

Digital Financial Inclusion and Provincial Economic Growth in China: Evidence from a Two-Way Fixed-Effects Panel, 2011–2023

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Abstract: This paper asks whether the rapid expansion of digital payment platforms such as Alipay and WeChat Pay is linked to provincial economic growth in China, and if so, through what channel. It builds a panel of all 31 provincial-level regions from 2011 to 2023 ($n = 403$), combining the Peking University Digital Financial Inclusion (DFI) Index with official provincial data on GDP, urbanization, patents, foreign direct investment, higher-education enrollment, and industrial structure. A pooled regression finds a strong positive relationship between DFI and GDP ($\beta = 0.573$, $p < 0.001$), but this weakens sharply once province and year fixed effects are added ($\beta = 0.142$, $p = 0.117$) and is confirmed insignificant by a wild cluster bootstrap ($p = 0.187$), suggesting the pooled result largely reflects a shared national trend rather than a province-specific effect. Testing technological innovation as a possible channel, DFI strongly predicts patent applications ($\beta = 0.775$, $p < 0.001$), and patents in turn predict GDP after controlling for DFI ($\beta = 0.093$, $p = 0.023$), with a significant indirect effect confirmed both analytically (Sobel $p = 0.042$) and by a percentile bootstrap (95% CI [0.007, 0.150]). This innovation channel is sensitive to further checks, however: it weakens once foreign direct investment, education, and industrial structure are added as controls (b-path p rises to 0.259), and a panel unit-root test cannot rule out non-stationarity in the patent series. An exploratory instrumental-variables estimate using each province's distance to Hangzhou finds a large, positive association between instrumented DFI and GDP, but with a first-stage F-statistic that is only borderline strong (10.8) and an exclusion restriction that cannot be verified, so it is offered as a supplementary check rather than a resolution of endogeneity. Overall, this paper finds no robust direct effect of digital financial inclusion on provincial GDP, but suggestive evidence of an innovation-related channel that does not fully survive additional controls. A separate test for moderation by urbanization is statistically unstable and is not treated as evidence.

Keywords: Digital payments, financial inclusion, economic growth, technological innovation, mediation, instrumental variables, panel data, China.

1. Introduction

1.1. Background and Motivation

Over the past decade, mobile payment platforms such as Alipay and WeChat Pay have grown from a Beijing–Shanghai phenomenon into a nationwide financial infrastructure. The Peking University Digital Financial Inclusion (DFI) Index tracks this growth at the provincial level from 2011 onward, and its most recent update shows the national median index value rising more than eleven-fold between 2011 and 2023 [1]. Over the same period, every one of China's 31 provinces recorded sustained GDP growth. The obvious question is whether these two trends are actually connected — and if they are, whether digital payments contribute to growth directly, or through some other mechanism.

Answering this question requires more than a simple national-level correlation, which cannot distinguish a real relationship from two series that happen to rise together, and cannot test any specific mechanism. This paper instead builds a province–year panel: all 31 provinces observed annually from 2011 to 2023 — the most recent year for which the DFI Index, GDP, urbanization, patent, foreign direct investment, higher-education, and industrial-structure data are all available on a consistent basis — giving 403 observations in total. This structure makes it possible to control for what is different about each province and what is common to each year, to test a specific mediating mechanism (technological innovation), and to run three further robustness checks: a set of omitted-variable controls, a panel unit-root test, and an

instrumental-variables estimate.

1.2. Research Questions and Framework

This study asks three questions:

RQ1: Once province-specific characteristics and common national trends are controlled for, is digital payment adoption and financial inclusion still related to provincial GDP?

RQ2: Does technological innovation — proxied by patent applications — mediate this relationship, and does that mediation survive the inclusion of other time-varying provincial covariates?

RQ3: Does the level of urbanization moderate the strength of this relationship?

1.2.1. Theoretical Framework

The framework tested here follows a simple logic. Digital payment platforms lower the cost of transacting and extend credit and financial services to firms and individuals who previously had limited access to them. This can raise GDP directly, by expanding consumption and easing cash-flow constraints for small businesses. But it can also work indirectly: easier access to finance may free up capital for research and product development, so that digital finance shows up in growth mainly by first showing up in innovation. Urbanization is included as a moderator because digital infrastructure, and the ability to use it, is generally denser in cities — so the same increase in digital financial inclusion might produce a larger effect in a more urbanized province than in a rural one.

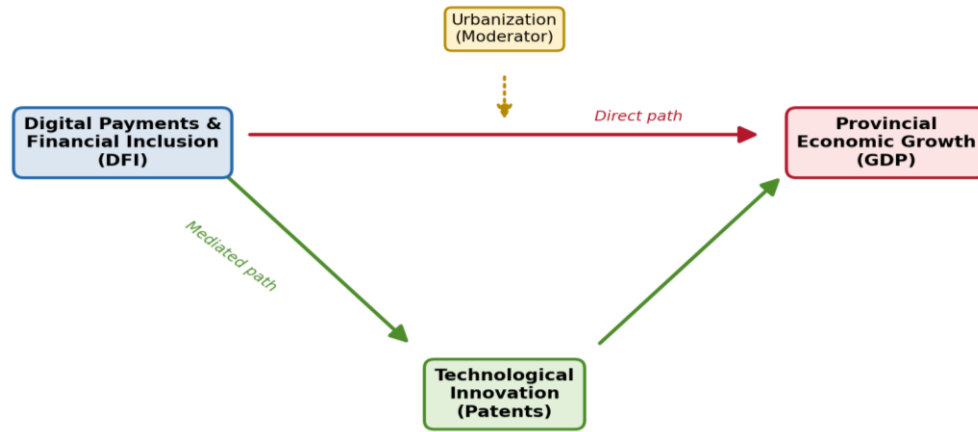


Figure 1. Conceptual framework: direct, mediated, and moderated paths

Figure 1 summarizes the three paths this paper tests: a direct path from digital payments and financial inclusion to GDP, a mediated path running through technological innovation, and urbanization entering as a moderator of the direct path.

1.2.2. Key Variables

Independent variable — Digital Payments / Financial Inclusion (DFI): measured using the Peking University–Ant Financial DFI Index, which combines breadth of coverage, depth of usage, and digitization level.

Dependent variable — Economic Growth: provincial GDP, in 100 million yuan.

Mediating variable — Technological Innovation: number of domestic patent applications accepted per province per year.

Moderating variable — Urbanization: the urban share of a province's resident population.

Control variables — Foreign direct investment (total investment of foreign-funded enterprises), higher-education enrollment per 100,000 population, and the secondary-industry share of GDP, each measured annually per province.

2. Literature Review and Hypotheses

2.1. Literature Review

2.1.1. Digital Finance and Economic Growth

A number of studies link digital financial inclusion to regional economic outcomes in China. Guo et al., who built and maintain the DFI Index used in this paper, document its steady rise across every province between 2011 and 2023, with the gap between the highest- and lowest-ranked provinces narrowing sharply in the earlier years of the sample before that convergence slowed in the most recent updates [1]. Xie et al. (2018) find that digital finance is associated with more entrepreneurial activity, especially in regions where traditional banking is weaker, and interpret this as evidence that digital financial infrastructure eases financing constraints for smaller, credit-constrained firms and individuals [2]. Zhang et al. (2024) find that digital financial inclusion is associated with a narrowing of urban–rural gaps in household spending [3], while Wei, Di, and Zhou (2025) show that a digital divide across cities can limit how evenly these gains are shared [4].

2.1.2. Innovation as a Transmission Channel

A separate line of work points to innovation as a plausible link between digital finance and growth. The financing-constraint mechanism documented by Xie et al. (2018) — cheaper access to credit and payment infrastructure lowering

the cost of financing for smaller, credit-constrained firms [2] — gives a specific channel to test, rather than a general claim that digital finance and growth move together: if digital finance eases financing constraints, it should show up first in the innovative and entrepreneurial activity that financing constraints normally suppress, and only later, if at all, in aggregate output. That is the channel this paper investigates directly using province-level patent data.

2.1.3. A Methodological Caution

A separate, more methodological literature warns against reading a strong correlation between two rising series as evidence of a causal link. Granger and Newbold (1974) formalized this problem for trending time series [5], which matters here because both the DFI Index and provincial GDP rise almost every year of the sample. Kerola and Mojon (2021) add a China-specific warning: provincial GDP figures, summed across provinces, exceeded the national total by a widening margin for years before a 2019–2020 statistical revision, so provincial panels need to be handled carefully [6]. Khera et al. (2021), using cross-country IMF data, find that the growth effects of digital financial inclusion are generally positive but modest and vary a lot by country [7] — a further reason to test this relationship with proper panel controls rather than a raw correlation.

2.1.4. Urbanization as a Moderating Factor

Finally, NBS's own urbanization series and related work by the Peterson Institute for International Economics document large, persistent gaps in urbanization across Chinese provinces — from under 25% in Tibet to close to 90% in Shanghai over this sample period [8, 9]. Li et al. (2022) find that the consumption effects of digital financial inclusion vary spatially in ways correlated with regional development [10], which motivates testing urbanization as a moderator here.

2.1.5. Identifying Causal Effects: The Distance-to- Hangzhou Instrument

A recurring concern with any cross-sectional or panel estimate of the DFI–GDP relationship is reverse causality: provinces that are already growing quickly, or innovating more, may simply adopt digital finance faster, rather than digital finance driving growth. The DFI Index's own developers document that digital financial inclusion in China is organized around Hangzhou — headquarters of Ant Group, the operator of Alipay — as a geographic center, with DFI levels declining systematically in cities farther from Hangzhou even after controlling for year effects [1]. Zhang, Wan, Zhang, and He (2019) exploit this pattern directly, using

each city's distance to Hangzhou as an instrument for digital financial inclusion in a study of inclusive growth; that paper is, by download count, the most-downloaded article in the history of Economic Research Journal, China's leading economics journal, and its instrument has been widely adopted in the subsequent literature [15]. Because distance to Hangzhou is fixed and plausibly unrelated to a province's own growth prospects except through its effect on digital-finance adoption, it offers a way to check — though not conclusively resolve — whether the DFI–GDP association survives an instrumental-variables approach. Section 3.5 and Section 5.5 apply this instrument at the provincial level.

2.2. Hypotheses

H1: Digital payment adoption and financial inclusion have a positive effect on provincial GDP, net of province-specific characteristics and common national trends.

H2: Technological innovation, proxied by patent applications, mediates the relationship between digital financial inclusion and GDP.

H3: Urbanization may moderate the effect of digital financial inclusion on GDP, with a stronger effect expected in more urbanized provinces.

3. Methodology

3.1. Baseline Regression Model

To examine the relationship between digital financial inclusion and provincial GDP, this study begins with a pooled log-linear regression:

$$\ln(\text{GDP})_{it} = \beta_0 + \beta_1 \ln(\text{DFI})_{it} + \varepsilon_{it} \quad (1)$$

Where i indexes province ($i = 1, \dots, 31$) and t indexes year ($t = 2011, \dots, 2023$), so that GDP_{it} is Gross Domestic Product of province i in year t (100 million yuan), DFI_{it} is the Digital Financial Inclusion Index of province i in year t , β_1 is the elasticity of GDP with respect to DFI, and ε_{it} is the error term.

A log–log form is used specifically so that β_1 can be read directly as an elasticity. Provincial GDP level (rather than GDP per capita) is used as the dependent variable because the province and year fixed effects in Section 3.2 absorb time-invariant differences in population size and density across provinces, so β_1 is identified from within-province growth over time rather than from cross-sectional differences in scale.

3.2. Fixed-Effects Panel Model

The pooled model above cannot separate a genuine relationship from two things that simply happen to move together: differences between provinces that never change (geography, industrial base, history) and shocks that hit every province in the same year (national policy, a nationwide statistical revision). To remove both, the model is extended to a two-way fixed-effects specification:

$$\ln(\text{GDP})_{it} = \alpha + \beta_1 \ln(\text{DFI})_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (2)$$

Where μ_i is a province fixed effect that absorbs anything about province i that does not change over time, and λ_t is a year fixed effect that absorbs anything common to all 31 provinces in year t . With both terms included, β_1 is identified only from within-province, year-on-year variation, which makes this a substantially stricter test of the DFI–GDP relationship than the pooled model. Standard errors are clustered by province throughout, and Section 5.1 additionally reports a wild cluster bootstrap given the small

number of clusters (31).

3.3. Mediation Model: Technological Innovation

To test whether technological innovation mediates the DFI–GDP relationship (H2), this study follows the three-step approach of Baron and Kenny (1986) [11], applied within the same fixed-effects panel:

Step 1 (total effect):

$$\ln(\text{GDP})_{it} = \alpha_1 + c \cdot \ln(\text{DFI})_{it} + \mu_i + \lambda_t + \varepsilon_{1it} \quad (3)$$

Step 2 (a-path):

$$\ln(\text{Patents})_{it} = \alpha_2 + a \cdot \ln(\text{DFI})_{it} + \mu_i + \lambda_t + \varepsilon_{2it} \quad (4)$$

Step 3 (b-path and remaining direct effect c'):

$$\ln(\text{GDP})_{it} = \alpha_3 + c' \cdot \ln(\text{DFI})_{it} + b \cdot \ln(\text{Patents})_{it} + \mu_i + \lambda_t + \varepsilon_{3it} \quad (5)$$

Where Patents_{it} is domestic patent applications accepted in province i , year t ; a is the effect of DFI on Patents; b is the effect of Patents on GDP, controlling for DFI; and c' is the direct effect of DFI on GDP, controlling for Patents. The primary evidence for mediation in this paper is not the stepwise significance pattern of a , b , and c' by itself — which is now understood to be an unreliable basis for concluding mediation on its own — but the indirect effect $a \cdot b$, tested directly via the Sobel (1982) standard error and corroborated with a percentile bootstrap in Section 5.2. The three-step decomposition is retained because it is informative about which path (a or b) is doing the work, not as the sole test of mediation.

3.4. Moderation Model: Urbanization

To test H3, an interaction term between DFI and urbanization is added to the two-way fixed-effects model:

$$\ln(\text{GDP})_{it} = \alpha_4 + \beta_1 \ln(\text{DFI})_{it} + \beta_2 \text{Urban}_{it} + \beta_3 (\ln(\text{DFI})_{it} \times \text{Urban}_{it}) + \mu_i + \lambda_t + \varepsilon_{it} \quad (6)$$

Where Urban_{it} is the urban share of the resident population of province i in year t (%). A statistically significant β_3 would indicate that the strength of the DFI–GDP relationship depends on how urbanized a province is.

3.5. Additional Robustness Checks

Three additional checks address specific limitations that arise from relying on fixed effects alone: the possibility of omitted time-varying confounders, the possibility that trending series produce a spuriously significant mediation result, and the possibility that the DFI–GDP relationship itself is reverse-causal.

Omitted time-varying controls. Equation (2) is re-estimated with three additional province–year covariates added directly: $\ln(\text{FDI})_{it}$ (log foreign direct investment actually committed to province i in year t), $\ln(\text{Education})_{it}$ (log higher-education students per 100,000 population), and Indstr_{it} (the secondary-industry share of provincial GDP, in percent). These were chosen because they are standard covariates in the provincial growth literature and are each plausibly correlated with both digital-finance adoption and GDP, making them candidate confounders if omitted.

Panel unit-root test. Because both $\ln(\text{GDP})$ and $\ln(\text{DFI})$ rise almost monotonically over the sample period, Section 5.4 reports a Fisher-type (Maddala–Wu) panel unit-root test, which combines province-by-province augmented Dickey–Fuller test p -values into a single chi-squared statistic, applied

separately to $\ln(\text{GDP})$, $\ln(\text{DFI})$, and $\ln(\text{Patents})$.

Exploratory instrumental-variables estimate. Section 5.5 re-estimates the DFI–GDP relationship using each province's great-circle distance from its capital city to Hangzhou, $\ln(\text{Distance-to-Hangzhou})_i$, as an instrument for $\ln(\text{DFI})_i$, following the logic in Section 2.1.5. Because the instrument is time-invariant, this specification includes year fixed effects but not province fixed effects, and so identifies the DFI–GDP relationship from between-province rather than within-province variation — a different, and only partly comparable, source of identifying variation from the rest of the paper. As Section 5.5 explains, the exclusion restriction for this instrument cannot be verified, so it is used here as a supplementary, exploratory check rather than a definitive causal estimate.

4. Data

4.1. Data Sources and Variable Definitions

This study draws on seven publicly available, official data sources covering all 31 provincial-level regions of mainland China from 2011 to 2023:

DFI Index — Peking University Institute of Digital Finance and Ant Group [1]: a composite index of coverage, usage depth, and digitization, updated through 2023 in its most recent (fourth) release, which the index's own developers report is comparable both across provinces and over time within this update.

Provincial GDP — NBS National Data, Gross Regional Product series, in 100 million yuan [12].

Urbanization rate — urban resident population ÷ total resident population, from NBS provincial population series [9].

Patent applications — number of domestic patent applications accepted per province, NBS provincial series,

consistent with WIPO's indicator definition [13].

Foreign direct investment — total investment of foreign-funded enterprises per province, NBS provincial series, in 10,000 US dollars [16].

Higher-education enrollment — number of students enrolled in regular institutions of higher education per 100,000 population, NBS provincial series [17].

Industrial structure — secondary-industry (mining, manufacturing, construction, and utilities) share of provincial GDP, computed from NBS's Gross Regional Product by industry series [18].

All seven series were retrieved directly from NBS's National Data platform and the Peking University Digital Finance Institute's published reports, at the same provincial level, so no cross-level interpolation was needed. The 2011–2023 window is the full period covered by the most recent update of the Peking University–Ant Group DFI Index [1], which its developers report as methodologically consistent across the whole period, so no mixing of index vintages is involved.

Three isolated, implausible values in the raw FDI series were corrected before analysis: Tibet's 2023 figure, Tianjin's 2021 figure, and Henan's 2023 figure each departed from their neighboring years by a factor of roughly 10, consistent with a data-entry or scraping artifact in the source export rather than a genuine one-year shock, and were replaced with a linear interpolation from the adjacent years. A separate, much larger rise in Hainan's FDI figures after 2020 was retained unadjusted: it coincides with, and is consistent with, Hainan's designation as a Free Trade Port in 2020, which is documented to have drawn a wave of foreign-funded holding-company registrations to the province; a robustness check excluding Hainan is available on request.

4.2. Descriptive Statistics

Table 1. Descriptive statistics, province–year panel, 2011–2023 ($n = 403$)

Variable	Mean	Std. Dev.	Min	Max	N
GDP (100M yuan)	27,962.1	24,692.6	620.8	137,905.4	403
DFI Index	254.48	111.33	16.22	473.83	403
Urbanization (%)	59.69	12.92	22.65	89.58	403
Patent applications	114,229	164,095	170	993,480	403
FDI (10,000 USD)	303,686	726,237	726	9,748,500	403
Higher-ed. students (per 100,000 pop.)	2,808.7	907.2	1,082	5,839	403
Secondary industry (% of GDP)	39.15	7.97	14.56	61.34	403

Table 1 shows a wide spread across provinces: GDP ranges from Tibet's 620.8 (100 million yuan, 2011) to Guangdong's 137,905.4 (2023), and the DFI Index ranges from 16.22 (Tibet, 2011) to 473.83 (Shanghai, 2023). FDI is heavily right-skewed, largely reflecting Hainan's post-2020 Free Trade Port

surge discussed above; results involving FDI should be read with that in mind. This spread, combined with thirteen years of within-province change, is what identifies the fixed-effects models in Section 5.

4.3. Trend Analysis

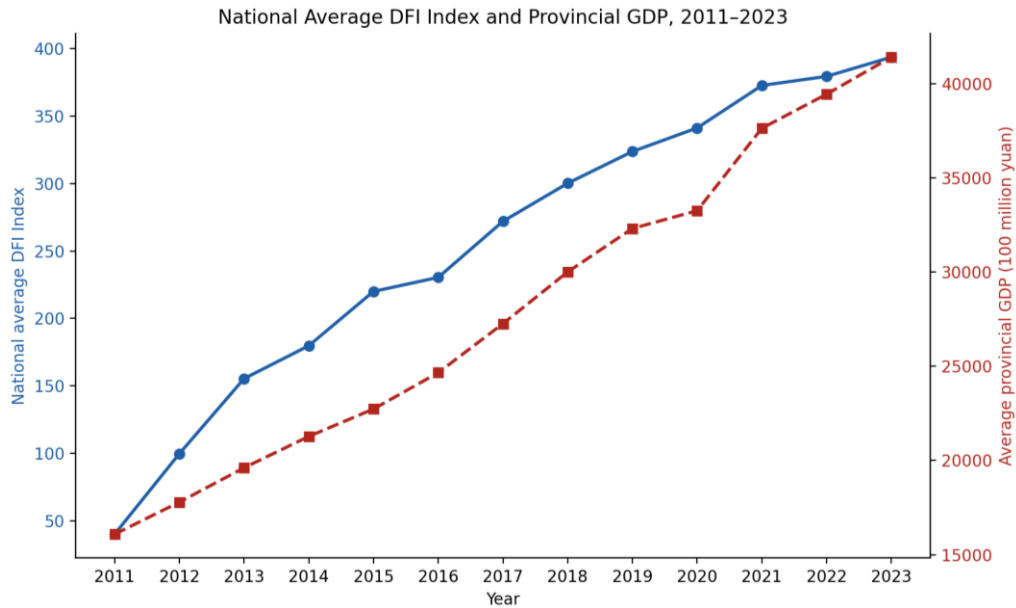


Figure 2. National average DFI Index and provincial GDP, 2011–2023

Figure 2 shows the national average DFI Index and average provincial GDP both rising steadily over the sample period, with the DFI Index's growth visibly slowing in the most recent years while GDP growth remains comparatively steady. This shared upward trend for most of the period is exactly why a fixed-effects approach is needed: a plain correlation between two rising series can overstate the strength of any real relationship [5, 6].

5. Results

5.1. Baseline and Fixed-Effects Regression Results

Table 2. Direct effect of $\ln(\text{DFI})$ on $\ln(\text{GDP})$: pooled vs. fixed-effects models

Model	β ($\ln \text{DFI}$)	Std. Error	p-value	N
Pooled OLS	0.573	0.057	< 0.001	403
Province fixed effects	0.410	0.017	< 0.001	403
Province + year fixed effects	0.142	0.090	0.117	403

Table 2 compares three specifications of the direct DFI–GDP relationship, moving from a pooled regression to a fully controlled panel model.

The pattern is clear and consistent: the estimated elasticity falls from 0.573 (pooled) to 0.410 once province fixed effects are added, and to 0.142 — no longer statistically distinguishable from zero — once year fixed effects are added as well. Since year fixed effects absorb anything common to all provinces in a given year, including the shared upward trend in Figure 2, this points to the pooled result being driven largely by that shared trend rather than a real province-specific relationship. A wild cluster bootstrap on the two-way fixed-effects estimate, applying Rademacher weights under the null across 1,999 replications to address the small number of provincial clusters, gives $p = 0.187$ — if anything less significant than the $p = 0.117$ obtained from standard clustered standard errors, reinforcing rather than overturning this conclusion. Figure 3 shows the pattern directly, comparing the raw scatter of $\ln(\text{GDP})$ against $\ln(\text{DFI})$ with the same data after removing province and year means.

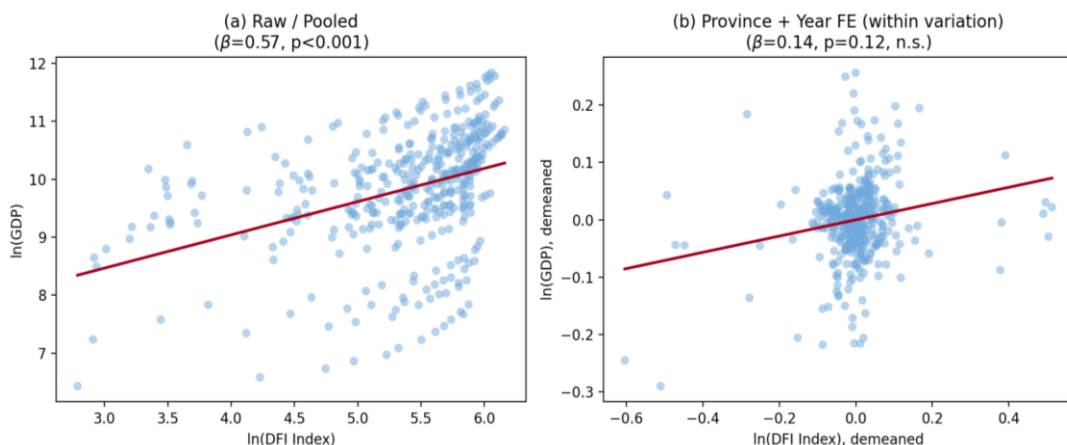


Figure 3. Raw correlation vs. within-panel variation, DFI–GDP

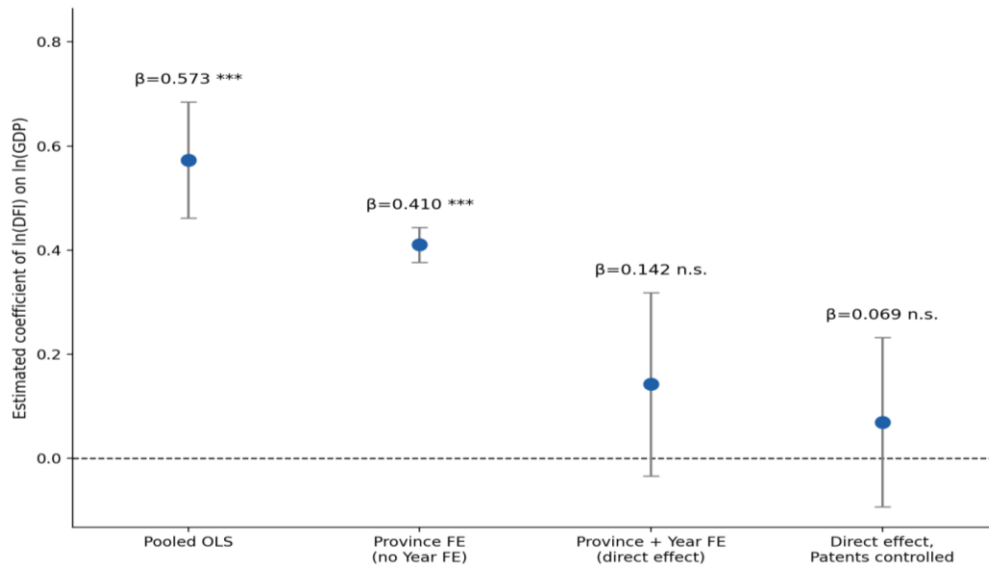


Figure 4. Estimated DFI coefficient across model specifications (* $p < .05$, ** $p < .01$, *** $p < .001$; n.s. = not significant)

Figure 4 plots the coefficient and its 95% confidence interval across all four specifications used in this paper, including the mediation-adjusted estimate from Section 5.2. The confidence interval for the two-way fixed-effects estimate crosses zero, which is why this paper does not treat the direct effect alone as reliable evidence — and why the mediation analysis below matters.

5.2. Mediation Analysis: Technological Innovation

Table 3 reports the three-step mediation test from Section 3.3.

Table 3. Mediation analysis: DFI $\rightarrow$ innovation $\rightarrow$ GDP (province + year fixed effects)

Path	Coefficient	Std. Error	p-value
a-path: DFI $\rightarrow$ Patents	0.775	0.173	< 0.001
b-path: Patents $\rightarrow$ GDP (DFI controlled)	0.093	0.041	0.023
c': DFI $\rightarrow$ GDP (Patents controlled)	0.069	0.083	0.404
c: total effect (Table 2, two-way FE)	0.142	0.090	0.117
Indirect effect ($a \times b$, Sobel test)	0.072	0.036	0.042

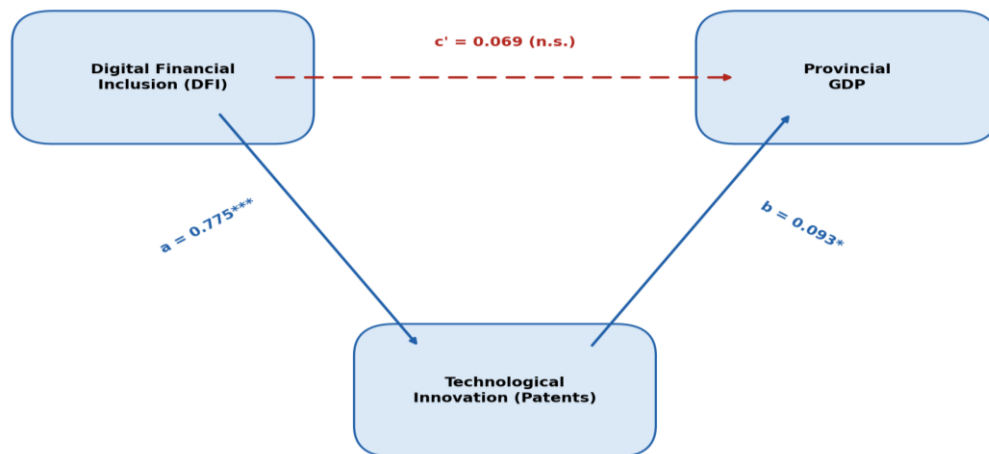


Figure 5. Mediation of the DFI–GDP relationship through technological innovation (* $p < .05$, ** $p < .01$, *** $p < .001$; n.s. = not significant)

These results support H2. DFI strongly predicts patent applications ($a = 0.775$, $p < 0.001$): a one-percent rise in the DFI Index goes with roughly a 0.78 percent rise in patent applications, after province and year fixed effects. Patents in turn significantly predict GDP even with DFI held constant ($b = 0.093$, $p = 0.023$). Once patents are in the model, the direct effect of DFI drops further, from an already-insignificant 0.142 to 0.069, and stays insignificant ($p = 0.404$). A significant a-path, a significant b-path, and a direct effect that disappears follow the textbook signature of full mediation, but as Section 3.3 notes, this stepwise pattern is not treated as the primary evidence here; the indirect effect itself is tested

directly below.

To test the indirect effect directly rather than inferring it from the pattern of a , b , and c' , the product $a \cdot b$ is evaluated using the Sobel (1982) standard error, $SE(ab) = \sqrt{(b^2 \cdot SE_a^2 + a^2 \cdot SE_b^2)}$. This gives an indirect effect of 0.072 ($SE = 0.036$, $z = 2.04$, $p = 0.042$, 95% CI [0.003, 0.142]), which is statistically distinguishable from zero, though only narrowly. This is an analytic (delta-method) approximation, so it was cross-checked with a percentile bootstrap: provinces were resampled with replacement (2,000 replications), the a - and b -paths were re-estimated within the two-way fixed-effects specification in each replicate, and the resulting indirect

effects ($a \times b$) were used to form a percentile confidence interval. This gives a bootstrap mean indirect effect of 0.071 and a 95% CI of [0.007, 0.150], which excludes zero and is consistent with the Sobel-based estimate above; only 1.25% of the 2,000 replicates fell at or below zero. The percentile bootstrap therefore corroborates the delta-method result. Section 5.3, however, shows that this evidence weakens once additional time-varying controls are added, and Section 5.4 flags a stationarity concern specific to the patent series; both qualify how much weight the mediation finding can carry.

Table 4. Robustness: adding FDI, higher-education enrollment, and industrial structure as controls (province + year fixed effects; dependent variable $\ln(\text{GDP})$ unless noted)

Variable	Direct-effect model β (p)	Mediation b/c' model β (p)	a-path model β (p)
$\ln(\text{DFI})$	0.077 (0.203)	0.056 (0.360)	0.620 (< 0.001)
$\ln(\text{Patents})$	—	0.034 (0.259)	—
$\ln(\text{FDI})$	0.013 (0.276)	0.010 (0.435)	0.096 (0.316)
$\ln(\text{Education})$	0.048 (0.701)	0.052 (0.659)	-0.130 (0.740)
Industrial structure (%)	0.015 (0.001)	0.013 (0.001)	0.049 (< 0.001)

Two things stand out. First, the direct DFI–GDP coefficient remains statistically insignificant with controls added ($\beta = 0.077$, $p = 0.203$), consistent with Table 2 — the null direct-effect finding (H1 not supported) is not an artifact of omitted variables. Second, and less reassuringly for H2, the b-path (Patents $\rightarrow$ GDP) weakens from $p = 0.023$ without controls to $p = 0.259$ with them, while industrial structure itself is a strong, highly significant predictor of GDP in every specification ($p \leq 0.001$ throughout).

The most plausible reading of this pattern is that industrial structure and patents are correlated with each other, not just with GDP: provinces with a larger secondary-industry base — more manufacturing, construction, and utilities activity — both file more patents and produce more GDP, for reasons that may have comparatively little to do with digital finance. Once industrial structure is in the model, it absorbs part of what the simpler mediation model in Table 3 had attributed to patents.

5.3. Robustness: Omitted Time-Varying Controls

The results in Sections 5.1 and 5.2 control only for province and year fixed effects. Table 4 re-estimates the direct-effect and mediation models with $\ln(\text{FDI})$, $\ln(\text{Education})$, and industrial structure (%) added directly as controls, per Section 3.5.

This does not mean the innovation channel is spurious: the a-path (DFI $\rightarrow$ Patents) remains strong and highly significant with controls added ($\beta = 0.620$, $p < 0.001$), so DFI's link to innovation itself is robust. It is specifically the second half of the channel — innovation translating into measured GDP — that is sensitive to this addition. The mediation evidence in Section 5.2 should therefore be read as suggestive of a real but only partially isolated channel, not as a fully confounder-robust result.

5.4. Robustness: Panel Unit-Root Test

Because $\ln(\text{GDP})$ and $\ln(\text{DFI})$ both trend upward over almost the entire sample period, Table 5 reports a Fisher-type (Maddala–Wu) panel unit-root test, which combines 31 province-level augmented Dickey–Fuller test p-values (with a linear trend and one lag) into a single chi-squared statistic under the null that every province's series contains a unit root.

Table 5. Fisher-type (Maddala–Wu) panel unit-root test, 2011–2023

Series	Fisher statistic	df	Combined p-value	Interpretation
$\ln(\text{GDP})$	81.92	62	0.046	Reject unit root (marginal)
$\ln(\text{DFI})$	152.91	62	< 0.001	Reject unit root
$\ln(\text{Patents})$	40.29	62	0.985	Fail to reject unit root

With only thirteen years per province, this test has limited power, and its results should be read with that caveat. Even so, the combined test rejects a unit root for $\ln(\text{GDP})$ ($p = 0.046$, though only marginally) and rejects it strongly for $\ln(\text{DFI})$ ($p < 0.001$), consistent with both series being trend-stationary rather than purely stochastic trends over this window. The patent series is different: the test fails to reject a unit root in $\ln(\text{Patents})$ ($p = 0.985$), meaning the data do not rule out patents following a stochastic, non-stationary trend. This is a specific caveat for the mediation results in Section 5.2, since a non-stationary mediator raises some risk that the a-path and b-path coefficients partly reflect a spurious trend relationship rather than a stable structural one, even after year fixed effects. It does not overturn the mediation finding — year fixed effects already remove any trend common to all provinces — but it is a reason, alongside Section 5.3, to treat the mediation

evidence as suggestive rather than definitive.

5.5. Exploratory Check: Instrumental-Variables Estimate Using Distance to Hangzhou

Section 2.1.5 described the use of distance to Hangzhou as an instrument for digital financial inclusion in the literature. This section applies that instrument at the provincial level as an exploratory check on reverse causality, not as a definitive causal estimate: each province's capital-city distance to Hangzhou (great-circle distance, log-transformed) instruments for $\ln(\text{DFI})$ in a two-stage least-squares model with year fixed effects (province fixed effects cannot be included alongside a time-invariant instrument).

Table 6. Instrumental-variables (2SLS) estimate using distance to Hangzhou, with year fixed effects; N = 403; first-stage F-statistic ≈ 10.8

Stage / Model	Coefficient	Std. Error	p-value
First stage: $\ln(\text{Distance-to-HZ}) \rightarrow \ln(\text{DFI})$	-0.056	0.017	0.001
Second stage (2SLS): $\ln(\text{DFI}) \rightarrow \ln(\text{GDP})$	5.682	1.186	< 0.001
For comparison, pooled OLS with year FE only (no instrument)	3.001	0.663	< 0.001

The first stage confirms the expected pattern: provinces farther from Hangzhou have systematically lower DFI ($\beta = -0.056$, $p = 0.001$), though the first-stage F-statistic of approximately 10.8 is only borderline above the conventional rule-of-thumb threshold of 10 for a strong instrument, so this result should be treated cautiously — with a province-level panel of 31 units, there is far less statistical power than in the city-level analyses in the literature that originally validated this instrument. The second-stage estimate is large, positive, and highly significant ($\beta = 5.682$, $p < 0.001$), substantially larger than the corresponding OLS coefficient using the same cross-sectional variation ($\beta = 3.001$). Two things temper how this result should be read. First, because the instrument is time-invariant, this specification identifies the DFI–GDP relationship from between-province geographic variation, not the within-province variation used in Table 2's two-way fixed-effects model, so the two estimates are not directly comparable — they answer related but distinct questions. Second, the exclusion restriction cannot be guaranteed: distance to Hangzhou is highly correlated with general coastal proximity, which plausibly affects GDP through trade access, infrastructure, and historical development patterns quite apart from digital finance. This check is therefore reported as supportive of a positive geography-driven association between digital finance and growth, not as a resolution of the reverse-causality concern raised in Section 7.3.

5.6. Exploratory Moderation Analysis: Urbanization (Not Confirmatory)

An exploratory test of urbanization as a moderator was attempted using the specification in Section 3.4. The estimated model is statistically unstable — its within-panel R^2 is negative (-0.62) — which indicates unreliable, poorly identified estimates rather than a dependable result. Because of this instability, the full coefficient table is not reported in the main text; it is provided in Appendix B for transparency, and H3 is accordingly treated as untested rather than supported or refuted in this dataset. This result is not part of the paper's main contribution and should not be read as evidence either way.

5.7. Summary of Findings

H1 (direct effect): Not supported once province and year fixed effects are added — the pooled correlation mostly reflects a shared national trend, confirmed by a wild cluster bootstrap and unaffected by adding FDI, education, and industrial-structure controls.

H2 (mediation via innovation): Partially supported — DFI predicts patents robustly, and the indirect effect is significant by both the Sobel test and a percentile bootstrap, but the patents-to-GDP link weakens once industrial structure is controlled for, and the patent series does not clearly reject non-stationarity, so this evidence is best read as suggestive rather than fully confounder-robust.

H3 (moderation via urbanization): Untested — the model shows clear signs of instability (negative within-panel R^2), so the nominally significant interaction cannot be treated as

evidence either way.

Reverse causality: An instrumental-variables check using distance to Hangzhou finds a large, positive, significant association between instrumented DFI and GDP, but with a borderline-strong first stage and an exclusion restriction that cannot be guaranteed, so this supports rather than settles the case that the relationship is not purely reverse-causal.

6. Discussion

This paper's specifications identify the DFI–GDP relationship from different sources of variation, which is why they give different answers. Pooled OLS uses all variation and finds a strong association ($\beta = 0.573$). The two-way fixed-effects model isolates within-province, year-on-year variation and finds no significant effect ($\beta = 0.142$, $p = 0.117$; confirmed by a wild cluster bootstrap, $p = 0.187$). The instrumental-variables model uses only between-province geographic variation and finds a large, significant association ($\beta = 5.682$). Together, these results suggest that most of the raw DFI–GDP correlation reflects a shared national trend or stable cross-province geographic differences, not a within-province effect of rising DFI on rising GDP. This paper does not find a direct, within-province effect that survives its strictest test.

The mediation results point to a more specific, only partially confirmed channel. DFI robustly predicts patent applications, with or without controls ($a = 0.775$ without controls, 0.620 with them; both $p < 0.001$). Patents predict GDP after controlling for DFI in the baseline model ($b = 0.093$, $p = 0.023$), but this weakens sharply once industrial structure is added ($p = 0.259$): provinces with a larger secondary-industry base both patent more and produce more GDP, so part of the apparent innovation channel may reflect industrial composition instead. A further caveat: the patent series does not clearly reject a unit root, unlike GDP and DFI, adding some risk that the a - and b -paths partly reflect a trend rather than a stable relationship. This is consistent with prior evidence that digital finance eases financing constraints for smaller firms [2] and with cross-country findings that digital-inclusion growth effects are typically modest [7], but the innovation channel here is suggestive, not confounder-robust.

The moderation result is a caution, not a finding: its negative within-panel R^2 signals an unstable model, since urbanization barely varies within a province once fixed effects are included. It is reported in Appendix B for transparency and carries no weight in this paper's conclusions.

7. Conclusion, Policy Implications, and Future Outlook

7.1. Conclusion

This paper finds no statistically robust direct effect of digital financial inclusion on provincial GDP in China once province and year fixed effects, additional time-varying controls, and a small-cluster-robust bootstrap are all applied: the pooled association is real, but it does not survive these stricter tests. What the data support instead is a more specific

and only partially confirmed story: DFI robustly predicts patent applications, and the resulting indirect effect on GDP is statistically significant by two different tests, but the second half of that channel — patents translating into GDP — weakens once industrial structure is controlled for, and the patent series does not clearly reject non-stationarity. An exploratory instrumental-variables estimate using distance to Hangzhou is consistent with a genuine, not purely reverse-causal, relationship, but its evidentiary weight is limited by a borderline first stage and an unverifiable exclusion restriction. Urbanization's moderating role remains untested, not because the interaction coefficient is small, but because the model estimating it is unstable.

7.2. Policy Implications

Do not rely on a raw, national or pooled correlation between digital finance and growth to justify policy — in this data, that correlation does not survive controlling for province and year effects.

Where a growth effect exists, it is more likely to run through innovation than through a direct channel, so pairing digital-finance expansion with innovation and R&D support is a more defensible policy pairing than promoting digital finance on its own — though this channel is not yet fully robust to confounders.

Provinces where DFI is rising but patenting is not may be worth examining directly, as a possible sign of remaining financing frictions.

7.3. Limitations and Future Outlook

Four limitations remain despite the robustness checks in Section 5. First, while FDI, higher-education enrollment, and industrial structure are controlled for directly, other time-varying province-level covariates — for example, local government spending composition or credit-market conditions — remain omitted; industrial structure in particular absorbs some of what looks like an innovation-mediated effect in the simpler model, so the mediation result should be read as suggestive of a real but not fully isolated channel. Second, standard errors are clustered by province, but with only 31 clusters this is close to the lower bound at which cluster-robust inference is considered dependable; the wild cluster bootstrap applied throughout this paper consistently confirms rather than overturns its null and significant findings, which is reassuring but does not eliminate the underlying small-cluster concern. Third, reverse causality is addressed only partially: the distance-to-Hangzhou instrument in Section 5.5 is consistent with a genuine relationship, but its first-stage strength is borderline at the provincial level and its exclusion restriction cannot be verified. Fourth, patent counts are an imperfect proxy for innovation and say nothing about patent quality; province-level patent-quality or citation data are not available through NBS's public data platform and would require a proprietary database.

Two further data limitations are worth flagging explicitly. Provincial GDP may still carry some of the double-counting bias documented before China's 2019–2020 statistical revision [6], which this paper does not adjust for and for which no publicly available corrected historical series exists. The urbanization moderation result (Section 5.6, Appendix B) is explicitly unreliable and should not be read as evidence either way.

Future work could use patent quality measures instead of

raw counts if a suitable licensed data source becomes available, apply a within-province instrumental-variable design (rather than the purely cross-sectional one used here) to better address reverse causality, and re-test urbanization using city-level data, which would offer more within-unit variation than the provincial measure used here.

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Appendix

Table A1 reports the DFI Index and GDP (100 million yuan) for all 31 provinces in the first and last sample years. Full year-by-year data for 2011–2023, including urbanization, patents, FDI, education, and industrial structure, is available from the author on request.

Table A1. DFI Index and GDP by province, 2011 and 2023

Province	DFI 2011	DFI 2023	GDP 2011	GDP 2023
Beijing	79.41	463.63	17653.3	47353.7
Tianjin	60.58	422.89	8240.8	17211.8
Hebei	32.42	376.11	21711.7	45660.0
Shanxi	33.41	384.24	11005.1	26050.8
Inner Mongolia	28.89	374.63	9610.8	25020.5
Liaoning	43.29	379.66	16585.3	31389.8
Jilin	24.51	359.80	7857.7	13942.7
Heilongjiang	33.58	362.37	10074.0	16470.7
Shanghai	80.19	473.83	20406.1	51404.5
Jiangsu	62.08	438.61	49689.5	130924.3
Zhejiang	77.39	453.78	32353.1	85619.6
Anhui	33.07	405.80	16532.7	48227.5
Fujian	61.76	430.45	18195.2	54801.7
Jiangxi	29.74	389.50	11717.4	32677.1
Shandong	38.55	404.79	39622.0	94206.4
Henan	28.40	396.82	26212.2	60627.7
Hubei	39.82	411.33	20228.3	56794.3
Hunan	32.68	381.26	19176.4	50667.5
Guangdong	69.48	426.48	54009.6	137905.4
Guangxi	33.89	369.94	10472.7	27501.7
Hainan	45.56	390.82	2525.1	7590.2
Chongqing	41.89	395.00	10314.1	30614.3
Sichuan	40.16	384.11	21313.1	61353.4
Guizhou	18.47	359.52	5690.5	21513.7
Yunnan	24.91	366.84	9666.2	30595.8
Tibet	16.22	356.30	620.8	2532.9
Shaanxi	40.96	398.06	12320.1	33976.5
Gansu	18.84	362.62	4880.7	12345.7
Qinghai	18.33	351.08	1388.3	3849.2
Ningxia	31.31	373.64	1959.3	5368.8
Xinjiang	20.34	360.81	6607.6	19603.3

Appendix B: Exploratory Moderation Analysis (Full Results)

Table B1 reports the moderation test from Section 3.4, re-estimated on the full 2011–2023 panel. As explained below the table, this specification is statistically unstable (negative within-panel R^2) and the coefficients should not be read as evidence of a real moderation effect; the table is included for transparency only.

Table B1. Moderation analysis: $DFI \times \text{urbanization} \rightarrow \text{GDP}$ (province + year fixed effects, 2011–2023; within-panel $R^2 = -0.62$)

Term	Coefficient	Std. Error	p-value
DFI	0.039	0.064	0.548
Urbanization (%)	0.027	0.010	0.005
$DFI \times \text{Urbanization}$	−0.0041	0.0017	0.020

This test should be read as exploratory rather than as a confirmatory test of H3, for the following reason: once province fixed effects are in the model, urbanization — which barely moves within a province from year to year — has very little independent variation left to explain, since the fixed effects already absorb most of it. Consistent with this, the within-panel R^2 for this specification is negative, which is a clear warning sign of an unstable, poorly identified model rather than a dependable estimate. The interaction term's nominal significance ($p = 0.020$) should not be read as evidence for or against H3; given the model instability, this test does not provide a reliable basis for accepting or rejecting moderation, and H3 is accordingly treated as untested rather than supported or refuted in this dataset.