithm of modern secondary school enrollment (*ln_edu*) as our primary time-varying control variable in the baseline specification, alongside two-way fixed effects, to help satisfy the IV's exclusion restriction. This streamlined baseline specification is intentionally designed to align with modern causal inference principles based on two critical considerations.

First, regarding the measurement precision of the confounding human capital channel, we specifically employ the scale of secondary school students rather than primary or tertiary students. In the Chinese urban context, primary education is universally compulsory, thereby lacking sufficient regional variance to capture human capital disparities. Conversely, college students exhibit high spatial mobility, reflecting a city's higher education infrastructure rather than the endogenous human capital of its resident labor force. Secondary school enrollment, heavily tethered to local household registration (Hukou) and reflecting intensive educational sorting mechanisms (e.g., the high school entrance examination), serves as a reliable and spatially sticky indicator of a region's local educational investment.

Second, this parsimonious setup helps circumvent the "bad control" problem commonly cautioned against in modern econometrics [21]. In our estimation framework, unobserved time-invariant regional characteristics (e.g., geography, innate endowments) and macroscopic temporal shocks are absorbed by the City and Year fixed effects. Introducing additional contemporaneous economic indicators—such as foreign direct investment, government expenditure, or infrastructural density—could compromise the identification, as these variables are susceptible to being post-treatment outcomes simultaneously influenced by the ICH policy itself. Therefore, pairing a targeted human capital proxy with two-way fixed effects provides a credible baseline specification that mitigates over-controlling bias. Furthermore, to address potential concerns that *ln_edu* itself might be partially endogenous or insufficient to block other time-varying persistent channels, we supplement this baseline setup in Section 6.2 with pre-determined baseline trend controls, Hansen *J* overidentification tests [9], and the plausibly exogenous bounds of Conley [8].

3.2. Rationale for the 2000–2019 Sample Period

The panel time frame for this study is set to the period from 2000 to 2019. This specific temporal boundary is deliberately chosen based on rigorous causal inference logic and macroeconomic realities, primarily driven by three critical considerations:

1. Establishing a Sufficient Pre-treatment Window: A fundamental prerequisite for evaluating multi-period DID models is verifying the parallel trend assumption before the policy intervention. Since the State Council announced the first batch of national ICH items in 2006, extending the sample start year back to 2000 provides a substantial 6-year (2000–2005) "clean" pre-treatment window. This allows the event-study design to

adequately identify and rule out any confounding pre-existing trends, thereby enhancing the internal validity of the causal identification.

2. Capturing Long-run Dynamic Effects: The 2000–2019 period effectively encompasses the rollout of the first four batches of national ICH designations (2006, 2008, 2011, and 2014). By the end of 2019, the initial batch had a 13-year policy fermentation period, and even the fourth batch had been implemented for 5 years. This extended post-treatment tracking period provides a valuable observation window for adequately estimating the long-term, dynamic economic dividends generated by the ICH policy through channels such as brand premium and industrial upgrading.

3. Isolating Severe Exogenous Macroeconomic Shocks: Truncating the sample at 2019 serves as a crucial "firewall" for our identification strategy. The outbreak of the COVID-19 pandemic in early 2020 inflicted unprecedented exogenous shocks on the global and Chinese macroeconomy, causing massive disruptions, particularly in the cultural and tourism sectors. Including data from 2020 onwards would introduce severe omitted variable bias and confounding effects that would confound the estimated economic dividends of the ICH policy. Furthermore, the fifth batch of national ICH items was announced in 2021; locking the timeline at 2019 cleanly excludes this latest batch, preventing the entanglement of anomalous pandemic-era economic fluctuations with our policy evaluation.

3.3. Descriptive Statistics and Spatial Distribution

Table 1 presents the descriptive statistics for the main variables used in this study. Following the two-step sample filtering and the linear interpolation of sporadic missing values detailed in Section 3.1, the final dataset constitutes a fully balanced panel of 5720 city-year observations, spanning all 286 retained prefectural-level cities across the 20-year period from 2000 to 2019 ($286 \times 20 = 5720$).

Table 1. Descriptive statistics of the main variables.

Variable	Obs	Mean	Std. Dev.	Min	Max
ln_gdp	5720	9.984	0.926	7.056	12.223
pShock	5720	0.630	0.483	0.000	1.000
number	5720	4.306	6.034	0.000	47.000
ln_edu	5720	12.140	0.728	8.949	13.732
final_iv_asinh	5720	26.371	23.190	0.000	101.504

The dependent variable, the natural logarithm of per capita GDP (*ln_gdp*), has a mean of 9.984 and a standard deviation of 0.926, reflecting substantial economic disparities among the cities over the two decades. Regarding the policy variables, the binary shock indicator (*pShock*) has a mean value of 0.630, suggesting that approximately 63.0% of the total observations fall into the post-treatment category (i.e., possessing at least one national ICH item). Crucially, the continuous policy variable, the cumulative number of ICH items (*number*), reveals a mean of 4.306 but a remarkably high standard deviation of 6.034, with values ranging from 0 to 47. This extreme variation strongly justifies our empirical strategy of favoring the continuous DID specification over the binary one, as the extensive margin (*pShock*) fails to capture the immense differences in policy intensity. Finally, the instrumental variable (*final_iv_asinh*) records a full set of 5720 valid observations. The high standard deviation (23.190) demonstrates rich cross-sectional variation in historical cultural endowments, laying a solid data foundation for our 2SLS causal identification.

However, while the pooled summary statistics provide a static overview of the sample, they inherently obscure the profound temporal dynamics and spatial heterogeneity of China's cultural heritage policy. To intuitively illustrate the institutional rollout process

and the geographical context underpinning our empirical design, we further map the data along both temporal and spatial dimensions.

First, regarding the temporal dimension, Figure 1 visually demonstrates that the cumulative number of national ICH items per city exhibits a distinct step-wise growth pattern. The four vertical dashed lines correspond to the official announcements of the four major batches of national ICH lists by the State Council in 2006, 2008, 2011, and 2014. This staggered rollout process perfectly captures the dynamic nature of our policy shock, theoretically and empirically validating our choice of a multi-period DID estimation framework.

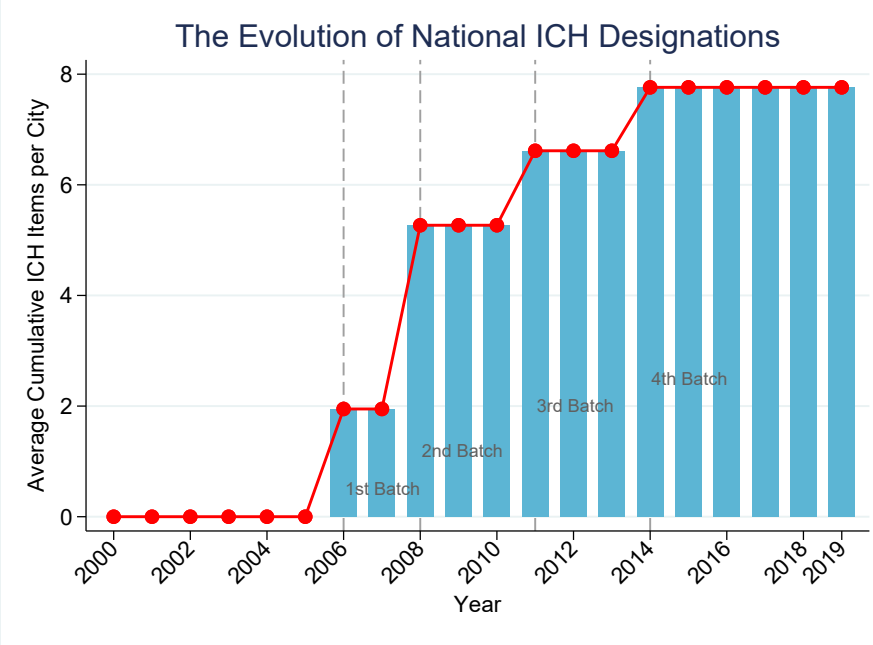


Figure 1. The evolution of national Intangible Cultural Heritage (ICH) designations (2000–2019).

Second, extending beyond the temporal rollout, Figure 2 presents the spatial distribution of ICH endowments alongside regional economic development across Chinese prefectural-level cities in 2019.

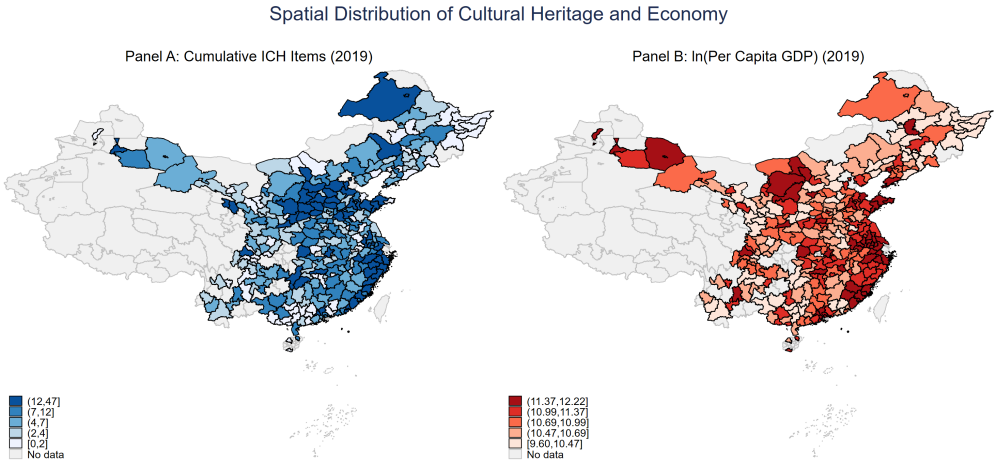


Figure 2. Spatial distribution of Intangible Cultural Heritage (ICH) endowments and per capita GDP across Chinese prefectural-level cities (2019).

Panel A displays the cumulative number of approved national ICH items. The geographical clustering reveals that regions with the richest cultural endowments are predominantly located in the central and western inland areas, as well as in historically significant

traditional hubs. In contrast, Panel B maps the regional economic landscape using the natural logarithm of per capita GDP. The visual gradient demonstrates a well-documented economic pattern: the most economically vibrant and industrialized regions are highly concentrated along the eastern coastal corridors.

Comparing the two panels reveals a clear spatial divergence. The geographic clusters of dense cultural heritage do not structurally align with the clusters of contemporary economic prosperity. This objective spatial variance visually underscores the methodological necessity of employing an instrumental variable (IV) strategy in our subsequent econometric analysis. Relying solely on raw, cross-sectional correlations would likely yield biased estimates due to these inherent regional heterogeneities.

4. Empirical Strategy

4.1. Baseline Difference-in-Differences Model

To evaluate the causal impact of the National ICH policy on the macroeconomic growth of prefectural-level cities, we estimate the following multi-period DID model with two-way fixed effects:

$$\ln GDP_{it} = \alpha_0 + \beta_1 pShock_{it} + \gamma \ln edu_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (1)$$

Furthermore, while Equation (1) captures the extensive margin of the policy (i.e., whether a city is treated), it inherently obscures the cumulative nature of cultural endowments. A city granted multiple ICH items over the years is expected to attract substantially more cultural tourism and fiscal subsidies than a city with only a single item. To accurately capture this intensive margin and preserve the variation in policy intensity, we replace the binary treatment indicator with a continuous variable representing the cumulative count of ICH items. This continuous DID model, which serves as our primary specification for subsequent analysis, is defined as:

$$\ln GDP_{it} = \alpha_0 + \beta_2 number_{it} + \gamma \ln edu_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (2)$$

where i and t index city and year, respectively. The dependent variable, $\ln GDP_{it}$, is the natural logarithm of per capita GDP for city i in year t. $pShock_{it}$ is a dummy variable that equals 1 if city i has been granted at least one national ICH item by year t, and 0 otherwise. $number_{it}$ denotes the cumulative number of approved national ICH items in city i up to year t. We include $\ln edu_{it}$, the natural logarithm of the number of secondary school students, to control for the confounding effects of modern human capital. μ_i and λ_t represent city fixed effects and year fixed effects, respectively, which absorb time-invariant unobserved city characteristics and macroeconomic shocks common to all cities. ε_{it} is the error term.

4.2. Event Study and Parallel Trend Test

A fundamental assumption of the DID approach is that the treatment and control groups would have followed parallel trends in the absence of the policy. To test this assumption and examine the dynamic effects of the ICH policy, we employ an event-study design:

$$\ln GDP_{it} = \alpha_0 + \sum_{k=-5, k\neq -1}^{5} \beta_k D_{it}^k + \gamma \ln edu_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (3)$$

where D_{it}^k is a set of relative time dummy variables. Let s_i be the year when city i received its first national ICH designation; then $k = t - s_i$ indicates the number of years relative to the initial treatment. Following standard literature [22], we exclude the year immediately preceding the policy ($k = -1$) as the baseline reference period. The coefficients β_k capture the dynamic effects of the ICH policy k years after (or before) its implementation. To mitigate

the potential endpoint binning bias often associated with heterogeneous treatment effects in staggered DID models (e.g., mixing long-run effects with never-treated comparisons), we restrict the estimation sample for this event study to the event window $k \in [-5, 5]$, alongside the pure never-treated control group. This method is commonly used in the literature [5,23].

4.3. Instrumental Variable Approach (IV-2SLS)

The baseline DID estimates may suffer from endogeneity issues, primarily due to self-selection bias (e.g., cities with different economic trajectories may possess varying fiscal capacities to apply for ICH status). To address this, we introduce an instrumental variable (IV) approach, utilizing historical cultural endowments. Specifically, we interact a cross-sectional historical Confucian culture index (comprising the historical count of schools, academies, and Confucius temples) with a linear time trend. The Two-Stage Least Squares (2SLS) models are specified as follows:

First Stage:

$$number_{it} = \rho_0 + \theta IV_{it} + \rho_1 \ln edu_{it} + \mu_i + \lambda_t + \epsilon_{it} \tag{4}$$

Second Stage:

$$\ln GDP_{it} = \alpha_0 + \beta_{IV} \widehat{number}_{it} + \gamma \ln edu_{it} + \mu_i + \lambda_t + \upsilon_{it} \tag{5}$$

where the instrument is constructed as $IV_{it} = \text{asinh}(confucian_i) \times (t - 1999)$. The inverse hyperbolic sine transformation, $\text{asinh}(\cdot)$, is applied to retain cities with zero historical sites while smoothing extreme values [24]. Crucially, by controlling for modern education ($\ln edu_{it}$), we block the channel through which historical academies might affect current economic growth via modern human capital accumulation, thereby helping to satisfy the exclusion restriction. $\widehat{number}_{it}$ is the fitted value from the first stage, and β_{IV} identifies the plausible causal effect.

5. Empirical Results and Discussion

5.1. Baseline and Dynamic Effects

The baseline estimation results are shown in Table 2.

Table 2 reports the baseline empirical results using the multi-period DID model. Column (1) estimates the extensive margin using the binary policy shock (*pShock*), while Column (2) captures the intensive margin utilizing the cumulative count of ICH items (*number*). Both specifications control for modern human capital (*ln_edu*) and two-way fixed effects. Contrary to conventional expectations, the estimated coefficient for *pShock* is statistically insignificant, and *number* displays a statistically significant negative sign (-0.005, $t = -2.41$). However, this initial negative or insignificant result is highly consistent with our theoretical intuition regarding the endogeneity of policy adoption. Specifically, the designation of national ICH status is not randomly assigned. Local governments suffer from a potential "negative selection bias": cities experiencing economic bottlenecks, industrial transitions, or slower growth momentum may possess stronger incentives to actively apply for national cultural heritage status. Local governments often view cultural branding as an alternative revitalization strategy to secure fiscal transfers and stimulate local industries. This endogenous selective application can downward-bias standard TWFE estimators, potentially masking the positive economic dividends of the ICH policy.

Table 2. Baseline DID estimation results for the impact of ICH on urban economic growth.

Variables	(1) ln *GDP*	(2) ln *GDP*
pShock (Binary)	0.012	
	(0.46)	
number (Count)		−0.005 **
		(−2.41)
ln_edu	−0.076 **	−0.075 **
	(−2.47)	(−2.44)
City FE	Yes	Yes
Year FE	Yes	Yes
Observations	5720	5720
R-squared	0.969	0.969

Notes: Robust t-statistics clustered at the city level are reported in parentheses. ** $p < 0.05$.

Before introducing our instrumental variable strategy to address this selection bias, it is necessary to examine the dynamic trajectory of the policy and verify whether the parallel trend assumption holds within the baseline DID framework. The results of the baseline event-study analysis are presented in Figure 3.

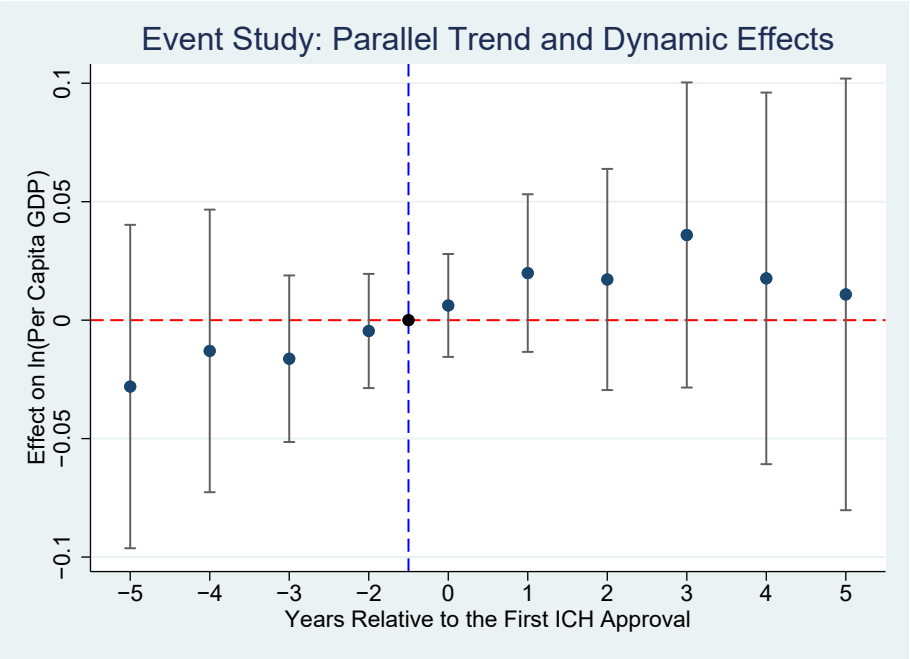


Figure 3. Event study and parallel trend test of the ICH policy. The red dashed line represents the zero-effect baseline, and the blue dashed line indicates the year of policy implementation (relative year 0). The estimation sample is restricted to the $[-5, 5]$ event window to mitigate endpoint binning bias.

As illustrated in Figure 3, the estimated coefficients for all pre-treatment periods ($t = -5$ to $t = -2$) are statistically insignificant and fluctuate closely around the zero baseline, indicating no discernible pre-existing diverging trends between treated and control cities prior to the ICH designation. Following policy implementation ($t \geq 0$), the dynamic coefficients shift upward into positive territory, although the confidence intervals remain relatively wide due to the aforementioned negative selection bias.

To further ensure that these pre-treatment parallel trends are not distorted by the negative-weighting bias inherent in traditional TWFE event studies under staggered adoption [5], we cross-validate our dynamic specification using the cutting-edge heterogeneity-robust estimator developed in [6], termed CSDID hereinafter. Using never-treated cities as a clean control group, the dynamic event-study estimates and 95% confidence intervals derived from this approach are illustrated in Figure 4.

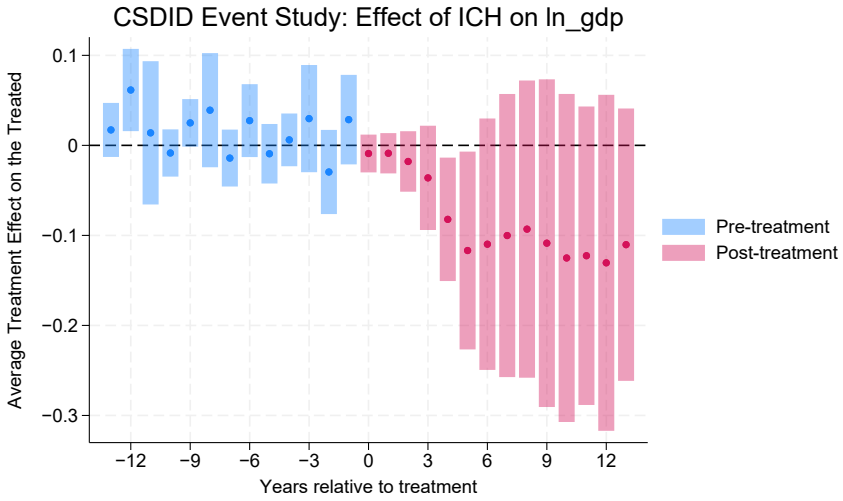


Figure 4. Event study results based on the Callaway and Sant'Anna [6] estimator.

As demonstrated in Figure 4, the formal CSDID pre-trend test yields a joint chi-square statistic with $p = 0.5666$. Apart from a minor endpoint fluctuation at the distant horizon ($t = -12$) driven by small cohort sample sizes in the earliest years, all pre-treatment coefficients throughout the eleven consecutive years immediately preceding ICH designation ($t = -11$ to $t = -1$) are statistically indistinguishable from zero, fluctuating smoothly around the zero baseline. This pattern provides robust validation of the parallel trends assumption, confirming that treatment and control cities exhibited comparable evolutionary trajectories prior to the policy shock after eliminating potential TWFE contamination.

Following the policy implementation, the uninstrumented post-treatment trajectory in Figure 4 exhibits a downward-skewed pattern that remains largely statistically insignificant across most horizons (yielding an overall post-treatment ATT (average treatment effect on the treated) of -0.082, $p = 0.138$, with only transitory dips around $t = 4$ and $t = 5$). Rather than indicating an adverse policy impact, this downward-biased trajectory aligns closely with the endogenous "negative selection bias" noted in our baseline models—where economically lagging cities or those undergoing structural bottlenecks tended to actively pursue national heritage status as an alternative revitalization stimulus. Because neither standard TWFE nor CSDID resolves the endogeneity arising from non-random policy applications across cities, relying solely on DID-type estimators remains insufficient. To mitigate this confounding reverse-causality bias and achieve more credible causal identification, we next introduce our plausibly exogenous historical IV-2SLS strategy.

5.2. IV-2SLS Results and Economic Significance

Table 3 reports the estimation results of the Two-Stage Least Squares (IV-2SLS) model. Once endogeneity is addressed using the historical Confucian index instrument, a significant positive impact of the ICH policy emerges.

The first-stage results strongly support our instrument choice: the Kleibergen-Paap rk LM statistic is 37.697 ($p < 0.001$), rejecting the underidentification null hypothesis. More importantly, the Kleibergen-Paap rk Wald F-statistic reaches 46.168, which substantially exceeds the Stock-Yogo 10% critical value of 16.38, alleviating weak-instrument concerns.

In the second stage, the coefficient on *number* is 0.016 and statistically significant at the 5% level ($p = 0.029$, $t = 2.20$). This result implies meaningful economic significance: holding modern human capital and fixed effects constant, each additional national Intangible Cultural Heritage item approved in a city is associated with an approximately 1.6% increase in its per capita GDP, lending empirical support to Hypothesis 1. This finding indicates that cultural heritage is not merely a historical relic to be preserved, but an active catalyst for modern urban macroeconomic growth, driving regional economies through brand premiums and characteristic industrial upgrading.

Crucially, the notable reversal from the negative and insignificant baseline estimates to these positive and significant IV estimates corroborates our theoretical intuition regarding "negative selection bias." Under the standard TWFE framework, the positive economic dividends of the ICH policy were masked by the endogenous self-selection of economically disadvantaged or transitioning cities, which actively seek cultural heritage branding as a substitute growth engine. By utilizing a plausibly exogenous historical instrument, we mitigate this structural downward bias, demonstrating that traditional cultural endowments, when officially recognized, serve as an effective driver for contemporary urban economies.

Table 3. Instrumental variable (IV-2SLS) estimation results on the full sample.

Variables	**IV-2SLS: ln *GDP***
number (Cumulative ICH)	0.016 **
	(2.20)
ln_edu	−0.077 **
	(−2.47)
City FE	Yes
Year FE	Yes
Observations	5720
Underidentification (K-P rk LM stat.)	37.697 ***
Weak IV test (K-P Wald F stat.)	46.168

Notes: Robust t-statistics clustered at the city level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$. K-P refers to the Kleibergen-Paap robust statistics. The Stock-Yogo 10% maximal IV size critical value for the weak instrument test is 16.38.

To intuitively illustrate how unobserved endogeneity downwardly biases the economic evaluation of cultural heritage, Figure 5 plots the scatter distribution alongside two distinct linear fitted lines. The red dashed line represents the biased OLS estimation, which exhibits a relatively flat positive slope. This attenuation is a classic manifestation of negative selection bias: cities confronting economic bottlenecks often exhibit a stronger administrative incentive to apply for national ICH titles as an alternative revitalization strategy, thereby masking the true economic dividends in raw data.

In sharp contrast, the solid navy line depicts the IV-2SLS fitted trajectory, plotted using the exogenous variation in ICH designations isolated by the historical Confucian academy instrument. Once the endogenous administrative incentives are purged, the causal IV fitted line reveals a substantially steeper upward slope. This powerful visual divergence confirms that the conventional OLS approach severely underestimates the macroeconomic benefits of the ICH policy, rigorously validating the necessity and effectiveness of our instrumental variable strategy.

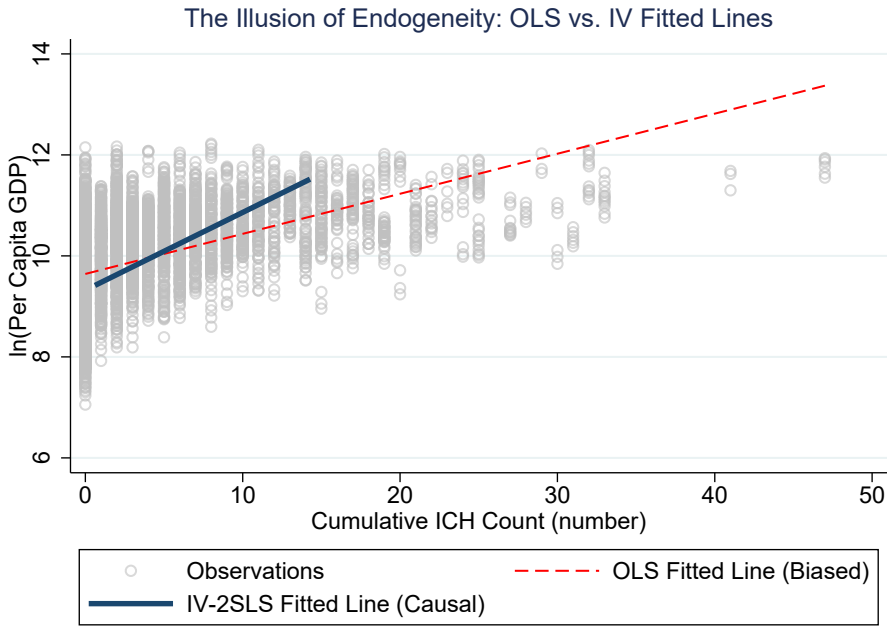


Figure 5. The illusion of endogeneity: Comparison of OLS and IV-2SLS fitted lines.

6. Robustness Checks

6.1. Robustness Checks for IV-2SLS Estimations

To ensure the reliability of our causal identification strategy, we conduct an extensive array of robustness checks in Table 4, addressing potential concerns regarding extreme outliers, spatial and administrative hierarchy, alternative treatment definitions, pre-existing city-specific economic trends, and policy anticipation effects.

As shown in Table 4, our core findings exhibit remarkable stability across all specifications. First, to rule out the influence of extreme values, Column (2) winsorizes all continuous variables at the 1% level, yielding a consistent positive coefficient ($0.017, p < 0.05$). Second, in China's regional governance system, provincial capitals and sub-provincial cities typically enjoy privileged policy quotas, stronger administrative implementation capacities, and exert spatial siphoning effects on neighboring prefectures. Excluding these 25 core cities in Column (3) produces an even slightly larger estimate ($0.019, p < 0.05$), confirming that our results are not driven by administrative hierarchy or core-periphery spatial dependence.

Third, in Column (4), we replace the cumulative count of ICH projects with its inverse hyperbolic sine transformation (*asinh_number*) to account for potential right-skewness in treatment intensity. The coefficient remains positive and statistically significant ($0.103, p < 0.05$), supported by a strong Kleibergen-Paap Wald F-statistic of 62.672. Fourth, in Column (5), to address concerns that cities with different initial endowments might follow divergent macroeconomic growth trajectories—and to prevent the time-varying educational control from acting as a "bad control" affected by post-treatment economic feedback—we replace the contemporaneous control with predetermined Year 2000 baseline economic and educational levels interacted with linear time trends (*trend_base_gdp* and *trend_base_edu*). Notably, the negative and highly significant coefficient on *trend_base_gdp* ($-0.028, p < 0.01$) captures regional macroeconomic convergence, while the effect of *number* becomes even more pronounced in both magnitude and significance ($0.034, p < 0.01$). Finally, Column (6) incorporates the one-period lead of the policy shock (*lead*1*_pShock*) to test for potential anticipation effects prior to official designation. The lead term is statistically indistinguishable from zero ($-0.007, t = -0.27$), whereas the main ICH coefficient remains unchanged at 0.016 ($p < 0.05$), ruling out confounding pre-policy anticipation behaviors.

Table 4. Comprehensive robustness checks for the IV-2SLS estimations.

Variables	(1) Base	(2) Winsor	(3) No Cap.	(4) Alt. Treat	(5) Pre-Trend	(6) Lead
number	0.016 **	0.017 **	0.019 **		0.034 ***	0.016 **
	(2.20)	(2.45)	(2.27)		(3.04)	(2.20)
asinh_number				0.103 **		
				(2.33)		
ln_edu	−0.077 **	−0.097 ***	−0.067 **	−0.081 ***		−0.077 **
	(−2.47)	(−3.13)	(−2.08)	(−2.63)		(−2.47)
trend_base_edu					−0.008 **	
					(−2.43)	
trend_base_gdp					−0.028 ***	
					(−8.64)	
*lead*1_*pShock*						−0.007
						(−0.27)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5720	5720	5220	5720	5720	5720
K-P LM (*p*-val)	37.70 (0.00)	37.686 (0.00)	33.195 (0.00)	46.50 (0.00)	15.99 (0.00)	37.69 (0.00)
Weak IV (K-P F)	46.168	46.113	39.777	62.672	18.969	45.775

Notes: Robust t-statistics clustered at the city level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$. Column (1) reports the baseline IV-2SLS estimate. Column (2) winsorizes continuous variables at the 1st and 99th percentiles. Column (3) excludes 25 provincial capitals and sub-provincial cities. Column (4) employs an alternative treatment definition using the inverse hyperbolic sine transformation of ICH (*asinh_number*). Column (5) controls for pre-existing city-specific economic and educational trajectories by interacting initial Year 2000 log per capita GDP and log education with linear time trends (*trend_base_gdp* and *trend_base_edu*). Column (6) tests for potential anticipation effects by incorporating a one-year lead indicator of the policy shock (*lead*1_*pShock*).

6.2. Exclusion Restriction Sensitivity and Control Variable Validity

A critical identification challenge raised in the literature on historical persistence is whether historical Confucian culture index satisfies the strict exclusion restriction—namely, affecting contemporary urban economic growth exclusively through cultural heritage accumulation rather than through unobserved direct channels. Furthermore, one might question whether the absolute number of secondary school students primarily reflects urban population scale rather than educational density. To rigorously address these concerns, we conduct a set of sensitivity analyses in Table 5, combining reduced-form estimation, the plausibly exogenous IV framework of Conley et al. [8], overidentification tests [9], and population-adjusted control specifications.

First, Column (1) of Table 5 presents the reduced-form relationship between our historical instrument ($final_iv_asinh$) and contemporary per capita GDP. The instrument exerts a positive and statistically significant reduced-form effect of 0.0021 ($t = 2.40, p = 0.017$). Building upon this benchmark, we follow Conley et al. [8] to relax the strict exclusion restriction ($\gamma = 0$) by permitting the historical instrument to exert a direct effect $\gamma > 0$ on urban economic growth. Specifically, by subtracting $\gamma \cdot final_iv_asinh$ from the dependent variable, we evaluate how much direct leakage is required to invalidate our second-stage inference. As shown in Column (2), even when we assume $\gamma = 0.0005$—meaning that nearly 25% of the total reduced-form effect ($0.0005/0.0021 \approx 23.8\%$) operates through direct, non-ICH channels—the IV estimate of *number* remains positive and statistically significant ($0.012, p < 0.10$). In further sensitivity bounds, the positive effect only loses statistical significance when γ approaches 0.0010 (where nearly half of the reduced-form effect is assumed to be a direct violation). Moreover, when we disaggregate our historical cultural proxy into two separate instruments—historical academies and Confucian temples—in Column (3),

the resulting Hansen J overidentification statistic is 0.118 with a p-value of 0.7314, failing to reject the null hypothesis of instrument coherence. Together, while these sensitivity checks cannot definitively prove strict instrument exogeneity, they provide reassuring supporting evidence for the plausibility of our identification strategy.

Table 5. Exclusion restriction sensitivity (Conley Plausexog) and population scale adjustments.

Variables	(1) Reduced-Form	(2) Conley ($\gamma = 0.0005$)	(3) Overid. Test	(4) Pop. Control
number		0.012 *	0.006	0.012 *
		(1.72)	(1.10)	(1.65)
final_iv_asinh	0.0021 **			
	(2.40)			
ln_edu	−0.079 ***	−0.077 **	−0.076 **	
	(−2.59)	(−2.51)	(−2.49)	
stu_pop_ratio				−0.224 ***
				(−6.15)
ln_tot_pop				−0.104 ***
				(−2.99)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	5720	5720	5720	5720
K-P rk LM stat. (p-val)	–	37.70 (0.00)	40.42 (0.00)	38.23 (0.00)
Weak IV (K-P Wald F)	–	46.168	27.284	43.910
Hansen J stat. (p-val)	–	–	0.118 (0.7314)	–

Notes: Robust t-statistics clustered at the city level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Column (1) reports the reduced-form OLS regression of *ln_gdp* on the historical instrument (*final_iv_asinh*). Column (2) implements the Conley et al. [8] sensitivity check by allowing a direct exclusion-restriction violation of $\gamma = 0.0005$ (approximately 25% of the total reduced-form effect). Column (3) disaggregates the historical instrument into separate academy and Confucian temple instruments to conduct the Hansen J overidentification test. Column (4) replaces absolute student enrollment with the student-to-employment ratio (*stu_pop_ratio*) and explicitly controls for total urban employment population (*ln_tot_pop*).

Second, to address the concern that absolute secondary school enrollment (*ln_edu*) may confound human capital density with overall city population size, Column (4) reconstructs the control set using the ratio of secondary school students to total urban employment population (*stu_pop_ratio*) while simultaneously controlling for the natural logarithm of total employment across the primary, secondary, and tertiary sectors (*ln_tot_pop*). Because contemporaneous industrial and service employment is itself an endogenous outcome channel of ICH revitalization (as demonstrated in Section 7), directly controlling for *ln_tot_pop* represents a highly conservative "over-control" test that absorbs part of the treatment's mediating pathway. Even under this stringent specification, the coefficient of *number* remains positive and marginally significant ($0.012, t = 1.65, p = 0.10$), with a robust Kleibergen-Paap Wald F-statistic of 43.910. Together with the predetermined baseline trend specification in Table 4 (Column 5), these results confirm that our identification is neither driven by population scale bias nor compromised by bad-control feedback.

7. Mechanism Analysis: Industrial Restructuring Toward the Real Economy

The baseline and IV estimates indicate that the ICH policy significantly boosts urban per capita GDP. However, evaluating the underlying transmission channels is essential to understanding how intangible cultural endowments translate into macroeconomic growth. To explore this, we adopt the mechanism identification strategy widely recommended in

modern applied microeconomics [21]. Traditional mediation analysis often suffers from endogeneity bias—specifically the "bad control" problem—when incorporating the mediator directly into the baseline regression, and finding multiple plausibly exogenous instrumental variables for both the policy and the mediator simultaneously is practically infeasible.

Therefore, following established causal inference practices, we employ a "dependent variable replacement" approach. Since the positive macroeconomic impact of revitalizing the real economy and expanding industrial employment is a well-established consensus in development economics, our mechanism analysis focuses on estimating the causal effect of the ICH policy (*number*) on the urban employment structure by replacing the original dependent variable (*ln_gdp*) with structural employment indicators. Specifically, to empirically test Hypothesis 2 and address the concern that sectoral employment shares may be mechanically confounded by total population fluctuations or cross-sectoral denominator effects, we specify our mechanism models using the natural logarithms of absolute employment in the secondary industry (*ln_secondPop*) and the tertiary industry (*ln_thirdPop*). By maintaining our historical IV-2SLS framework alongside baseline TWFE estimations (reported in Table 6), we ensure that the identified transmission channels effectively mitigate the aforementioned negative selection bias.

Table 6. Mechanism analysis based on absolute sectoral employment (*ln_secondPop* and *ln_thirdPop*).

Variables	**Secondary Industry (*ln_secondPop*)**		**Tertiary Industry (*ln_thirdPop*)**	
	(1) TWFE	**(2) IV-2SLS**	**(3) TWFE**	**(4) IV-2SLS**
pShock	0.156 ***		0.020	
	(3.11)		(0.69)	
number		0.110 ***		0.025 ***
		(6.25)		(3.90)
ln_edu	0.111	0.104	0.120 ***	0.119 ***
	(1.52)	(1.29)	(2.77)	(2.73)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	5720	5720	5720	5720
K-P rk LM stat. (*p*-val)	–	37.70 (0.00)	–	37.70 (0.00)
Cragg-Donald Wald F	–	930.898	–	930.898
Weak IV (K-P Wald F)	–	46.168	–	46.168

Notes: Robust t-statistics clustered at the city level are reported in parentheses. *** $p < 0.01$. Columns (1) and (2) examine the impact of ICH on the log of absolute secondary industry employment (*ln_secondPop*), while Columns (3) and (4) examine the log of absolute tertiary industry employment (*ln_thirdPop*).

The results in Table 6 provide strong empirical validation for Hypothesis 2. In the TWFE specifications, the binary policy shock (*pShock*) significantly expands absolute secondary industry employment by approximately 15.6% ($0.156, t = 3.11, p < 0.01$) in Column (1), whereas its impact on absolute tertiary industry employment in Column (3) is small and statistically insignificant ($0.020, t = 0.69$). When addressing endogeneity via our IV-2SLS framework, Column (2) demonstrates that each additional national ICH project increases absolute secondary industry employment by 11.0% ($0.110, t = 6.25, p < 0.01$), supported by a strong Kleibergen-Paap rk Wald F-statistic of 46.168. While the IV-2SLS estimate for tertiary industry employment in Column (4) is also positive and statistically significant ($0.025, t = 3.90, p < 0.01$), its economic magnitude is less than one-fourth of that observed in the secondary sector (0.025 vs. 0.110). This stark structural asymmetry indicates that China's ICH revitalization does not merely foster a "tourism-only" service expansion; rather, it drives a substantial secondary-sector employment expansion, consistent with theoretical

channels such as brand premiums and reduced transaction costs in the real economy. Note that because city-level secondary-sector employment is an aggregate macroeconomic indicator, these estimates provide suggestive evidence of broader industrial employment growth rather than directly isolating labor expansion within heritage-specific manufacturing sub-sectors.

8. Heterogeneity Analysis: Boundary Conditions and Catch-Up Effects

Having established the robust causal effect of the ICH policy and identified the revitalization of the secondary sector as its primary transmission channel, it is imperative to recognize that regional development in China is highly uneven. In accordance with Hypothesis 3 and the law of diminishing marginal returns, the marginal economic dividends of cultural branding may differ substantially across cities at different stages of development. While heavily advanced cities already possess saturated manufacturing ecosystems, lagging cities may leverage official ICH designation as a scarce comparative advantage to catalyze catch-up growth. To rigorously test these boundary conditions without incurring split-sample weak-instrument biases, we conduct two dimensions of full-sample interaction heterogeneity analysis: first, examining the moderating role of initial economic endowment (Year 2000 per capita GDP); and second, evaluating the moderating effect of initial industrial foundations (Year 2000 secondary sector employment scale).

8.1. Heterogeneity Analysis: The Role of Initial Economic Endowment

To investigate whether the economic dividends of ICH vary across cities with different developmental starting points without suffering from split-sample weak-instrument biases, we estimate a unified full-sample interaction IV-2SLS model. Specifically, we construct a binary indicator *High_GDP*, which equals 1 if a city's initial per capita GDP in the baseline year 2000 exceeds the sample median, and 0 otherwise. Both the endogenous policy variable (*number*) and the interaction term (*number* × *High_GDP*) are instrumented using *final_iv_asinh* and *final_iv_asinh* × *High_GDP*.

The estimation results in Table 7 provide compelling support for Hypothesis 3. First, the Kleibergen-Paap rk Wald F-statistic reaches 17.132 (and the Cragg-Donald Wald F-statistic reaches 379.307), well above the Stock-Yogo 10% critical value of 7.03, effectively alleviating the weak-instrument concern. Second, the main effect of *number* is 0.046 ($t = 3.77, p < 0.01$), indicating that for economically lagging cities (Low Initial GDP), each additional ICH project significantly boosts per capita GDP by approximately 4.6%. Conversely, the interaction term *number* × *High_GDP* is negative and highly significant ($-0.037, t = -4.84, p < 0.01$), reducing the net marginal effect in economically advanced cities to 0.009. This confirms that intangible cultural heritage serves as a powerful catch-up engine for less-developed regions, where marginal returns to cultural branding and characteristic manufacturing are substantially higher.

8.2. Heterogeneity Analysis: The Role of Initial Industrial Foundation

In addition to initial economic endowments, a city's pre-existing industrial foundation shapes its capacity to absorb and monetize cultural assets. Using the median of secondary industry employment (*secondPop*) in the baseline year 2000, we define the binary indicator *High_Ind* and estimate the corresponding full-sample interaction IV-2SLS specification in Table 8.

Table 7. Heterogeneity analysis based on initial economic endowment (Year 2000).

Variables	(1) Full-Sample Interaction	(2) Implied High-GDP Effect
number (Low Initial GDP Baseline)	0.046 ***	0.009
	(3.77)	[Net: 0.046 − 0.037]
number × *High_GDP*	−0.037 ***	–
	(−4.84)	–
ln_edu	−0.068 **	−0.068 **
	(−2.31)	(−2.31)
City FE	Yes	Yes
Year FE	Yes	Yes
Observations	5720	5720
K-P rk LM stat. (*p*-val)	29.03 (0.0000)	29.03 (0.0000)
Cragg-Donald Wald F stat.	379.307	379.307
Kleibergen-Paap rk Wald F stat.	17.132	17.132

Notes: Robust t-statistics clustered at the city level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$. The model is estimated using a unified full-sample interaction IV-2SLS framework ($N = 5720$). The Kleibergen-Paap rk Wald F-statistic is 17.132, exceeding the Stock-Yogo (2005) 10% maximal IV size critical value of 7.03.

Table 8. Heterogeneity analysis based on initial industrial foundation (Year 2000).

Variables	(1) Full-Sample Interaction	(2) Implied High-Ind. Effect
number (Low Initial Industry Baseline)	0.044 ***	0.012
	(3.51)	[Net: 0.044 − 0.032]
number × *High_Ind*	−0.032 ***	–
	(−4.00)	–
ln_edu	−0.092 ***	−0.092 ***
	(−2.91)	(−2.91)
City FE	Yes	Yes
Year FE	Yes	Yes
Observations	5720	5720
K-P rk LM stat. (*p*-val)	24.98 (0.0000)	24.98 (0.0000)
Cragg-Donald Wald F stat.	340.583	340.583
Kleibergen-Paap rk Wald F stat.	14.867	14.867

Notes: Robust t-statistics clustered at the city level are reported in parentheses. *** $p < 0.01$. The model is estimated using a unified full-sample interaction IV-2SLS framework ($N = 5720$). The Kleibergen-Paap rk Wald F-statistic is 14.867, exceeding the Stock-Yogo (2005) 10% maximal IV size critical value of 7.03.

The empirical findings reported in Table 8 reveal a highly consistent structural pattern. The Kleibergen-Paap rk Wald F-statistic of 14.867 (and Cragg-Donald F-statistic of 340.583) confirms strong instrument identification across the full sample. In Column (1), the marginal effect of ICH for cities with a low initial industrial foundation is positive and highly significant ($0.044, t = 3.51, p < 0.01$), whereas the interaction term *number*×*High_Ind* is significantly negative ($-0.032, t = -4.00, p < 0.01$), yielding a net marginal effect of 0.012 for heavily industrialized cities. This demonstrates that for cities lacking diversified modern manufacturing, intangible cultural heritage introduces a vital catalyst for characteristic industrial revitalization and structural upgrading.

9. Conclusions and Policy Implications

9.1. Conclusions

This study investigates the causal nexus between Intangible Cultural Heritage (ICH) policies and macroeconomic urban development, effectively challenging the prevailing

stereotype that cultural endowments solely drive the tourism and service sectors. By constructing a panel dataset of 286 Chinese prefectural-level cities from 2000 to 2019 and employing a rigorous IV-2SLS identification strategy—instrumented by the historical distribution of Ming and Qing Dynasty Confucian culture index—we successfully isolate the causal economic dividends of the ICH policy from the inherent "negative selection bias" of local administrative incentives.

Our empirical investigation yields three primary findings. First, the ICH policy acts as a robust and significant catalyst for urban macroeconomic expansion, increasing per capita GDP by approximately 1.6% per additional project and demonstrating the economic utility of cultural heritage preservation. Second, by examining absolute sectoral employment, we uncover a pronounced secondary-sector employment expansion. Rather than fostering a "tourism-only" service economy, each additional ICH project expands absolute secondary industry employment by 11.0%, more than four times its 2.5% impact on tertiary employment—indicating that heritage policies substantially support real-economy industrial employment alongside traditional services. Third, full-sample interaction heterogeneity analyses reveal a compelling "catch-up effect" governed by diminishing marginal returns. The marginal macroeconomic dividends of the ICH policy are most pronounced in cities with below-median initial economic endowments (4.6%) and lagging initial industrial foundations (4.4%), demonstrating that cultural heritage revitalization serves as a potent engine for regional convergence and latecomer industrialization.

9.2. Policy Implications

The findings of this study offer crucial, actionable insights for policymakers aiming to leverage cultural heritage for sustainable urban transition and inclusive growth:

- **Redefining the Cultural Economic Paradigm: From "Ticket Economy" to "Heritage Manufacturing".** Local governments must strategically pivot away from the superficial model of building homogeneous ancient towns or relying solely on tourism ticket revenues. Policymakers should embed ICH items directly into the real economy by establishing heritage-based manufacturing parks, subsidizing traditional craftsmanship workshops, and fostering brand premiums for local specialty processing (e.g., ceramics, textiles, brewing). Integrating cultural heritage with modern manufacturing supply chains serves as a highly effective tool to anchor the real economy and defend against the risks of premature deindustrialization.
- **Harnessing the "Catch-Up Effect" with Bundled Investments for Inclusive Regional Convergence.** Our heterogeneity analysis reveals that culturally rich but economically lagging cities experience the highest marginal returns from national ICH designations (4.6% for low-GDP cities and 4.4% for low-industry cities). This indicates that national cultural branding can effectively counteract regional disparities rather than exacerbating the "Matthew Effect." To fully consolidate and scale up these high marginal dividends in less-developed regions, higher-level governments should pair cultural designations with targeted complementary investments—such as modern logistics, digital e-commerce platforms, and standardized craft industrial parks—enabling latecomer cities to translate cultural comparative advantages into sustained industrial upgrading.
- **Facilitating the Sustainable Transition of Old Industrial Bases.** The robust complementarity between ICH and industrial foundations indicates that cultural policies can act as a "catalyst for transition" in resource-exhausted cities and old industrial bases. Instead of completely abandoning their historical heavy-industry infrastructure, these cities should be encouraged to pursue heritage-led "re-industrialization." By transforming idle factories and utilizing skilled industrial workers to scale up

heritage-based manufacturing, municipalities can achieve a greener, more sustainable industrial structure.

9.3. Limitations and Future Directions

While this study provides novel macro-level insights, it is subject to certain limitations that present avenues for future research. First, our mechanism analysis relies on city-level aggregate sectoral employment data. Future studies utilizing micro-level firm data could explore exactly how heritage enterprises innovate, survive, and compete within the manufacturing sector. Second, this study primarily focuses on the quantitative accumulation of ICH items. Future research could employ textual analysis or big data to evaluate the "commercialization rate" or "digital visibility" of specific ICH items. Finally, cultural economics is inherently geographical. Subsequent studies could employ spatial econometric models (e.g., Spatial Durbin Models) to investigate whether ICH development in one city generates positive economic spillover effects for neighboring regions or results in competitive cannibalization, thereby further enriching the spatial understanding of cultural sustainability.

Author Contributions: Conceptualization, X.J.; methodology, X.J.; software, C.S.; validation, C.S. and J.Y.; investigation, X.J.;data curation, J.Y.; writing—original draft preparation, C.S. and J.Y.; writing—review and editing, X.J.; visualization, C.S.; supervision, X.J.; project administration, J.Y.; funding acquisition, C.S. All authors have read and agreed to the published version of the manuscript.

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