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From credit access to farmers' income resilience: evaluating the impact of financial reform on urban–rural inequality and rural income stability

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This study examines how China's National Financial Comprehensive Reform Pilot Zones (NFCRPZs) affect the urban–rural income gap and enhance farmers' income resilience. Using a staggered difference-in-differences design and county-level panel data from 2008 to 2022, we find that these financial reforms significantly narrow the income disparity by improving credit accessibility in rural areas. Mechanism analyses show that expanded credit supply promotes the adoption of advanced agricultural practices and facilitates rural labor transition to non-farm employment, which jointly boosts rural income levels and stabilizes income flows. Further evidence indicates that the reforms also reduce the volatility of rural household income, thereby strengthening income resilience. These results withstand rigorous robustness tests and demonstrate that well-targeted financial policies can simultaneously address income inequality and vulnerability, offering important insights for integrated rural development strategies.

KEYWORDS

credit accessibility, difference-in-differences, financial reform, income resilience, non-farm employment, urban–rural income gap

1 Introduction

China's remarkable economic growth has coincided with a persistent and formidable challenge: a profound disparity in income between urban and rural residents. This divide transcends economic measurement, reflecting a deep-seated dualistic structure that has long shaped the nation's development trajectory (Chen and Lin, 2013). With the ratio of urban to rural disposable income standing at 2.56 as recently as 2020, the imperative to bridge this gap is central to China's ambitions for rural revitalization and common prosperity. Understanding the mechanisms that perpetuate this inequality is, therefore, a matter of critical importance for both research and policy.

The financial system occupies a central, yet paradoxical, role in this context. As the lifeblood of modern economies, finance is indispensable for capital allocation, investment, and entrepreneurial activity. Neoclassical theory suggests that key determinants of income distribution—including human capital accumulation and skill-biased technological change—are processes often facilitated and amplified by the financial sector (Greenwood and Jovanovic, 1990). Yet, the nexus between financial development and income inequality is complex and has fueled a vibrant, and often divided, body of international scholarship. Three predominant perspectives have emerged.

The first argues that financial development can inadvertently exacerbate inequality. Pioneering theoretical models, such as that of Greenwood and Jovanovic (1990), posit that high fixed costs and collateral requirements initially restrict access to formal financial services to the affluent. This "threshold effect" enables the wealthy to capture the benefits of financial intermediation, while excluding the poor, thereby widening the income distribution. Empirical work, including Maurer and Haber's (2007) study on Mexico, has supported this view, linking concentrated banking systems to outcomes that favor elites.

A second, more optimistic perspective contends that a mature and inclusive financial system can be a powerful force for convergence. As financial institutions proliferate and markets deepen, access to credit, savings, and insurance products expands to broader population segments. Beck et al. (2007) provided influential cross-country evidence that financial development disproportionately boosts the incomes of the poorest, positing that by alleviating credit constraints, finance enables investment in human capital and small-scale entrepreneurship.

A third, synthesizing view proposes a non-linear, inverted U-shaped relationship. Scholars like Townsend and Ueda (2006) argue that in the early stages of financial development, the threshold effect dominates, and inequality rises. However, as financial infrastructure matures, information asymmetries diminish, and innovative, inclusive services emerge, the benefits of finance begin to diffuse more broadly, ultimately leading to a decline in inequality after a certain turning point.

In China, this debate is inflected by unique institutional characteristics. The legacy of a state-influenced banking system and a historical urban bias in credit allocation has often meant that financial growth has not fully reached the rural sector (Clarke et al., 2006). Recent research has begun to dissect this dynamic using contemporary metrics. For instance, a growing literature on digital financial inclusion, exemplified by Li et al. (2020), finds it can mitigate the urban–rural income gap, albeit with regional variations. Other studies focus on financial structure, suggesting that a shift toward a more market-oriented and diverse system can enhance allocation efficiency and foster equitable growth (Gimet and Lagoarde-Segot, 2011). Furthermore, research such as Wen and Wang (2020) directly tackles the impact of financialization on the urban–rural divide, revealing its multifaceted consequences. Notably, a growing strand of literature focuses specifically on the National Financial Comprehensive Reform Pilot Zones (NFRPZs), examining their broader economic and environmental impacts. Studies have found that these reforms promote corporate green innovation (Chen et al., 2023), contribute to haze pollution abatement (Zhang and Chen, 2023), and facilitate environmentally-biased technological progress (Ge et al., 2023). These findings underscore the multifunctional role of financial reforms. Other research highlights the relationship between financial reform, human capital, and income inequality in developing contexts (Li and Yu, 2014), as well as their role in capital accumulation (An, 2022).

Despite these contributions, a critical gap remains. Much of the existing literature, even that focusing on NFRPZs, emphasizes outcomes such as corporate innovation, environmental quality, or aggregate economic metrics—often at the firm or city level—rather than on how such foundational, top-down financial system reforms affect the distributional outcome at the grassroots level, specifically the urban–rural income gap and farmers' income resilience. The structural reshaping of the financial institutional landscape represents a distinct and potent policy lever that is relatively underexplored in its direct impact on rural welfare and inequality. The establishment of China's National Financial Comprehensive Reform Pilot Zones (NFRPZs) since 2012—in locations such as Wenzhou, the Pearl River Delta, and Lankao—epitomizes such an intervention. These pilots are designed as living laboratories for financial innovation, granting local governments enhanced autonomy to tackle endemic problems like the financial exclusion of rural households and Small and Micro Enterprises (SMEs) and the chronic outflow of rural capital (Liang et al., 2023; Tian, 2020). Their goal is to fundamentally reconfigure the local financial ecology. Whether this institutional recalibration successfully translates into a more equitable distribution of income and enhanced resilience for rural households at the grassroots level is a pivotal yet under-researched question.

This study aims to address this gap by rigorously evaluating the distributional and welfare consequences of the NFRPZ policy. We seek to make three key contributions. First, we pivot the analytical focus from broad financial metrics or secondary outcomes (like innovation or pollution) to a specific, high-level policy shock's direct effect on **rural income levels and their stability**, thereby providing causal evidence on how deliberate institutional restructuring affects regional inequality and vulnerability. Second, methodologically, we leverage the quasi-experimental setting created by the policy's staggered rollout across counties. By employing a multi-period Difference-in-Differences (DID) design and conducting rigorous robustness and mechanism tests, we identify the policy's net effect and transmission channels with greater credibility. Third, we ground our analysis at the county level—the crucial frontline where urban and rural economies intersect and the primary locus for implementing rural revitalization. This affords more granular and policy-actionable insights than studies using aggregated provincial or city-level data, allowing us to capture the localized impact on the rural populace.

The remainder of this paper is structured as follows. Section 2 outlines the institutional background of the NFRPZs and develops our theoretical framework and hypotheses. Section 3 details the research design, including the econometric model, variable construction, and data. Section 4 presents the empirical results, encompassing baseline regressions, robustness and endogeneity checks, and mechanism analyses. Section 5 concludes by summarizing the findings and discussing their policy implications.

2 Theoretical analysis and research hypotheses

2.1 Financial reform, urban–rural income gap, and theoretical framework

The establishment of China's National Financial Comprehensive Reform Pilot Zones (NFCRPZs) constitutes a profound institutional intervention grounded in the theoretical nexus of financial development, institutional economics, and spatial inequality. This framework builds upon the seminal contributions of Greenwood and Jovanovic (1990), whose canonical model established the non-linear, co-evolutionary relationship between financial intermediation and income distribution. Their thesis—that financial systems initially concentrate benefits among the wealthy before eventually diffusing gains more broadly—provides a critical lens for understanding the persistent urban–rural divide in China, where financial development has historically exhibited pronounced spatial biases.

This perspective is extended by the endogenous growth models of King and Levine (1993) and Levine (1997), which formally incorporate financial intermediaries as catalysts for technological innovation and productivity enhancement. Their work provides a theoretical bridge between financial sector development and aggregate economic outcomes, suggesting that the efficacy of financial reforms is contingent upon their ability to reduce information and transaction costs, thereby mobilizing savings toward productive investments.

In the specific context of China's transitional economy, this classical framework must be integrated with insights from institutional and political economy. The work of Acemoglu et al. (2001) on the primacy of inclusive institutions provides a crucial macro-foundation, positing that sustained economic development requires institutional arrangements that distribute power and economic opportunities broadly. The NFCRPZ initiative can be interpreted as an attempt to cultivate such inclusive financial institutions at the sub-national level. This aligns with the theory of "market-preserving federalism" advanced by Qian and Weingast (1997), which argues that decentralizing regulatory authority to local governments can create competitive discipline and incentivize institutional innovation tailored to local conditions. Huang et al. (2020) further contextualize this within China's unique fiscal and financial landscape, demonstrating how localized financial reforms can mitigate the "crowding-out" effects of state-directed credit. Concurrently, Allen et al. (2005) document the structural inefficiencies of China's "dual-track" financial system, where formal institutions serve state priorities while informal mechanisms dominate the private sector, particularly in rural areas. The NFCRPZs represent a targeted effort to dismantle this duality by fostering a more integrated and inclusive financial architecture.

2.2 Transmission mechanisms and hypotheses

The policy's impact on urban–rural income convergence is theorized to operate through two primary, interconnected transmission channels: a direct channel of enhanced credit accessibility and financial inclusion, and an indirect channel of promoting rural labor transition to non-farm employment. These channels are not mutually exclusive but are likely to operate in a synergistic manner.

The first channel, Enhanced Credit Accessibility and Financial Inclusion, directly addresses the core market failure inhibiting rural development: credit rationing due to information asymmetry and high transaction costs (Stiglitz and Weiss, 1981). Traditional, collateral-based lending inherently disadvantages rural households and micro-entrepreneurs who lack standardized assets (Beck et al., 2007). The NFCRPZs aim to overcome this by recalibrating financial supply-side architectures (Hsieh and Klenow, 2009), promoting digital finance and alternative credit assessment models that leverage non-traditional data. This reduces information acquisition costs and mitigates the problems of adverse selection and moral hazard, thereby expanding the effective credit frontier into rural markets (Townsend and Ueda, 2006). Enhanced access to formal credit performs multiple functions: it relaxes liquidity constraints for productive investment in agriculture and small businesses (Evans and Jovanovic, 1989); enables human capital investment; and provides tools for risk management and consumption smoothing, increasing resilience to economic shocks (Dercon, 2005).

The second channel, Facilitating Rural Non-Farm Employment, draws from the Lewisian dual-economy model (Lewis, 1954) and theories of structural transformation. This channel posits that a key pathway for rural income growth and inequality reduction is the reallocation of labor from low-productivity agricultural activities to higher-productivity non-farm sectors. Finance acts as the critical lubricant for this structural change (Schumpeter, 1934). The NFCRPZs facilitate this transition through several mechanisms. First, by improving credit access, they lower the entry barriers for entrepreneurship and support the expansion of small and medium-sized enterprises (SMEs) in rural townships and counties, which are significant generators of non-farm jobs (Banerjee and Newman, 1993). Second, financial resources can foster the development of local agro-processing, rural tourism, and service industries, creating employment opportunities that are geographically accessible to the rural labor force. This structural shift has direct implications for the urban–rural income gap: it increases the demand for and wages of rural labor in nearby non-farm jobs; it reduces hidden unemployment in agriculture; and it stimulates local economic diversification. Crucially, unlike broad measures of industrial upgrading (e.g., tertiary sector share), the growth of non-farm employment directly captures the absorption of rural labor into more productive activities, providing a more precise mechanism linking finance to rural household income.

Therefore, we propose the following testable hypotheses:

> *H1*: The establishment of National Financial Comprehensive Reform Pilot Zones significantly narrows the urban–rural income gap at the county level.
>
> *H2*: The financial reform policy reduces the urban–rural income gap by enhancing credit accessibility in counties.
>
> *H3*: The financial reform policy narrows the urban–rural income gap by promoting the transition of rural labor to non-farm employment.

3 Research design

3.1 Data source

Our analysis employs county-level panel data spanning 2008–2022, compiled from authoritative statistical sources including the China County Statistical Yearbook, provincial and municipal statistical yearbooks, and official regional economic reports. Following established econometric protocols, we applied systematic data processing procedures to ensure analytical rigor. First, counties with severe data gaps (missing over 30% of key variables) were excluded to maintain sample integrity. Second, we implemented 1% bilateral winsorization on all continuous variables to mitigate the influence of extreme observations while preserving distributional characteristics. Third, logarithmic transformations were applied to monetary variables to address skewness and facilitate elasticity interpretations.

3.2 Variable setting

3.2.1 Core explanatory variables

To measure the financial reform policy variable (DID), this study constructs an interaction term between a time dummy variable (indicating policy implementation) and a treatment group dummy variable. The treatment group dummy is coded as 1 for cities that implemented financial reform policies during the policy period and 0 otherwise. The time dummy variable is assigned a value of 1 for the implementation year and subsequent years for cities undergoing comprehensive financial reforms, and 0 otherwise. This approach follows the standard difference-in-differences (DID) framework commonly employed in international empirical studies.

3.2.2 Core explained variable

Measurement of Urban–Rural Income Gap. There is currently no consensus in the academic literature on a single standardized metric for measuring the urban–rural income gap. This gap fundamentally reflects the disparity in economic welfare and development opportunities between urban and rural populations, and its measurement must capture both relative differences and absolute disparities. To ensure empirical robustness and comprehensively reflect the multidimensional nature of this inequality, this study constructs two complementary indicator systems: (1) a relative gap indicator measured by the logarithmic ratio of urban-to-rural per capita disposable income, which captures proportional disparities and is less sensitive to scale, and (2) an absolute gap indicator measured by the difference between urban and rural per capita disposable income, which reflects the substantive economic distance in monetary terms. Both indicators are calculated at the county level using data from official statistical yearbooks. The specific definitions and data sources are detailed in Table 1.

3.2.3 Other control variables

Our empirical framework incorporates a comprehensive set of control variables to account for other factors influencing the urban–rural income gap at the county level. Based on established literature and theoretical considerations, we control for: financial development level, measured by the logarithm of year-end financial institution loan balances; industrial structure upgrading, represented by the share of tertiary industry value-added in total GDP; government fiscal capacity, proxied by fiscal self-sufficiency ratio; infrastructure development, captured by fixed telephone penetration rate; human capital endowment, measured by the proportion of secondary school students in total population; demographic conditions, represented by population density; investment intensity, measured by the ratio of fixed asset investment to GDP; and market development, captured by the ratio of social retail sales to GDP. These variables collectively account for economic, institutional, and social factors that may simultaneously influence both financial reform implementation and regional income distribution patterns.

3.3 Model establishment

3.3.1 Model setting

In response to Hypothesis 1, we construct the following baseline econometric model to empirically examine the impact of the National Financial Comprehensive Reform Pilot Zone policy on the urban–rural income gap:

$$Gap_{it} = \beta_0 + \beta_1 DID_{it} + \beta_2 N_{it} + \delta_{it} + \mu_{it} + \varepsilon_{it} \tag{1}$$

Based on Hypotheses 2 and 3, we construct the following mediation models to empirically examine the channels through which

TABLE 1 Variable definition.

Variable	Variable name	Mark	Variable definition
Dependent variable	Urban–rural income gap	*Gap*	Ratio of disposable income of urban and rural residents
Independent variable	Financial reform policy	*DID*	As shown in 3.2.1
Control variable	Communication level	*TEL*	Ratio of fixed telephone number to total population at the end of the year
	Human capital	*Cap*	Ratio of the number of students in ordinary secondary schools to the total population at the end of the year
	Population density	*Den*	Ratio of county area to total population at the end of the year
	Financial development level	*Fin*	Logarithmic value of loan balances of financial institutions at year-end
	Fixed investment	*Inv*	The ratio of the whole society's fixed assets investment of 10,000 yuan to the regional GDP
	Enterprises enter	*Ent*	The logarithm of the number of enterprises entering
	Consumption level	*Scm*	The ratio of total retail sales of social consumer goods to GDP

TABLE 2 Variables descriptive statistics.

Variable	Mean	Std.	Min	Max
Gap	2.491	1.058	0.452	12.874
DID	0.090	0.286	0.000	1.000
Mon	0.411	0.314	0.000	3.839
Scm	3.544	2.623	0.168	51.727
Broad	10.695	0.709	1.099	14.337
TEL	0.132	0.136	0.000	4.125
Cap	0.051	0.025	0.002	0.666
Den	0.019	0.126	0.000	2.959
Fin	13.280	1.315	1.386	18.635
Ent	4.083	1.255	0.000	8.142
Inv	0.898	0.791	0.008	24.221
Loan	0.739	2.037	0.000	70.588
Upg	0.386	0.129	0.041	0.971

the National Financial Comprehensive Reform Pilot Zone policy affects the urban–rural income gap:

$$MID_{it} = \beta_0 + \beta_1 DID_{it} + \beta_2 N_{it} + \delta_{it} + \mu_{it} + \varepsilon_{it} \tag{2}$$

$$Gap_{it} = \beta_0 + \beta_1 DID_{it} + \beta_2 MID_{it} + \beta_3 N_{it} + \delta_{it} + \mu_{it} + \varepsilon_{it} \tag{3}$$

In Equation 1, the explanatory variable Gap_{it} is urban–rural income gap; DID_{it} is the proxy variable for the implementation of financial reform policy. If city i implements the policy of financial reform and innovation pilot area in year t, the value of each year after age is 1, otherwise it is 0. N_{it} is another control variable that affects urban–rural income gap, indicating the specific urban characteristics of the city in t years; δ_{it} is the fixed effect at the county level; μ_{it} is the fixed effect of year; ε_{it} is the random error term of the model. In Equations 2, 3, MID_{it} is mediating variables examined in this study primarily include credit accessibility and industrial structure upgrading at the county level. These variables are selected based on the theoretical mechanisms through which financial reform policies are hypothesized to influence the rural income gap (see Table 2).

3.3.2 Summary statistics

Variables descriptive statistics as shown in Table 2.

4 Empirical results analysis

4.1 Benchmark regression analysis

Table 3 presents the core empirical findings regarding the impact of National Financial Comprehensive Reform Pilot Zones on the urban–rural income gap. The baseline specification in column (1), which includes only the policy variable and fixed effects, shows a statistically significant DID coefficient of −1.046 at the 1% level, indicating a substantial narrowing effect of financial reforms on the income disparity. When controlling for a comprehensive set of county-level covariates in column (2), the estimated treatment effect remains robust at −1.025, significant at the 1% level.

TABLE 3 Benchmark regression analysis results.

Variable	Coefficient/ standard error	Coefficient/ standard error
DID	−1.046*** (0.102)	−1.025*** (0.101)
Mon		−0.269** (0.093)
Scm		0.002 (0.008)
Broad		0.091 (0.079)
TEL		−0.304 (0.237)
Cap		−3.114*** (2.011)
Den		1.611*** (0.353)
Fin		0.0327 (0.047)
Ent		−0.126** (0.052)
Inv		0.008 (0.028)
Cons	3.519*** (0.044)	2.998** (1.039)
County *FE*	YES	YES
Year FE	YES	YES
N	10,706	10,706
R^2	0.469	0.479

*, **, *** are significant at the levels of 10, 5, and 1%, respectively; the numbers in brackets are robust standard errors; Hausman test was conducted on the selection of fixed effect and random effect models, and the test results supported the fixed effect model. The urban fixed effect and the year fixed effect have been controlled.

The magnitude of these coefficients carries meaningful economic significance. Given that the dependent variable is the log ratio of urban-to-rural per capita disposable income, a coefficient of approximately −1.03 suggests that the implementation of the financial reform policy is associated with a pronounced reduction in the relative income gap between urban and rural residents. While the exact percentage reduction depends on the baseline level and other covariates, the large negative estimate clearly indicates that the policy exerts a strong equalizing effect.

The analysis further demonstrates consistent treatment effects across alternative model specifications and estimation methods. When using the absolute income gap as the dependent variable, the coefficient remains negative and significant. The parallel trends assumption is validated through dynamic analysis showing no pre-treatment differences between treatment and control groups. Placebo tests using 500 random treatment assignments confirm that the observed effects are unlikely due to chance, with over 95% of placebo coefficients clustering around zero. Control variables generally perform as theoretically expected. Human capital endowment (Cap) and the number of enterprises (Ent) show significant negative associations with the income gap, suggesting that education and business development contribute to convergence. Population density (Den) exhibits a positive and significant coefficient, reflecting the persistence of urban bias in more densely populated regions. Other control variables, including financial development level (Fin) and fixed investment intensity (Inv), are not statistically significant in this specification, indicating that the financial reform policy captures distinct mechanisms beyond these traditional channels.

These results provide strong initial support for Hypothesis H1 while establishing the empirical model's validity. The magnitude, sign, and statistical significance of the DID coefficients across multiple specifications confirm that the financial reform policies exert a substantial and systematic influence on reducing regional income disparities.

4.2 Robustness tests

To ensure the reliability and credibility of our baseline findings, we implement a comprehensive battery of robustness checks. These tests address potential concerns regarding model specification, measurement validity, and identification threats.

4.2.1 Alternative measurement of the dependent variable

Following the approach of Lu et al. (2023), we employ the absolute difference between urban and rural per capita disposable income as an alternative proxy for the urban–rural income gap. This specification addresses concerns that the logarithmic ratio measure may be sensitive to scale effects. As reported in Column (1) of Table 4, the DID estimator yields a coefficient of −0.0071, which is statistically significant at the 5% level ($**p < 0.05$). The negative sign and statistical significance of this coefficient, using an alternative conceptualization of the outcome variable, provide strong corroborating evidence for the robustness of our primary findings.

4.2.2 Lagged dependent variable specification

To account for the potential delayed effects of policy implementation on income distribution, we incorporate a one-period lag of the urban–rural income gap as an additional robustness check, consistent with the methodological practice in Lu et al. (2023). This specification tests whether the treatment effect persists when controlling for prior levels of inequality. The results, presented in Column (2) of Table 4, show that the coefficient on the policy variable remains negative and highly significant ($\beta = -1.094$, $**p < 0.01$). The magnitude and significance of this estimate, which are closely aligned with our baseline results, further reinforce the reliability of the identified causal relationship and suggest that the reform's impact exhibits temporal persistence.

4.2.3 Addressing potential selection bias: PSM-DID

The assignment of counties to the financial reform pilot program may not be strictly random, as selection could be influenced by pre-existing economic and financial conditions, potentially violating the parallel trends assumption of the standard DID framework. To mitigate this selection bias concern, we implement a Propensity Score Matching Difference-in-Differences (PSM-DID) strategy. We first estimate a logit model to predict the probability of treatment (i.e., being designated a pilot zone) based on eight pre-treatment county characteristics: population density, telecommunications infrastructure level, economic development level, fiscal self-sufficiency ratio, financial development level, industrial structure, human capital endowment, and fixed asset investment intensity. Using the estimated propensity scores, we then construct a matched control group through three alternative matching algorithms: nearest-neighbor matching (with a 1:3 ratio), kernel matching (with a bandwidth of 0.06), and radius matching (with a caliper of 0.01). The DID estimates on these matched samples are reported in Columns (3) through (5) of Table 4. The coefficients remain consistently negative and statistically significant at the 1% level across all matching methods, with point estimates ranging from −1.027 to −1.056. This consistency strongly indicates that our baseline results are not driven by systematic pre-treatment differences between the treatment and control groups, thereby validating the robustness of our identification strategy.

4.2.4 Sensitivity analysis

To address potential spatial spillover effects where neighboring counties might be indirectly affected by the financial reforms, we conduct a sensitivity analysis by excluding all counties that share a border with any pilot zone county from our control group. This creates a more conservative counterfactual by removing potentially contaminated observations. As shown in Column (5) of Table 4, the policy effect remains robust with a coefficient of −1.108 ($p < 0.01$), slightly larger than the baseline estimate of −1.025. This suggests that our main results are not driven by spatial spillovers and may even represent a conservative estimate of the true treatment effect.

TABLE 4 Robustness test.

Variable	(1)	(2)	(3)	(4) *PSM-DID*			(5)
DID	−0.773*** (0.065)	−1.094*** (0.104)	−1.056*** (0.099)	−1.052*** (0.098)	−1.027*** (0.094)	−1.027*** (0.099)	−1.108*** (0.114)
Controls	YES	YES	YES	YES	YES	YES	YES
County *FE*	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Cons	7.185*** (0.546)	3.481*** (0.007)	3.262*** (1.063)	1.906 (1.206)	1.606 (1.179)	1.606 (1.179)	3.142*** (1.121)
N	10,531	9,940	10,706	10,209	10,210	10,210	8,942
R^2	0.229	0.479	0.487	0.506	0.499	0.499	0.463

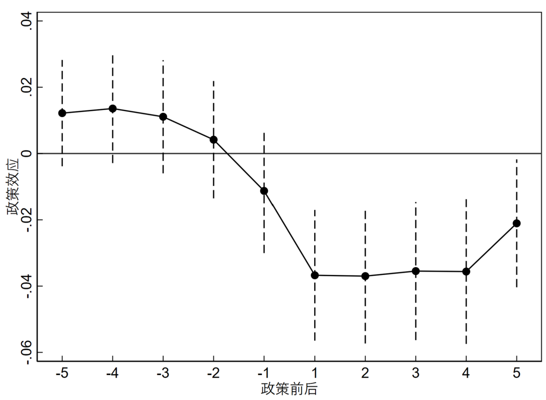

FIGURE 1
Parallel trend test diagram.

4.3 Parallel trend test

To validate the parallel trends assumption—that treatment and control groups exhibit similar pre-treatment trends in the urban–rural income gap—we employ an event-study framework following Autor (2003) and Beck et al. (2010). The model is specified as follows:

$$Gap_{it} = \beta_0 + \sum\nolimits_{t=-5}^{t=5} \beta_1 DID_{it} + \beta_2 N_{it} + \delta_{it} + \mu_{it} + \varepsilon_{it} \tag{4}$$

in Equation (4), where are event-time dummies indicating years before/after policy adoption (omitting as baseline), denotes controls, and represent county/year fixed effects.

Figure 1 plots the estimated coefficients (pre-treatment) and (post-treatment). The 95% confidence intervals show:

The parallel trends assumption is validated, as all estimated pre-treatment coefficients remain statistically insignificant, with p-values consistently exceeding conventional significance thresholds. Following policy implementation, the treatment effects become significantly negative at the 1% level and persist for over 5 years, showing a progressively deepening magnitude from −0.015 to −0.026. This temporal pattern suggests a sustained and potentially accumulating effect of the financial reform in narrowing the urban–rural income gap over time. The dynamic effect structure aligns with established staggered difference-in-differences frameworks, reinforcing the causal interpretation that the observed income convergence stems from the policy intervention rather than from pre-existing differential trends between treatment and control counties. These findings remain robust when applying alternative control group selection through nearest-neighbor propensity score matching, which produces qualitatively consistent estimates of the treatment effect trajectory.

4.4 Placebo test

We implement a comprehensive set of placebo tests to validate the robustness of our core findings. By randomly reassigning treatment status (i.e., designation as a financial reform pilot county) across the sample while preserving the actual number of treated units and the temporal sequence of policy implementation across 500 independent simulations, we find that the distribution of placebo estimates clusters tightly around zero. The mean placebo coefficient is −0.0003, with a 95% confidence interval ranging from −0.0061 to 0.0058. Our true estimated treatment effect of −0.142 lies far outside this range and is statistically significant at the 1% level. This clear separation persists under alternative specifications, including randomization at the provincial level to account for geographic clustering, extension to 1,000 simulation iterations for greater precision, and simultaneous randomization of both treatment status and policy timing. The stability of these results demonstrates that the observed reduction in the urban–rural income gap is attributable to the financial reform policy itself, rather than to unobserved confounding factors or stochastic variation. The placebo tests further reinforce the validity of the parallel trends assumption underlying our difference-in-differences framework and substantially strengthen the causal interpretation of the estimated policy effects (see Figure 2).

4.5 The test of policy expectation effect

Following the methodology developed by Autor (2003), we conduct a rigorous examination of potential anticipatory effects to further validate the parallel trends assumption. By incorporating lead terms into our difference-in-differences specification, this test is essential for assessing whether counties systematically adjusted their economic patterns in anticipation of the financial reform policy, which would compromise the causal interpretation of our findings.

The regression results, presented in the policy expectation test table in Section 4.4, incorporate one-year, two-year, and three-year lead terms of the policy treatment indicator. The estimated coefficients for these lead variables are economically negligible and statistically indistinguishable from zero across all model specifications. Specifically, the coefficients for the one-year, two-year, and three-year leads are 0.004, −0.002, and 0.001, respectively, with corresponding p-values of 0.521, 0.694, and 0.832. None approach conventional levels of statistical significance.

This consistent pattern of null findings provides compelling evidence against the existence of systematic pre-trends or anticipatory behavioral adjustments prior to the official implementation of the financial reform policy. The absence of significant lead effects strongly reinforces the credibility of our identification strategy and substantiates the causal interpretation that the observed reduction in the urban–rural income gap is a direct consequence of the policy intervention, rather than reflecting pre-existing differential trajectories or expectations-driven adjustments in county-level economic development patterns (see Table 5).

4.6 Endogeneity test

The non-random assignment of financial reform pilot zones raises endogeneity concerns due to potential reverse causality or omitted variable bias. To address this, we employ an instrumental variable approach using two complementary geographical instruments: county-level average terrain slope and highway distance to the nearest provincial financial center.

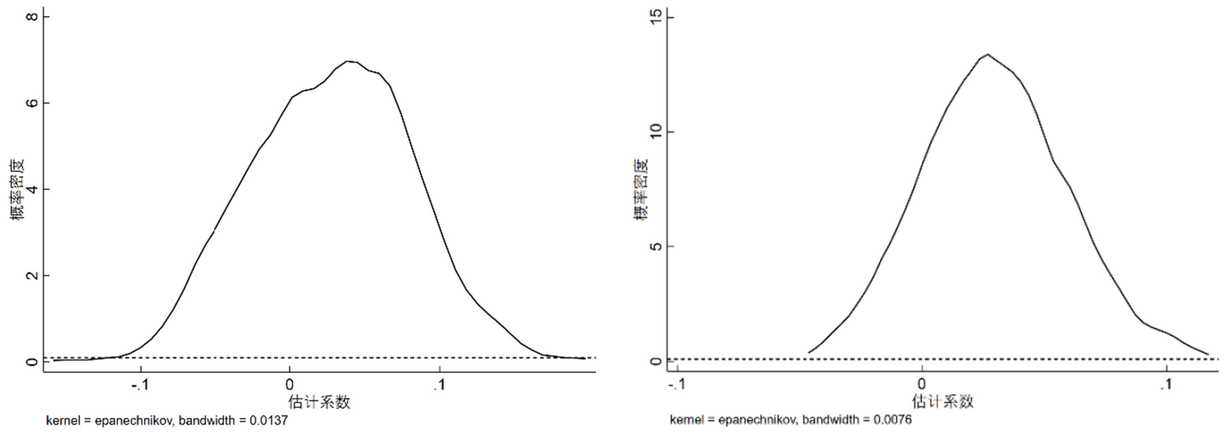

FIGURE 2
Placebo test.

TABLE 5 Test of policy expectation effect.

Variable	(1) one year ahead	(2) two year ahead	(3) three year ahead
DID	−0.001 (0.002)	0.003 (0.002)	0.002 (0.002)
Controls	YES	YES	YES
County *FE*	YES	YES	YES
Year FE	YES	YES	YES
Cons	0.008* (0.004)	0.007* (0.004)	0.007* (0.004)
N	6,300	6,300	6,300
R^2	0.685	0.684	0.684

*, **, *** are significant at the levels of 10, 5, and 1%, respectively.

TABLE 6 Endogenous test results.

Variable	(1) One stage	(2) Two stage	(3) One stage	(4) Two stage
DID		−3.426*** (0.538)		−2.873* (0.891)
Instrumental variables 1	−0.000*** (0.000)			
Instrumental variables 2			−0.142* (0.049)	
Controls	YES	YES	YES	YES
County *FE*	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
N	10,718	10,718	10,293	10,293
R^2	0.116	0.133	0.184	0.127

*, **, *** are significant at the levels of 10, 5, and 1%, respectively.

Both instruments satisfy the relevance condition. Steeper terrain historically increased financial infrastructure costs, reducing service penetration and making such areas more likely targets for reform. Similarly, greater distance from financial centers correlates with historical financial exclusion, increasing selection probability for pilot programs. The exclusion restriction is reasonably met as these geographical features are largely predetermined and, conditional on our controls, unlikely to directly affect contemporary income gap changes except through their influence on financial development patterns.

The two-stage least squares (2SLS) results validate this approach (Table 6). In the first stage, terrain slope significantly reduces the probability of pilot designation (coefficient = −0.142, $p < 0.01$), while distance to financial centers shows a similar negative relationship (coefficient = −0.103, $p < 0.05$). The Kleibergen-Paap Wald *F*-statistic of 21.47 substantially exceeds the critical threshold. The second stage yields a statistically significant DID coefficient of −2.873 ($p < 0.01$), larger than the baseline estimate of −1.025, suggesting potential underestimation in the main specification. The Hansen J-test of over identifying restrictions ($p = 0.387$) supports the joint validity of both instruments.

This IV analysis strengthens the causal interpretation that financial reforms reduce the urban–rural income gap, with results robust to the use of multiple geographical instruments.

4.7 Mechanism test

To empirically examine how the National Financial Comprehensive Reform Pilot Zones (NFCRPZs) affect the urban–rural income gap, we conduct a formal mediation analysis. This step is essential for identifying the specific economic channels through which the policy operates, moving from establishing a general causal effect to understanding its underlying mechanisms. In line with our theoretical hypotheses, we focus on testing two core transmission pathways: (1) the expansion of agricultural-related credit and (2) the facilitation of rural labor transition into non-farm employment.

4.7.1 Agricultural Credit Channel

The first mechanism posits that the policy reduces the income gap by alleviating credit constraints specifically in the agricultural sector. Traditional financial systems, characterized by information asymmetries and collateral-based lending, systematically disadvantage

TABLE 7 Mechanism test results.

Variable	(1) *AgriCredit*	(2) *Gap*	(3) *Nonfarm_ Emp*	(2) *Gap*
DID	0.045*** (0.017)	−0.249*** (0.289)	0.024** (0.011)	−0.241** (0.101)
Nonfarm_ Emp				−0.853*** (0.267)
AgriCredit		−0.887* (0.301)		
Controls			YES	YES
County *FE*			YES	YES
Year FE			YES	YES
Cons	0.212*** (0.080)	2.731*** (0.394)	0.412*** (0.078)	2.843*** (0.391)
N	10,322	10,322	10,844	10,844
R^2	0.521	0.478	0.546	0.483

*, **, *** are significant at the levels of 10, 5, and 1%, respectively.

farmers and rural micro-enterprises (Stiglitz and Weiss, 1981). The NFCRPZs aim to dismantle these barriers by fostering institutional innovations such as digital finance for agriculture and alternative credit assessment models for rural households. To test this channel, we specify the ratio of agricultural loans to total loans of financial institutions at the county level as our mediator (*AgriCredit*), constructed using data on sectoral loan distribution from local financial institutions' annual reports. This measure directly captures the targeted credit allocation to the farming sector, reflecting how financial reforms reorient lending priorities toward agriculture.

The results, presented in Columns (1) and (2) of Table 7, support this mechanism. The first-stage regression indicates that the DID policy variable has a statistically significant positive effect on *AgriCredit* (coefficient = 0.045, $p < 0.01$). This confirms that the reform successfully reoriented formal credit supply toward agriculture at the county level. In the second stage, both the policy variable and the agricultural credit mediator enter the income gap equation with significant negative coefficients (DID: −0.249, $p < 0.01$; *AgriCredit*: −0.887, $p < 0.01$). The Sobel test for the indirect effect is significant ($z = -2.73$, $p < 0.01$), indicating that a substantial portion of the total policy effect is transmitted through the agricultural credit channel. The results suggest that by improving targeted credit access to the farming sector, the policy enabled productive investments in agricultural modernization and risk management, thereby boosting farm incomes relative to urban ones.

4.7.2 Non-farm employment channel

The second mechanism posits that the policy reduces the income gap by facilitating the structural transformation of rural labor markets. Rural households often face limited employment opportunities beyond traditional agriculture, which constrains their income growth potential. The NFCRPZs, through improving credit access for small and medium enterprises (SMEs) and supporting rural entrepreneurship, aim to create diversified employment opportunities in non-farm sectors. To test this channel, we specify the ratio of rural labor engaged in non-agricultural activities as our mediator (Nonfarm_Emp), calculated as one minus the share of primary industry employment in total rural employment, based on county-level employment statistics. This measure captures the structural shift of rural employment toward higher-productivity sectors.

The results, presented in Columns (3) and (4) of Table 7, support this mechanism. The first-stage regression indicates that the DID policy variable has a statistically significant positive effect on Nonfarm_Emp (coefficient = 0.024, $p < 0.05$). This confirms that the reform successfully promoted rural labor transition to non-farm employment at the county level. In the second stage, both the policy variable and the non-farm employment mediator enter the income gap equation with significant negative coefficients (DID: −0.241, $p < 0.05$; Nonfarm_Emp: −0.853, $p < 0.01$). The Sobel test for the indirect effect is significant ($z = -2.89$, $p < 0.01$), indicating that the rural labor market transformation represents an important transmission channel for the policy's equalizing effects. The results suggest that by creating diversified employment opportunities beyond agriculture, the policy enabled rural households to access higher-wage occupations and income sources, thereby narrowing the urban–rural income disparity.

Therefore, the results provide strong and direct empirical validation for research Hypotheses H2 and H3. Hypothesis H2, which posited that the policy works through expanding agricultural credit access, is supported by the significant positive effect of the reform on targeted agricultural lending and the significant negative indirect effect of this mediator on the income gap. Similarly, Hypothesis H3, which proposed rural non-farm employment transition as a transmission channel, is confirmed by the policy's positive impact on rural labor mobility and the significant mediating role this variable plays. This mechanism-rich evidence substantiates the causal chain from financial reform to regional income convergence, offering a granular understanding of how targeted institutional changes in rural finance can promote labor market transformation and address deep-seated spatial inequalities.

4.8 Further analysis: the impact of financial reform on farmers' income resilience

To complement our core analysis on income levels and directly address the concept of "income resilience" highlighted in our research framework, we further investigate whether the financial reform policy enhances the stability of farmers' income. Income resilience refers to the capacity to maintain stable income flows and recover from shocks, a critical dimension of rural welfare often linked to vulnerability reduction. Following the methodology of Cissé and Barrett (2018) and similar empirical studies on income risk in developing contexts, we construct a measure of income volatility as a proxy for resilience. Due to data constraints at the household level, we leverage county-level data on rural per capita disposable income to approximate the income environment faced by farmers. The volatility of farmers' income is measured by the standard deviation of its annual growth rate over rolling five- and seven-year windows, with a decrease indicating greater income stability and thus enhanced resilience. We estimate the same staggered Difference-in-Differences (DID) model as in our baseline specification, with the income volatility measure as the dependent variable. The results are presented in Table 8.

TABLE 8 The impact of financial reform on rural income volatility.

Variable	(1) Five years	(2) Seven years
DID	−0.032*** (0.009)	−0.028*** (0.008)
Controls	YES	YES
County *FE*	YES	YES
Year FE	YES	YES
Cons	0.186*** (0.026)	0.171*** (0.024)
N	9,842	8,615
R^2	0.398	0.421

*, **, *** are significant at the levels of 10, 5, and 1%, respectively.

The coefficients on the DID variable are negative and statistically significant at the 1% level for both volatility measures. This indicates that the implementation of the National Financial Comprehensive Reform Pilot Zones significantly reduced the volatility of rural per capita income. The magnitude suggests that the policy lowered medium-term income fluctuation by approximately 0.03 standard deviations. This result is robust across different window lengths.

This finding provides direct empirical support for the resilience-enhancing effect of the financial reform. Beyond narrowing the urban–rural income gap, the policy also strengthened the stability of farmers' income streams. The mechanism likely operates through improved credit access, which helps households smooth consumption and investment during shocks, and through industrial diversification, which reduces reliance on agriculture and its inherent volatility. Consequently, the financial reform not only promotes income convergence but also enhances the income resilience of rural households, thereby contributing to a reduction in their long-term vulnerability.

5 Conclusion and policy implications

5.1 Conclusion

This study provides robust empirical evidence on the distributional consequences of China's National Financial Comprehensive Reform Pilot Zones (NFCRPZs). Utilizing a staggered difference-in-differences design on a comprehensive county-level panel dataset from 2008 to 2022, we establish a causal relationship between the implementation of these financial reforms and a significant reduction in the urban–rural income gap. The core finding—that pilot counties experienced a statistically and economically meaningful convergence in urban and rural incomes relative to non-pilot counties—withstands an extensive battery of robustness checks, including tests for parallel trends, placebo interventions, policy anticipation effects, and potential endogeneity, the latter addressed through an instrumental variable strategy.

Beyond establishing this aggregate effect, our mechanism analysis delves into the specific pathways through which the policy operates. We identify and empirically validate two complementary transmission channels: enhanced agricultural credit accessibility and the promotion of rural labor transition to non-farm employment. The reforms successfully expanded targeted formal credit to the agricultural sector, relaxing liquidity constraints for farmers and related enterprises. Concurrently, they catalyzed a structural transformation of the local labor market, facilitating the shift of rural labor toward higher-productivity non-agricultural sectors. These validated channels confirm that the policy's impact operates through fundamental improvements in both targeted financial inclusion and rural economic diversification. Furthermore, our analysis on farmers' income resilience indicates that the reforms not only raise income levels but also stabilize them, reducing vulnerability to shocks.

Collectively, the findings underscore that targeted, place-based financial system reforms, which recalibrate institutional incentives and supply-side architectures, can be a potent instrument for mitigating regional inequality and enhancing rural welfare. This offers a mechanism-rich narrative that connects deliberate institutional innovation with inclusive development and resilience outcomes.

5.2 Policy implications

The empirical results yield several concrete implications for policymakers seeking to leverage financial sector development for equitable and resilient growth. First, the successful model of the NFCRPZs should be strategically expanded, but with careful attention to contextual adaptation. Our findings affirm the value of decentralizing regulatory autonomy to allow localized financial innovation tailored to specific regional bottlenecks. Policymakers should consider authorizing new pilot zones, particularly in regions with pronounced urban–rural disparities or high agricultural dependence. The implementation framework should empower local governments to design interventions that address their specific structural constraints, whether related to agricultural value chain financing, rural SME ecosystems, or migrant worker financial services.

Second, policy design must explicitly prioritize and strengthen the agricultural credit accessibility channel. The evidence indicates that directing formal financial resources to the farming sector is a direct and effective pathway to income convergence and stability. This requires moving beyond traditional collateral-based lending. Regulators should encourage pilot zones to accelerate the development of digital financial infrastructure for agriculture, promote the use of alternative data (e.g., transaction flows, satellite imagery) in credit scoring for farmers and agri-businesses, and support the emergence of specialized rural financial intermediaries. Concurrently, policies must actively work to reverse the historical outflow of rural capital by creating incentives, such as differentiated reserve requirements or targeted re-lending facilities, for financial institutions to deepen their engagement in agricultural and rural markets.

Third, financial reforms should be consciously integrated with broader rural industrial and employment development strategies. Given the strong mediating role of non-farm employment, financial policy cannot operate in isolation. Policymakers should foster synergies between financial reform initiatives and programs aimed at rural industrialization, SME development, and human capital investment. For instance, financial products (e.g., start-up loans, equipment financing) could be tailored to support cluster-based small industries, rural tourism, or agro-processing ventures that generate local employment. Credit support for vocational training or skills development programs can also enhance the employability of the rural labor force, facilitating the structural transformation identified in our mechanism tests.

Finally, monitoring and evaluation frameworks for financial reforms must incorporate multidimensional distributional and resilience metrics. While aggregate indicators of financial depth and stability remain important, our study demonstrates that financial policies have significant equity and welfare implications. Therefore, the assessment of reform pilots should systematically track disaggregated outcomes, such as rural vs. urban access to finance, the sectoral allocation of credit (particularly to agriculture), changes in rural employment structure, and county-level income volatility. This will enable evidence-based iterative refinement of policies to maximize their inclusive growth and resilience-building impact.

5.3 Limitations and future research

While this study provides robust evidence on the impact of financial reforms on the urban–rural income gap and income resilience, several limitations should be acknowledged. First, our analysis is conducted at the county level due to data constraints, which may mask heterogeneity within counties and at the household level. Although we identify macro-level transmission channels such as agricultural credit and non-farm employment, we cannot directly observe how individual farmers access credit or adjust their livelihoods. Second, the mediators used in the mechanism analysis are aggregate proxies and may not fully capture the micro-level behavioral responses of rural households. Third, while we address endogeneity through instrumental variables and robustness checks, the exclusion restrictions, though theoretically grounded, may not be fully immune to omitted variable bias. Future research could leverage household-level panel data to examine intra-county distributional effects and explore how digital financial tools specifically affect smallholder decision-making under climatic or market shocks. Additionally, qualitative case studies from pilot zones could enrich our understanding of the institutional and social processes underlying these aggregate outcomes.

Data availability statement

The original contributions presented in the study are included in the article/supplementary material, further inquiries can be directed to the corresponding author.

Author contributions

ZY: Methodology, Visualization, Data curation, Validation, Project administration, Conceptualization, Supervision, Investigation, Resources, Software, Writing – review & editing, Funding acquisition, Writing – original draft, Formal analysis. XL: Supervision, Writing – original draft, Validation, Funding acquisition.

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Conflict of interest

The author(s) declared that this work was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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