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Structural Shift in the Regional Disparity of Digital Financial Inclusion in China and the Provincial Border Effect

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Abstract: Drawing on a balanced panel of 329 prefecture-level cities from the Peking University Digital Financial Inclusion Index over 2011–2023, this study applies the adjacent-city-pair border effect test, which, so far as our search extends, has not previously been used in research on digital financial inclusion. Aggregate disparity has narrowed markedly, with the Gini coefficient of the aggregate index falling from 0.175 to 0.046. The spatial structure, however, reversed over the same period: for usage depth, the between-province contribution rose from 56.2% to 75.9%, the Dagum transvariation component fell from 18.8% to 10.6%, and Moran's I under same-province weights rose from 0.438 to 0.700; a randomization test shows the between-group share under the true provincial grouping is 8.3 times that under random groupings. The Phillips-Sul log t test rejects overall convergence for the aggregate index ($t = -5.020$) but not for the three sub-dimensions, while 24 of 26 testable provinces fail to reject within-province convergence in usage depth. Using 867 geographically adjacent city pairs and controlling for centroid distance, the within-pair baseline difference and six city-level variables covering output per capita, population, industrial structure and financial depth, cross-province pairs display a usage-depth gap 0.0091 larger than within-province pairs ($P = 0.035$; randomization-inference empirical $P = 0.013$), robust across distance bandwidths, caliper-matched samples and alternative border definitions, with an insignificant pseudo-border placebo. For coverage breadth, the sign reverses across specifications, so no robust effect is found. Access to digital finance has become broadly equalized across provinces, whereas usage intensity remains organized along provincial administrative boundaries.

Keywords: Digital financial inclusion; Border effect; Regional disparity; Dagum Gini decomposition; Convergence club

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1. Introduction

The combination of digital technology and inclusive finance has been expected to overcome the geographical constraints of traditional financial services. Since 2011, the coverage of digital payment, digital credit, and

digital insurance in China has expanded rapidly, and regional gaps in financial services have narrowed accordingly. Most existing studies follow this line of reasoning and focus on whether aggregate disparity has declined and how quickly [1,2]. The Institute of Digital Finance at Peking University [3] notes in its latest index report that the pace of regional convergence has slowed appreciably in recent years and that whether disparity narrows further depends chiefly on regional differences in usage depth.

A decline in aggregate disparity, however, is not equivalent to a more balanced spatial pattern. Disparity may narrow evenly across cities, or it may be reorganized along some boundary even as it narrows. If the latter holds, then even when aggregate disparity is already small, policy should no longer target the absolute level of lagging cities but rather the boundary along which disparity is sustained. This distinction matters especially in digital finance: the technology is not constrained by geography, so if its spatial pattern still follows administrative boundaries, the binding constraint is institutional rather than technological.

Four findings emerge. Aggregate disparity narrowed substantially, largely before 2016. The structure of disparity reversed in an unambiguous direction, and a randomization test rules out a statistical artifact. The full sample rejects overall convergence for the aggregate index but not for the sub-dimensions, whereas most provinces do not reject within-province convergence. Finally, among geographically adjacent city pairs, cross-province pairs display significantly larger usage-depth gaps than within-province pairs, with no such pattern for coverage breadth. The contributions are twofold: the study moves regional disparity research from the "level" of disparity to its "structure"; and it applies the adjacent-city-pair border effect test to digital financial inclusion, a design that, so far as our search extends, has not previously been used in this field and that compares city pairs of comparable distance but different administrative affiliation, thereby distinguishing administrative affiliation from broad geographical gradients more sharply than decomposition methods allow. Six layers of evidence support the core result: a within-pair baseline-difference control, baseline-caliper matching, distance-bandwidth sensitivity, alternative border definitions, a pseudo-border placebo, and a randomization test.

2. Literature review and positioning

Three strands of literature bear on this study. The first measures regional disparity and the spatial pattern of digital financial inclusion (DFI). Lü and He [1] characterized regional differences and convergence in China's fintech using the Dagum Gini coefficient, kernel density estimation, Markov chains and σ- and β-convergence tests; Zhang *et al.* [2] applied a Dagum decomposition and spatial autocorrelation analysis to 20 urban agglomerations; Ding and Tian [4] measured the dynamic coordinated development of the DFI system and its spatio-temporal disparity from a geographical spillover perspective; Chai *et al.* [5] examined the spatial convergence of DFI effects on the urban–rural income gap with prefecture-level data; and Zeng *et al.* [6] traced the evolution of development modes from both sides of the market and document persistent imbalance. Such studies generally group by the four major regions or urban agglomerations and converge on the conclusion that aggregate disparity is falling while between-region differences remain dominant. The second strand concerns border effects. McCallum [7] estimated the segmentation created by the Canada–U.S. border with a gravity equation; Wang *et al.* [8] reviewed the theory and measurement of border effects; Meng *et al.* [9] stressed that the economic effects of administrative boundaries in China remain under-explored. On Chinese evidence, Guo and Minier [10] identified a discontinuity in economic activity at provincial borders using county data on either side; Yang *et al.* [11] examined how border effects alter regional development in a quasi-natural experiment; Li *et al.*

[12] applied a refined border approach to policy differences across county boundaries; and Cai *et al.* [13] estimated the provincial border effect on trade flows using prefecture-level VAT invoice data. The third strand lies at the intersection of the digital economy and administrative boundaries: Guo *et al.* [14] use satellite night-light data to examine how the digital economy affects border areas, treating the easing of administrative fragmentation as a key mechanism; Zhang and Sun [15] studied digital infrastructure, border effects, and regional development gaps; and Yang and Xia [16] found that although the digital economy raises both intra- and inter-provincial trade, it does not significantly weaken the border effect—implying that digitalization need not dissolve administrative boundaries, which is precisely the proposition tested here. Recent micro evidence in leading finance journals supports this framing. Hau *et al.* [17] showed that Ant Group's credit supply is tailored to borrower characteristics rather than local credit market conditions, so two vendors with identical characteristics receive the same terms wherever they are located, whereas traditional banks' lending allocation is subject to local government influence; Higgins [18] showed that the diffusion of digital payments depends heavily on complementary adoption by local merchants and exhibits pronounced network externalities. The two map onto the "access" and "usage" stages characterized here.

The positioning of this study follows. The first strand measures the grouping sources of disparity, but such a decomposition can only establish whether disparity lies between or within groups; it cannot distinguish whether between-group differences arise from broad geographical and economic gradients or from an institutional discontinuity at the boundary, and the two carry sharply different policy implications. The second and third strands supply the tools for identifying the role of boundaries, but their objects of study are trade flows, aggregate output or development gaps, and so far as our search extends, they have not been applied to digital financial inclusion itself.

3. Research design

3.1. Data and sample

The core data are the prefecture-level city series of the Peking University Digital Financial Inclusion Index, whose compilation method is described in Guo *et al.* [19]. The main sample covers 2011–2023, matching the years for which the index is officially released. Cities with no missing values on the aggregate index, coverage breadth, usage depth, and digitization level are retained, yielding a balanced panel of 329 cities, 13 years, and 4,277 observations, assigned to 31 provincial-level administrative units by administrative division code.

The raw data contain an additional column for 2024. On inspection, the ratio of these values to their 2023 counterparts is exactly 1.08 for all 329 cities and all four dimensions, with a standard deviation of zero; the column is therefore a uniform proportional extension and carries no independent cross-sectional information. It should be emphasized that every measure used in this study—the Gini coefficient, the coefficient of variation, Theil shares, Dagum component shares, Moran's I of the log index, the provincial capital premium, and the border effect, whose dependent variable is a log difference—is invariant to a within-year proportional rescaling, so including this column cannot alter any result. The column is accordingly reported as an extended sample in the robustness analysis (last row of **Table 5**), where the estimate coincides with the main sample and thereby verifies this invariance directly.

Geographical adjacency is constructed from vector boundary data for China's prefecture-level administrative divisions: after merging the districts of the four municipalities directly under the central government into single units, two prefecture-level units are treated as adjacent if their boundaries share a vertex

or if their minimum distance is below 0.02 degrees (about 2 kilometers). This yields 971 adjacent pairs, with an average of 5.29 neighbors per unit; matching to the index panel leaves 867 usable city pairs, of which 583 are within-province, and 284 are cross-province.

To address the concern that the border effect may originate in other systematic differences between cross-province pairs, city-level controls are obtained from the CSMAR China Regional Economy Database, all at the whole-prefecture level and matched to the index panel by administrative division code. Six variables are used: gross regional product per capita, year-end registered population, the shares of secondary and tertiary industry in gross regional product, and year-end outstanding deposits and loans of financial institutions, corresponding to economic development, city size, industrial structure, and financial depth. Because a pair-level regression requires both cities to be non-missing in the same year, the intersection leaves 5,985 pair-year observations covering 661 city pairs, 53% of the full sample.

3.2. Methods

Four sets of methods are employed. The first is disparity decomposition. Following Dagum [20], the overall Gini coefficient is decomposed into the within-group contribution G_w, the net between-group contribution G_{nb}, and the transvariation contribution G_t, so that $G = G_w + G_{nb} + G_t$; a decline in G_t implies that boundaries between provincial distributions are becoming sharper [2]. The second is spatial association: a row-standardized weight matrix is constructed with "same province" as the adjacency criterion, and the global Moran's I of the log index is computed for each year [21,22]. The third is convergence testing, based on the log t test of Phillips and Sul [23], which estimates $\log(H_1/H_t) - 2\log(\log t) = a + b \log t + u_t$ with $r = 0.3$ and rejects convergence when the t statistic falls below -1.65 [24]. The fourth is distributional dynamics, in which Markov transition probability matrices are estimated [25].

Building on these, the core test of this study is the adjacent-city-pair border effect model:

$$|\ln X_{it} - \ln X_{jt}| = \alpha + \beta Cross_{ij} + \gamma \ln D_{ij} + \delta Base_{ij} + \lambda_t + \varepsilon_{ijt} \quad (1)$$

where i and j denote a pair of geographically adjacent cities, X is the corresponding index, *Cross* is a dummy equal to one when the two cities belong to different provinces, D is the great-circle distance between centroids, *Base* is the absolute difference in the 2011 log index between the two cities and controls for the within-pair difference in initial development, λ_t denotes year fixed effects, and standard errors are clustered at the city-pair level. β measures the additional gap of cross-province adjacent pairs relative to within-province pairs, conditional on comparable distance and comparable initial development.

4. Level evolution and structural reversal of regional disparity

4.1. Aggregate disparity has narrowed substantially and has stabilized

Between 2011 and 2023, the Gini coefficient of the aggregate index fell from 0.175 to 0.046, a decline of 74%; coverage breadth fell from 0.298 to 0.052, a decline of 82%; and usage depth fell from 0.184 to 0.057, a decline of 69%. Most of the decline occurred before 2016: by 2016 the Gini coefficient of the aggregate index had already fallen to 0.053, and thereafter it fluctuated within a narrow band of 0.046 to 0.054.

4.2. Reversal in the structure of disparity

Table 1 reports the Dagum decomposition and related structural indicators for usage depth. The net between-group contribution rose from 78.87% in 2011 to 87.51% in 2023, the within-group contribution edged down

from 2.32% to 1.89%, and the transvariation contribution fell from 18.81% to 10.61%. The pronounced decline in transvariation implies that the overlap between the distributions of cities in different provinces is shrinking and that the explanatory power of province as a grouping variable is strengthening. The Theil decomposition points in the same direction, with the between-province share rising from 56.2% to 75.9%; the same trend holds for the aggregate index and coverage breadth, whose between-group shares rose from 61.6% to 77.5% and from 59.8% to 71.8%. Two independent indicators corroborate this judgment: Moran's *I* under same-province weights rose monotonically from 0.438 to 0.700, and the premium of provincial capitals over other cities in the same province fell from 1.217 to 1.097.

Table 1. Dagum decomposition and structural indicators for usage depth

Year	Within G_w	Between G_{nb}	Transvariation G_t	Between-province Theil share	Same-province Moran's *I*	Capital premium
2011	2.32%	78.87%	18.81%	56.20%	0.438	1.217
2015	2.20%	81.88%	15.93%	70.60%	0.645	1.164
2019	2.10%	84.88%	13.02%	71.98%	0.659	1.130
2023	1.89%	87.51%	10.61%	75.92%	0.700	1.097

Note: The Dagum decomposition uses the 31 provincial-level administrative units as the grouping variable; the first three columns report the share of each component in the overall Gini coefficient. Moran's *I* uses a row-standardized weight matrix in which same-province pairs take the value one; the four municipalities have no same-province neighbors, so the standardization constant S_0 is 325.

4.3. Randomization test and comparison of grouping schemes

Because the Gini coefficient had fallen to a low level by 2023, a natural concern is whether the high between-province share merely reflects the large number of groups. Holding the size distribution of the 31 groups fixed, provincial labels are randomly permuted 2,000 times, and the between-province share is recomputed. Under random grouping, the mean share is only 0.0912, the 95th percentile is 0.1287, and the maximum is 0.1717, whereas the value of 0.7592 under the true grouping lies far outside this distribution, with an empirical *P*-value of 0.0000 and a ratio of 8.3. This ratio has risen over time, from 6.9 in 2011 to 7.6 in 2015, 7.8 in 2019, and 8.2 in 2023. Using usage depth in 2023, the between-group Theil share is 0.7592 under provincial grouping but only 0.4982 under the four-major-region grouping.

5. Convergence tests

5.1. The divide between the full sample and within provinces

Table 2 shows that conclusions differ across indices and must be stated separately. For the aggregate index, the full-sample log t statistic is −5.020, rejecting overall convergence at the 1% level; coverage breadth (t = 1.048), usage depth (t = −1.038), and digitization level (t = 3.772) do not reject overall convergence. The rejection therefore applies to the aggregate index only, not to the three sub-dimensions. The within-province tests set the threshold at no fewer than three cities, giving 26 testable provinces; 19 (73%) for the aggregate index and 24 (92%) for usage depth fail to reject within-province convergence, with median *t*-statistics of −0.72 and −0.05. The five units that cannot be tested are Beijing, Tianjin, Shanghai and Chongqing (one prefecture-level unit each) and Hainan (two), an exclusion that is structural rather than selective.

Table 2. Phillips-Sul log *t* convergence tests

Test scope	*b*	*t* statistic	Convergence hypothesis
Full sample, aggregate index	−0.340	−5.020	Rejected
Full sample, coverage breadth	0.081	1.048	Not rejected
Full sample, usage depth	−0.237	−1.038	Not rejected
Full sample, digitization level	1.169	3.772	Not rejected
Within province, aggregate index (26)	—	Median −0.72	19 not rejected (73%)
Within province, usage depth (26)	—	Median −0.05	24 not rejected (92%)

Note: $r = 0.3$, with HAC robust standard errors; convergence is rejected at the 5% level when the *t*-statistic falls below −1.65. Within-province tests are restricted to provinces with no fewer than three cities; Beijing, Tianjin, Shanghai, Chongqing and Hainan cannot be tested because they contain too few prefecture-level units.

5.2. Alternative groupings and alternative state definitions

To check whether the club pattern depends on a particular grouping, the log *t* test is repeated with the four major regions as units. For the aggregate index, the eastern ($t = -1.48$) and central ($t = -1.59$) regions do not reject convergence, while the northeastern ($t = -3.26$) and western ($t = -1.79$) regions do; for usage depth, none of the four rejects. This is consistent with the provincial grouping: evidence against overall convergence is concentrated in the aggregate index, and usage depth never rejects under any grouping.

Table 3 reports the diagonals of the Markov transition matrices under three state definitions. Under the quintile definition, the retention probabilities of the lowest and highest groups are 0.905 and 0.933 against 0.738, 0.725 and 0.838 for the intermediate groups; the quartile and tertile definitions display the same pattern of high retention at the extremes. The greater persistence at the two extremes therefore does not depend on a particular state definition. A spatial Markov chain conditioned on the provincial context shows that cities in the middle of the provincial distribution have the highest retention probabilities (diagonal means of 0.754 and 0.734), while cities at either end are lower (0.584 and 0.487).

Table 3. Diagonals of the Markov transition matrices for usage depth, 2016–2023

State definition	State 1	State 2	State 3	State 4	State 5
Tertiles	0.917	0.857	0.942	—	—
Quartiles	0.885	0.763	0.805	0.927	—
Quintiles	0.905	0.738	0.725	0.838	0.933

Note: States are defined by equal-sized groups of the relative index in each year; transition frequencies between adjacent years are row-standardized.

6. The provincial border effect

6.1. Baseline results and evolution over time

The decompositions and convergence tests above use province as the grouping variable and cannot distinguish an institutional discontinuity at the boundary from broad gradients between provinces. This section provides direct identification using 867 pairs of geographically adjacent cities. Within-province pairs have a mean centroid distance of 151.0 kilometers and cross-province pairs 191.7 kilometers, a significant difference ($P < 0.001$), so controlling for distance is essential. More importantly, cross-province pairs may also differ systematically in initial development, which motivates the additional control $Base_{ij}$.

Table 4 reports the estimates. With *Cross* alone, the log usage-depth gap of cross-province pairs exceeds that of within-province pairs by 0.0248; adding distance and year fixed effects reduces this to 0.0152; further controlling for the within-pair baseline difference gives 0.0100, still significant at the 5% level and equivalent to 11% of the within-province mean (0.0945). Coverage breadth and the aggregate index are insignificant under all three specifications. Year-by-year estimates show that the effect on usage depth has been consistently significant since 2014; because the within-province mean gap narrowed from 0.266 in 2011 to 0.050 in 2023, its relative magnitude rose from 14% to 22% in 2016 and 25% in 2023.

Table 4. Estimates of the provincial border effect and its evolution over time

Dependent variable/year	(1) *Cross* only	(2) + distance, year	(3) + baseline gap	Within-province mean	Relative magnitude
Usage depth (full period)	0.0248***	0.0152***	0.0100**	0.0945	11%
Coverage breadth (full period)	0.0003	−0.0130	0.0015	0.1478	1%
Aggregate index (full period)	0.0034	−0.0026	−0.0014	0.0937	−1%
Usage depth, 2011	—	0.0380	—	0.2658	14%
Usage depth, 2016	—	0.0103***	—	0.0471	22%
Usage depth, 2020	—	0.0121**	—	0.0686	18%
Usage depth, 2023	—	0.0124***	—	0.0501	25%

Note: The dependent variable is the absolute difference in log index values within an adjacent pair; the full-period sample comprises 867 pairs over 13 years. Year-by-year estimates control for centroid distance only, because the within-pair baseline gap is collinear with the dependent variable in 2011. Standard errors are clustered at the city-pair level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

6.2. Robustness checks

Table 5 reports six sets of checks. First, distance bandwidths: restricting centroid distances to below 300, 250, 200, 150 and 120 kilometers yields estimates between 0.0093 and 0.0111, all significant, so the result is not a residual reflection of the greater distance of cross-province pairs. Second, baseline-caliper matching: with calipers of 0.20, 0.15, and 0.10, the strictest leaves cross-province and within-province pairs with baseline gaps of 0.046 and 0.047, essentially balanced, and the border effect is 0.0101, significant at the 10% level. Third, alternative border definitions: the 705 pairs involving neither a provincial capital nor a municipality give 0.0133 (1% level), whereas merely excluding pairs involving a municipality lowers the estimate to 0.0044 and renders it insignificant. This contrast indicates heterogeneity across city types—provincial capitals, as regional financial centers, may absorb cross-province spillovers—so the border effect is primarily a phenomenon of ordinary prefecture-level cities, consistent with locally incorporated financial institutions operating mainly there, but implying that the conclusion should not be extrapolated to central cities. Fourth, endpoint-city fixed effects give 0.0152 (1% level). Fifth, a pseudo-border placebo on the 583 within-province pairs, with a fictitious border defined by the parity of city index numbers, yields 0.0027 (P = 0.487). Sixth, including the 2024 extended sample gives 0.0101, coinciding with the main-sample estimate of 0.0100.

Table 5. Robustness checks on the border effect (usage depth)

Specification	Border effect	*P* value	Pairs
Baseline (distance, year FE, baseline gap)	0.0100**	0.012	867
Distance < 300 km	0.0111***	0.007	805

Specification	Border effect	*P* value	Pairs
Distance < 250 km	0.0096**	0.021	768
Distance < 200 km	0.0093**	0.031	681
Distance < 150 km	0.0097**	0.050	510
Distance < 120 km	0.0111*	0.097	326
Baseline caliper < 0.20	0.0079*	0.069	485
Baseline caliper < 0.15	0.0093*	0.056	393
Baseline caliper < 0.10	0.0101*	0.075	301
Non-capital, non-municipality pairs only	0.0133***	0.001	705
Excluding pairs involving a municipality	0.0044	0.262	842
With endpoint-city fixed effects	0.0152***	<0.001	867
Pseudo-border placebo (within-province pairs)	0.0027	0.487	583
Including the 2024 extended sample	0.0101***	0.010	867

Note: Standard errors are clustered at the city-pair level. The baseline gap is not additionally controlled in the caliper-matched specifications.

6.3. City-level control variables

The specifications above use the within-pair baseline gap to control for differences in initial development, yet cross-province pairs may still differ systematically in contemporaneous economic size, industrial structure, and financial environment. This subsection adds six city-level controls to Equation (1) step by step, each entering as the absolute within-pair difference in logs (in levels for the industry shares). Results are reported in **Table 6**.

Column (1) is the full-sample baseline, 0.0100. Because the controls are not available everywhere, the estimation sample falls to 5,985 observations covering 661 pairs, so column (2) first re-estimates the baseline specification on that same subsample and obtains 0.0187, larger than in the full sample. The reason is that the missingness is not random: 29.3% of the pairs with controls are cross-province, against 43.7% of the pairs without, so the loss falls disproportionately on cross-province pairs. Column (2), not column (1), is therefore the appropriate benchmark for judging what the controls do. Adding controls from column (2) onward, the coefficient falls from 0.0187 to 0.0127 once output per capita and population are included, to 0.0103 once industrial structure is added, and to 0.0091 with all six controls, where it remains significant at the 5% level. The attenuation indicates that part of the border effect is indeed attributable to observable city differences, but the attenuated estimate is of the same order as the full-sample baseline of 0.0100 and remains significantly different from zero.

Three additional checks support this reading. First, randomization inference: retaining only within-province adjacent pairs and randomly designating about one third as pseudo-cross-province, the fully controlled specification is re-estimated 300 times; the null distribution has mean 0.0002, standard deviation 0.0035 and 2.5th and 97.5th percentiles of −0.0064 and 0.0073, so the empirical two-sided *P* value of 0.0091 is 0.013, corroborating the 0.035 from clustered standard errors without any parametric assumption about the error correlation structure. Second, a cross-dimension comparison: under the same specification and subsample, the border effect is 0.0034 ($P = 0.506$) for coverage breadth and −0.0002 ($P = 0.957$) for the aggregate index, neither significant; the contrast between bounded usage and borderless access therefore survives the controls, and it is itself a placebo test, since omitted regional heterogeneity would make all three dimensions significant simultaneously. Third, subsample checks. Under full controls, the 525 pairs involving neither a provincial

capital nor a municipality give 0.0153 ($P = 0.001$), excluding pairs that involve a municipality gives 0.0113 ($P = 0.018$), and pairs within 200 kilometers give 0.0106 ($P = 0.018$). It should be stated plainly that the subsamples within 150 kilometers (458 pairs) and with a baseline caliper below 0.15 (343 pairs) yield 0.0074 and 0.0065, neither significant; both contain fewer than half the pairs of the full sample and are clearly underpowered, so the main conclusion is not overturned on this basis, nor is the result concealed.

Table 6. Border effect after adding city-level controls (usage depth)

Specification	Border effect	*P* value	Observations	Pairs
(1) Full-sample baseline	0.0100**	0.012	11,266	867
(2) Baseline on the same subsample	0.0187***	0.001	5,985	661
(3) + GDP per capita, population	0.0127**	0.011	5,985	661
(4) + secondary and tertiary shares	0.0103**	0.025	5,985	661
(5) + deposits and loans (full controls)	0.0091**	0.035	5,985	661
Full controls: coverage breadth	0.0034	0.506	5,985	661
Full controls: aggregate index	−0.0002	0.957	5,985	661

Note: Each control enters as the absolute within-pair difference in logs, or in levels for the industry shares. All columns control for centroid distance, year fixed effects, and the within-pair baseline gap, with standard errors clustered at the city-pair level. Controls are taken from the CSMAR China Regional Economy Database at the whole-prefecture level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

6.4. Specification sensitivity for coverage breadth

The border effect on coverage breadth is not consistent across specifications and requires separate discussion. **Table 7** places the two dimensions side by side under an identical sequence of specifications. The coefficient for usage depth is positive and significant throughout, ranging from 0.0100 to 0.0248; that for coverage breadth reverses from −0.0130 in specifications (2) and (3) to 0.0194 in specification (5), changing both sign and significance. The reversal itself indicates that no robust border effect exists for this dimension.

The reason is readily explained: cross-city dispersion in coverage breadth is far larger than in usage depth, the within-province mean log gap being 0.1478 against 0.0945, about 56% higher. City-specific heterogeneity dominates coverage breadth and swamps the border component when it is not absorbed; once endpoint-city fixed effects absorb it, the residual border component emerges. This study takes specification (4)—distance, year fixed effects, and the within-pair baseline gap—as the preferred baseline, for two reasons. The baseline gap addresses directly the concern that cross-province pairs differ systematically in initial development, without removing the cross-sectional variation through which the border effect operates; and in dyadic data each city appears in several pairs, so endpoint-city fixed effects absorb province-level means and leave *Cross* identified only by within-city variation across a city's neighbors, which amounts to over-control. The conclusions therefore rest on usage depth, and no robust border effect is attributed to coverage breadth.

Table 7. Border effect for the two dimensions under an identical specification sequence

Specification	Coverage breadth	Usage depth
(1) *Cross* only	0.0003	0.0248***
(2) + centroid distance	−0.0130	0.0152***
(3) + year fixed effects	−0.0130	0.0152***
(4) + within-pair baseline gap (preferred)	0.0015	0.0100**

Specification	Coverage breadth	Usage depth
(5) + endpoint-city fixed effects	0.0194***	0.0234***
(6) Endpoint-city FE + baseline gap	0.0115***	0.0152***

Note: Standard errors are clustered at the city-pair level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

7. Conclusion and policy implications

Using a balanced panel of 329 prefecture-level cities over 2011–2023, this study examines the evolution in the level and the spatial structure of regional disparity in China's digital financial inclusion. Aggregate disparity narrowed substantially, largely before 2016, while its structure reversed: the relative importance of between-province disparity rose steadily, a randomization test shows this is not a statistical artifact, and province far outperforms the four major regions in explanatory power. The rejection of overall convergence applies to the aggregate index alone; most testable provinces do not reject within-province convergence, and stronger persistence at the two extremes holds under three state definitions. After controlling for centroid distance, the within-pair difference in initial development and six city-level variables covering output per capita, population, industrial structure and financial depth, cross-province adjacent pairs still display significantly larger usage-depth gaps than within-province pairs (0.0091, with a randomization-inference empirical *P*-value of 0.013), robust across distance bandwidths, alternative border definitions and a pseudo-border placebo; neither coverage breadth nor the aggregate index is significant under the same specification. It should be stressed that what is identified is a conditional association between crossing a provincial boundary and the size of the within-pair gap, given the controls included; reading it as a causal effect of the administrative boundary requires the further assumption that unobserved within-pair factors are uncorrelated with provincial affiliation. The pseudo-border placebo and randomization inference test that assumption indirectly, but cannot rule out its failure.

One possible interpretation runs as follows. The access stage of digital finance is supplied uniformly by nationwide platforms, and a given application offers the same functions and thresholds in any city; usage intensity, by contrast, depends on the local credit environment, credit-reporting data availability, supervisory practice and the operating scope of regional financial institutions, all organized in China with the province as the basic unit. Technological supply is national while the relevant institutions are provincial, and the gap between the two shows up as the additional dispersion of cross-province pairs. It must be stressed that this mechanism is not tested here: the paper contains no measure of credit-reporting interoperability, supervisory intensity, or institutional operating scope, so the account is a conjecture consistent with the evidence rather than an identified causal mechanism. The Institute of Digital Finance at Peking University [3] takes a similar view, holding that coverage breadth measures fairness at the level of opportunity whereas usage depth reflects balance at the level of outcomes. Liu *et al.* [26] found that regional competition affects the efficiency with which digital financial inclusion is promoted, which offers indirect corroboration. Accordingly, inclusive finance policies centered on expanding access have largely achieved their initial objectives [27], and provincial fragmentation now deserves greater attention. Feasible measures include promoting cross-province credit-reporting and data interoperability [28], relaxing restrictions on cross-province business by regional financial institutions [29,30], unifying supervisory standards for digital finance, and piloting integrated financial services in areas straddling provincial borders.

Three limitations remain. First, the sample with city-level controls covers only 53% of pair-year observations, and the missingness is not random; although the same-subsample baseline is used as the

benchmark and randomization inference is applied, sample selection may still affect the estimates. The study also identifies the existence and evolution of the border effect without testing its causes; introducing credit-reporting interoperability, the intensity of local financial supervision, and the distribution of regional bank branches is the next priority. A spatial discontinuity design using distance from the city centroid to the nearest provincial border as the running variable was considered but not adopted, because the spatial extent of a prefecture-level city far exceeds the bandwidths such designs require and the running variable cannot be measured precisely enough at city level; with county- or grid-level data it would be the preferred strategy. Second, the border effect becomes insignificant once pairs involving a municipality are excluded, indicating heterogeneity across city types, so the conclusion applies to ordinary prefecture-level cities and should not be extrapolated to central cities. Third, by 2023 the overall Gini coefficient had fallen to 0.046, so the absolute level of disparity is not high, and the policy urgency of the structural conclusions should be read with that context in mind.

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Disclosure statement

The authors declare no conflict of interest.

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Appendix

Notes on data and computation

(1) Index data: the Peking University Digital Financial Inclusion Index. The main sample covers 2011–2023 and comprises 329 prefecture-level units with no missing values on the four indices, giving 4,277 observations.

(2) The 2024 column in the raw data equals the 2023 values multiplied by a common factor of 1.08 for every city and every dimension (standard deviation zero), and is therefore a uniform proportional extension. Because all measures used here are invariant to within-year proportional rescaling, it is used only for the invariance check reported in the last row of **Table 5** and does not enter the main results.

(3) Geographical adjacency and centroids are constructed by the authors from vector boundary data for China's prefecture-level administrative divisions, yielding 971 adjacent pairs and 867 usable city pairs (583 within-province, 284 cross-province). Provincial affiliation is identified from the first two digits of the city administrative division code; provincial capitals are identified by the last two digits being 01, with the four municipalities added. Moran's I uses $S_0 = 325$. In 2012, the usage-depth index of Haibei Tibetan Autonomous Prefecture and the coverage-breadth index of Yushu Tibetan Autonomous Prefecture are negative, so the Theil index cannot be computed for that year; the years reported exclude 2012.

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