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Working Paper No. 3, 2025

Durham Business School working papers series ISSN 1749-3641

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Entrepreneurship, Economy and Inequality: Evidence from China's Return-Home Policy

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October 2025

Abstract

This study examines the economic and distributional effects of China's National Pilot Program for Returnee Entrepreneurship, which encourages rural migrants to return to their hometowns for business creation and employment. Drawing on county-level socioeconomic indicators and nationally representative household survey data, we find that the program substantially boosted local economic development. Yet the gains were uneven as household-level analysis reveals a significant rise in within-county inequality. The mechanism operates through unequal access to capital, skills, and risk tolerance, enabling better-endowed households to capture a disproportionate share of the benefits. These findings underscore a key policy trade-off: while returnee entrepreneurship initiatives can stimulate aggregate growth, they may simultaneously exacerbate disparities within rural communities.

JEL Codes: J61, O15, D63, L26

Keywords: Labor migration, Economic development, Inequality

*Corresponding author: habib.rahman@durham.ac.uk. We thank XXX for sharing the data used in this study. This work additionally benefit from valuable comments by XXX, XXX, XXX, XXX, XXX, XXX, and numerous seminar participants. Any errors in this manuscript are my own. We have no material financial interests related to the research described in this paper and provide all data and code used in this study at www.hrahman.net.

I Introduction

Reducing inequality has become a central policy concern worldwide. The United Nations' Sustainable Development Goals (SDGs), established in 2015, explicitly identify the reduction of inequality as Goal 10, emphasizing the importance of equal opportunities and inclusive growth. Past studies show that persistent income inequality can hinder long-run development by increasing political and social instability Shin (2012). Thus, understanding how region-specific policies affect the distribution of income is a critical question for both researchers and policymakers.

China provides an especially important case for examining this question. Since the reform and opening-up period, the country has experienced unprecedented economic growth, but also a sharp increase in income inequality (Xie and Zhou, 2014; Koh et al., 2020). One important driver has been large-scale rural–urban migration, which has simultaneously supported national growth while draining human capital from rural regions, leaving behind aging populations and disadvantaged households (Antman, 2011; Xie and Zhou, 2014). To counter these trends, the Chinese government has introduced a series of programs to encourage migrants to return to their hometowns and engage in local employment or entrepreneurship. By the end of 2022, over 12.2 million individuals had returned for rural employment or business ventures.[1]These policies aim not only to raise rural income but also to foster local human capital accumulation and rebalance regional development.

In this paper, we examine the impact of China's National Pilot Program for Returnee Entrepreneurship, first introduced in 2016, on household income inequality within counties. The program was rolled out in three phases: the first batch in February 2016, the second in December 2016, and the final batch in October 2017, covering a total of 341 pilot counties.

[1]Ministry of Agriculture and Rural Affairs, 2023.

The policy was designed to create a supportive ecosystem for returnee entrepreneurship by improving local infrastructure and facilities, providing technical assistance, offering financial subsidies, and encouraging the formation of industry clusters. Through these measures, the program sought to attract migrants back to their hometowns and stimulate economic development in rural areas.

Using four waves of the China Household Finance Survey (2013, 2015, 2017, and 2019), we match household data with county-level socioeconomic variables and the list of pilot counties released by the National Development and Reform Commission. To estimate the causal impact from this pilot policy, we employ a staggered difference-in-differences (DID) design, utilizing the estimator introduced by Callaway and Sant'Anna (2021)that exploits the phased rollout of the pilot across counties.

We begin by assessing the overall economic impact of the pilot policy using county-level socioeconomic indicators and find that the program generated clear positive effects on local development. However, a closer examination at the household level reveals that the policy significantly increased within-county income inequality. In particular, compared to non-pilot counties, pilot counties recorded a rise of 0.034 on the Kakwani index. We employ the Kakwani (1984) index of relative deprivation, which is well-suited for our setting as it captures income comparisons within a reference group, offering finer granularity than aggregate measures like the Gini (1921) or Theil (1967). These findings suggest that while the policy succeeded in promoting economic growth, it also unintentionally amplified disparities within treated counties.

A potential concern with our staggered difference-in-differences design is the possibility of bias arising from heterogeneous treatment timing. To address this, we first verify the parallel trends assumption and show that pre-treatment trends are consistent across pilot and non-pilot counties. In addition, given that the selection of counties into the entrepreneurship program followed a hierarchical application and evaluation process based on a recommended list, sample selection bias may be a concern. To mitigate this, we implement propensity score matching (PSM) and find results consistent with our baseline estimates. Finally, we test the robustness of our findings using alternative measures of inequality, replacing the Kakwani index with the Theil and Gini indices. Across all specifications, the results remain robust, reinforcing the validity of our conclusions.

To better understand the mechanisms driving these results, we next examine how the benefits of the entrepreneurship pilot program are distributed across households. Our analysis shows that the program disproportionately favors households with stronger initial endowments, those with greater financial resources, higher educational attainment, and stronger social networks. These households are better positioned to access credit, navigate bureaucratic processes, and bear the risks associated with entrepreneurship, enabling them to capture a larger share of the program's benefits. In contrast, households with limited assets or weaker human capital are less able to participate and benefit, resulting in widening income disparities within treated counties. This heterogeneity underscores that while the policy stimulates economic activity, its distributional consequences depend heavily on pre-existing household characteristics.

This paper makes three major contributions to literature. First, we build on existing studies that highlight the crucial role of rural labor in driving China's development (Wang and Conesa, 2022; Fu-Ning et al., 2013; He and Ye, 2013) and the welfare costs of large-scale rural outmigration, including left-behind children and elderly (Chang et al., 2011; Xie and Zhou, 2014). While prior work has largely focused on the negative consequences of outmigration, we provide new evidence on the consequences of government-led return migration and entrepreneurship policies. In doing so, we extend the broader literature on return migration and human capital accumulation (Dustmann and Kirchkamp, 2002).

Second, we contribute by jointly examining both aggregate and local outcomes of rural entrepreneurship policies. At the aggregate level, the programs increase economic activity and foster rural revitalization. However, at the local level, they widen inequality, as households with better access to capital and networks disproportionately capture the benefits. This dual finding underscores the importance of distinguishing between overall development gains and their uneven distribution across communities.

Third, we advance the literature on inequality and human capital heterogeneity (Andrés et al., 2010; Rougoor and Van Marrewijk, 2015) by uncovering the mechanisms through which inequality arises in treated areas. We show that divergence is not merely from differential program participation, but also from deeper structural barriers, such as credit constraints, education gaps, and differences in risk-bearing capacity. By highlighting these channels, our results illustrate how well-intentioned entrepreneurship policies may unintentionally amplify local disparities unless complemented by targeted interventions.

The remainder of the paper is organized as follows. Section II introduces the policy background. Section III outlines the theoretical framework. Section IV describes the data, measurement, and empirical strategy, including identification issues. Section V presents the results, robustness checks, and potential mechanisms. Section VII concludes.

II The 2016 Program for Returnee Entrepreneurship in China

China's support for rural labor migration and entrepreneurship has evolved substantially since 2008, with successive programs gradually expanding in scale and scope. Early efforts focused primarily on encouraging return migration, but by the mid-2010s policy attention shifted toward enhancing the quality and sustainability of rural entrepreneurship. This shift culminated in the introduction of the National Pilot Program for Returnee Entrepreneurship in 2016 that targeted selected counties with the explicit aim of fostering a more supportive entrepreneurial ecosystem for return migrants.

First, the policy focused on resource integration and service platform development. By investing in returnee entrepreneurship parks and related service infrastructure, local governments were tasked with providing incubator facilities, technical support, and information-sharing platforms. To reduce financing constraints, the program also introduced targeted tax incentives and financial subsidies.

Second, the program promoted the development of rural e-commerce and industry clusters, with a particular emphasis on the "Internet Plus Agriculture" model. By supporting e-commerce platforms for agricultural products and facilitating the formation of local industry clusters, the program aimed to leverage agglomeration economies, expand market access, and strengthen regional competitiveness.

Third, the program sought to link entrepreneurship with broader national priorities, especially poverty alleviation and rural economic transformation. Returnee businesses were expected not only to create employment opportunities for low-income households but also to contribute to the rural revitalization strategy and urban–rural integration.

The pilot was rolled out in three successive batches: 90 counties in February 2016, 116 counties in December 2016, and 135 counties in October 2017, for a total of 341 counties. Despite variation in timing, the core objective remained consistent: to test whether targeted institutional support could reduce the barriers facing returnee entrepreneurs and foster a sustainable rural entrepreneurship ecosystem.

III Conceptual framework

This section develops a conceptual framework to explain how the National Pilot Program for Returnee Entrepreneurship may affect rural income inequality. The policy alters household production decisions through three primary channels: (i) the non-agricultural entrepreneurship effect, (ii) the non-agricultural employment effect, and (iii) the stay-in-town effect. Each channel directly influences household income generation and, in turn, the distribution of income within rural areas.

First, policy incentives lower the barriers for migrant workers to return and establish enterprises in their home counties. Return migrants typically possess higher levels of human capital, financial capital, and social networks than those who remain (Dustmann and Kirchkamp, 2002; Piracha and Vadean, 2010). As a result, they are disproportionately likely to engage in entrepreneurial activities, thereby expanding the number and scope of non-agricultural enterprises.

Second, the expansion of non-agricultural enterprises is expected to stimulate employment opportunities beyond the agricultural sector. This not only facilitates the return migration of workers with heterogeneous skill profiles but also reshapes the local labor market by reallocating labor across sectors. Such reallocation may raise aggregate income levels by increasing labor productivity.

Third, greater entrepreneurial activity and the associated employment creation provide opportunities for local residents who otherwise would have remained in agricultural employment. Empirical evidence suggests that return migration contributes positively to the development of the local non-agricultural economy (Liang and Cheng, 2023). In this sense, the policy has the potential to enhance average household income in rural areas.

While these mechanisms raise overall income levels, they also carry implications for inequality. A large body of research indicates that entrepreneurship can increase regional income disparities through wealth accumulation effects and selective participation (Atems and Shand, 2018; Halvarsson et al., 2018). In particular, the clustering of returnees with better human and financial capital may generate crowding-out effects for disadvantaged groups, such as those with lower educational attainment or older age profiles. Recent evidence shows that returning migrants can reduce the relative income of non-returning rural households (Hu et al., 2023). Consequently, while the better-endowed households benefit disproportionately from entrepreneurial opportunities and expanded non-agricultural employment, less advantaged households may experience declining relative returns in agricultural or informal sectors.

IV Data and Measurement

A Policy Data

We construct the policy data from official releases of the National Development and Reform Commission (NDRC), which provide detailed information on the rollout of the National Pilot Program for Returnee Entrepreneurship. The data includes the list of designated pilot counties, the timing of implementation, and program details. In total, 341 counties were selected. The program was introduced in three waves: the first wave in February 2016 (90 counties), the second in December 2016 (116 counties), and the third in October 2017 (135 counties). This staggered rollout across counties and years forms the basis of our empirical strategy.

B Income Inequality Data

We measure household income inequality using data from the China Rural Household Panel Survey (CRHPS), which integrates two nationally representative surveys: the Chinese Family Database (CFD), maintained by Zhejiang University, and the China Household Finance Survey (CHFS), conducted by the Survey and Research Center for China Household Finance at Southwestern University. The CRHPS covers 29 provinces (autonomous regions and municipalities) and collects information at the individual, household, and village levels on a biennial basis. Since the policy intervention was first introduced in 2016, we use survey waves from 2013, 2015, 2017, and 2019 to capture both pre- and post-policy dynamics. With that, we obtained balanced panel data with 12454 household-level observations.

We employ the Kakwani (1984) index of relative deprivation at the county level to quantify inequality. This index is particularly suitable for our setting, as it captures the comparisons of income within a reference group, offering finer granularity than aggregate measures such as the Gini or Theil indices. Specifically, for a county with n households and income distribution $X = (x_1, x_2, \ldots, x_n)$, sorted in ascending order, the relative deprivation (RD) of household i is defined as:

$$RD_K(x_i, x_j) = \frac{1}{n\mu_X} \sum_{j=i+1}^{n} (x_j - x_i) = \gamma_{x_i}^{+} \left[\frac{\mu_{x_i}^{+} - x_i}{\mu_X} \right] \tag{1}$$

where $\gamma_{x_i}^{+}$ is the proportion of households in X with income above x_i, $\mu_{x_i}^{+}$ is the mean income of households in X with income exceeding x_i, and μ_X is the county mean income. By construction, the index ranges from 0 to 1, with higher values reflecting greater inequality.

C County Level Data

To complement the household survey data, we compile county-level socioeconomic and demographic characteristics from the China County Statistical Yearbook from 2013 to 2019. These data provide a comprehensive picture of local economic development and structural transformation during the study period. Specifically, we collect information on (i) Gross Domestic Product (GDP), (ii) the value added of the secondary industry, (iii) the output value of the tertiary industry, (iv) local government budget expenditure, (v) rural employment rate, and (vi) the number of startup enterprises.

These county-level covariates serve two purposes. First, they allow us to characterize the baseline economic heterogeneity across counties, which is critical for understanding the geographic scope of the policy. Second, these variables provide information on the broader economic and social dynamics at the county level, allowing us to assess the overall impact of the policy on local development beyond household income inequality. To mitigate the influence of extreme outliers, all continuous variables are winsorised at the 1% level.

D Descriptive Statistics

We begin by examining whether there are systematic differences between treated and control counties prior to the introduction of the policy. Table 1 reports the descriptive statistics of county-level variables in 2015. Consistent with the policy's design, treated counties were economically less developed than control counties. Their average GDP, secondary sector value added, tertiary sector value added, and local government budget expenditure are all significantly lower, reflecting the fact that the policy was targeted towards economically lagging regions in order to encourage return migration, stimulate entrepreneurship, and promote local development.

By contrast, the Kakwani index exhibits no statistically significant difference between treated and control counties before the intervention. This suggests that while treated counties were poorer in absolute terms, the distribution of income within them was not markedly different from that of more developed counties. Taken together, these patterns indicate that the policy targeted counties that lagged behind in economic development, but not necessarily in terms of income distribution.

While treated and control counties differ in pre-treatment development levels (as expected given the policy's targeting), our identification relies on parallel trends rather than level balance. Section V explains our identification strategy and provides graphical and regression evidence supporting this assumption.

Table 1: Descriptive Statistics

Variable	Treat=0		Treat=1		Difference
	Obs.	**Mean**	**Obs.**	**Mean**	
Kakwani Index	2583	0.480	576	0.474	0.005
GDP (billion yuan)	2583	38.050	576	22.328	15.722***
Secondary Sector Value Added (billion yuan)	2583	17.747	576	9.224	8.523***
Tertiary Sector Value Added (billion yuan)	2583	18.170	576	8.648	9.522***
Local Government General Budget Expenditure (billion yuan)	2583	6.493	576	4.002	2.491***
Rural Employment Rate	2583	0.565	576	0.603	-0.037***
Number of Startup Enterprises (thousands)	2583	7.715	576	7.316	0.399

Note: This table presents descriptive statistics and balance test results for the year 2015. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

V Identification Strategy

The policy was implemented in three distinct waves: February 2016, December 2016, and October 2017. Our data span the years 2013, 2015, 2017, and 2019. Hence, both 2013 and 2015 serve as pre-treatment periods for all counties. By 2017, counties treated in the first two waves had already been exposed to the policy, while counties treated in October 2017 remained untreated, as we expect most of the 2017 survey to have been conducted prior to implementation. By 2019, all treated counties had received exposure. The staggered timing of implementation allows us to employ a staggered difference-in-differences (DID) framework, leveraging both cross-county and temporal variation to identify the policy's causal effects.

We estimate treatment effects using the estimator proposed by Callaway and Sant'Anna (2021), which is well suited for staggered DID settings. A common approach in the literature is the two-way fixed effects (TWFE) model, but TWFE is known to produce biased estimates when treatment effects are heterogeneous across cohorts or over time. The Callaway–Sant'Anna (CS) estimator overcomes this by computing treatment effects separately for each cohort of counties and each time period, before aggregating them into an overall effect.

Formally, let G_j denote the period in which county j is first treated, with $G_j = \infty$ for never-treated counties. Let t index calendar years. The group-time average treatment effect (ATT) is defined as:

$$ATT(g, t) = E\left[Y_{ijt}(1) - Y_{ijt}(0) \mid G_j = g\right], \quad t \geq g \tag{2}$$

where g refers to the first treatment period of a given group (e.g., February 2016 or December 2016) and t refers to the calendar year in which outcomes are measured (2017 or 2019 in our case). $Y_{ijt}(1)$ and $Y_{ijt}(0)$ denote the potential outcomes for household i in county j in year t with and without treatment, respectively.

VI Results and Discussion

A County-Level Economic Development

First, we examine the overall impact of the policy at the county level to assess whether the program successfully promoted local economic development. Table 2 presents the staggered DID estimates of the policy intervention on key county-level indicators. Across all columns, the results show that the policy had a positive and statistically significant impact. Specifically, counties exposed to the program experienced increases in GDP, sectoral value added, local government expenditure, rural employment, and the number of startup enterprises.

These findings confirm that the policy effectively fostered county-level economic growth and improved the local economic environment. This provides an important starting point before we turn to the household-level analysis in the following sections.

Table 2: County-Level Economic Impact

	Dependent Variables					
	Log (GDP)	Log (Secondary)	Log (Tertiary)	Log (Gov.Exp.)	Rural Employ. Rate	Log (Startup)
	(1)	(2)	(3)	(4)	(5)	(6)
Policy Effect	0.039***	0.031*	0.095***	0.042***	0.008***	0.055***
	(0.010)	(0.016)	(0.011)	(0.009)	(0.002)	(0.015)
Observations	12,545	12,545	12,545	12,545	12,545	12,545

Note: This table presents CS estimator at the county level with various development indicators as the outcome. The robust standard error clustered at the county level is in parentheses. *Significant at 10% level, **significant at 5% level, ***significant at 1% level.

B Household-Level Impacts: Distributional and Inequality Effects

In this section, we zoom into the micro household level and examine how the pilot program affects household-level income inequality. Table 3 reports the results using three different estimators. Column 1 presents the benchmark CS staggered DID estimates, showing that the policy significantly increases within-county income inequality relative to the control counties.

Columns 2 and 3 present additional estimation approaches. Column 2 uses the conventional TWFE estimator, while Column 3 employs the Sun and Abraham (2021) estimator, which

Table 3: The Impact of Pilot Program on Income Inequality within Counties

	Dependent Variable: Kakwani Index		
	Callaway and Sant'Anna (2021)	**Two-way Fixed Effects**	**Sun and Abraham (2021)**
	(1)	(2)	(3)
Policy Effects	0.048***	0.027**	0.031***
	(0.013)	(0.012)	(0.010)
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Observations	12,545	12,545	12,545

Note: This table presents CS estimator using household survey data, with Kakwani index as the outcome. All estimation includes time- and county-fixed effects. The robust standard error clustered at the county level is in parentheses. *Significant at 10% level, **significant at 5% level, ***significant at 1% level.

accounts for heterogeneous treatment effects in staggered adoption settings. Both alternative approaches produce qualitatively similar results, confirming that the policy increases inequality. These findings indicate that, although the pilot program improves overall economic development at the county level as shown in Table 2, it simultaneously widens income disparities within treated counties.

C Robustness Checks

We now proceed with several robustness checks to examine the validity and stability of our findings. First, we assess the parallel trend assumption by testing whether treated and control counties exhibited similar pre-treatment dynamics. As shown in Table 1, the policy was primarily targeted at less developed counties, making it especially important to establish parallel trends in order to validate our causal interpretation.

To do so, we extend the CS estimator into an event-study specification, with the results displayed in Figure 1. The estimates show no evidence of significant pre-treatment differences, supporting the identifying assumption of our staggered-DID framework. Moreover, the post-treatment pattern reveals a widening inequality gap over time, suggesting that the distributional consequences of the policy intervention intensify rather than diminish in the years after implementation.

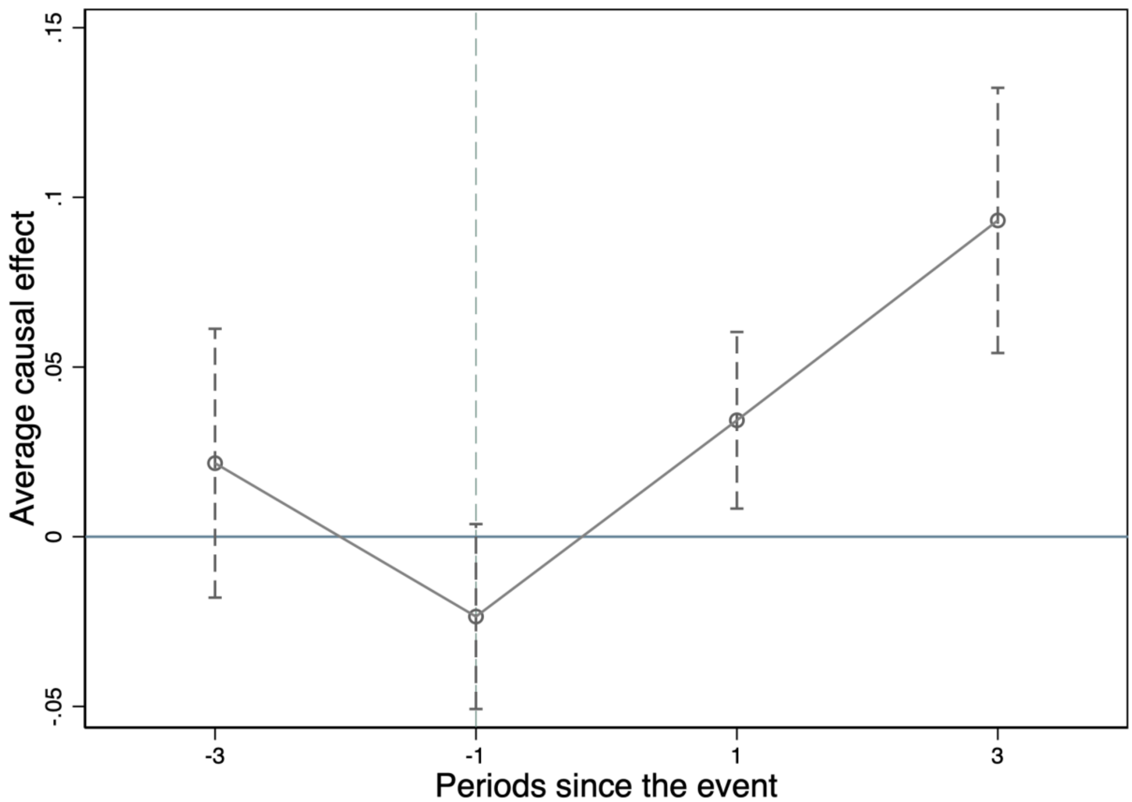

Figure 1: Event-study specification of CS estimator

Next, we address the possibility of sample self-selection in the designation of pilot counties. The selection of counties for the entrepreneurship program among returnees followed a hierarchical application and evaluation process based on a recommended list, which may introduce concerns that treated counties differ systematically from non-treated ones. To mitigate this concern, we implement a kernel propensity score matching (PSM) approach, matching counties according to key developmental variables in 2015 to account for differences in their level of development. Specifically, we match county level variables listed in Table 1 and 2, namely: (i) Gross Domestic Product (GDP), (ii) value added of the secondary industry, (iii) output value of the tertiary industry, (iv) local government budget expenditure, (v) rural employment rate, and (vi) the number of startup enterprises.

Column 1 of Table 4 reports the PSM results, which remain statistically significant and consistent with our baseline estimates. This indicates that the effect is not driven by pre-existing developmental differences between treated and untreated counties. Even after matching counties with similar levels of economic activity, industrial structure, fiscal capacity, and entrepreneurial base, the policy continues to widen inequality.

Lastly, we test the sensitivity of our findings by replacing the Kakwani index with two alternative measures of inequality: the Theil index and the Gini index. The Theil index captures inequality by accounting for disparities across the entire distribution and is particularly sensitive to differences at the tails. The Gini index, one of the most widely used inequality measures, reflects the extent of deviation from perfect equality in the distribution of income. Columns 2 and 3 of Table 4 present the results using these indices, which remain consistent with our baseline findings. This further confirms that our conclusions are not dependent on the specific inequality measure employed. Taken together, these robustness checks reinforce the reliability of our benchmark results.

Table 4: Robustness Checks: PSM and Alternative Outcome Variables

	Dependent Variables		
	Kakwani index	**Theil index**	**Gini index**
	(1)	(2)	(3)
Policy Effect	0.040***	0.078***	0.042***
	(0.013)	(0.005)	(0.003)
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Observations	10,719	12,034	12,034

Note: This table presents CS estimator using household survey data, with Kakwani index, Theil index and Gini index as the outcome. All estimation includes time- and county-fixed effects. The robust standard error clustered at the county level is in parentheses. *Significant at 10% level, **significant at 5% level, ***significant at 1% level.

D Potential Mechanisms

Having established the robustness of our benchmark results, we next examine the underlying mechanisms through which the policy shaped inequality. For this purpose, we return to a traditional TWFE framework and extend it into a triple-difference specification. The main reason is that the CS estimator is designed for settings with staggered adoption and heterogeneous treatment timing, but it is less convenient when analysing interactions with household-level characteristics, as it does not directly accommodate triple-difference structures. In contrast, the TWFE specification provides a straightforward way to introduce and interpret triple interactions, allowing us to identify how household characteristics operate as channels linking the policy to changes in inequality.

Formally, we estimate the following model:

$$Y_{ijt} = \alpha+\mu_j+\lambda_t+\delta H_i+\gamma_1(Treat_j\times Post_t)+\gamma_2(Treat_j\times H_i)+\gamma_3(Post_t\times H_i)+\beta(Treat_j\times Post_t\times H_i)+\varepsilon_{ijt} \tag{3}$$

where Y_{ijt} denotes the outcome variable for household i, in county j, and year t; μ_j and λ_t are county and year fixed effects; H_i represents household-level characteristics; $Treat_j$ is an indicator for treated counties; and $Post_t$ indicates the post-policy period. The coefficients on the pairwise interactions capture differential baselines and heterogeneous policy responses along two dimensions, while the triple interaction coefficient β is our main parameter of interest, identifying whether the policy's effect on inequality operates through a given household characteristic.

To examine these mechanisms, we divide our sample into two groups, those above and those below the county-year median for each characteristic for each year. This approach allows us to measure inequality within a county by comparing relatively better-off households to relatively worse-off households. In other words, this approach captures whether the policy had stronger effects on households in the top half of their county's distribution compared to those in the bottom half in terms of various characteristics. Analysing these allows us to understand how the policy shaped inequality.

The characteristics we examine include income, education, health, and risk tolerance. We first focus on log income as the outcome variable, aiming to identify which groups benefited more from the policy. Table 5 shows that across all four dimensions, the estimated triple-difference coefficients are positive and statistically significant. This indicates that households and individuals with higher income, higher education, better health, and greater willingness to take risks experienced disproportionately larger income gains from the policy.

This pattern suggests that the policy amplified pre-existing advantages, favoring households that were already better positioned to seize new opportunities. Greater wealth provided the resources needed for investment while higher education enhanced the ability to recognize and adapt to emerging opportunities. Similarly, better health reduced barriers to labor market participation and households with higher risk tolerance were more inclined to reallocate resources in response to the policy. Taken together, the evidence points to a common mechanism where the policy disproportionately rewarded households with stronger initial endowments in terms of financial, human, or behavioral, which reinforcing uneven income gains.

Table 5: Mechanisms of Policy Impact on Household Income

	Dependent Variable: Log Income			
	Income	**Education**	**Health**	**Risk Tolerance**
	(1)	(2)	(3)	(4)
Policy × Post × Characteristic	0.117*	0.296***	0.221***	0.301***
	(0.061)	(0.110)	(0.068)	(0.067)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	12,545	12,545	12,545	12,545
R^2	0.636	0.212	0.192	0.189

Note: This table presents CS estimator using household survey data, with log income as the outcome. All estimation includes time- and county-fixed effects. The robust standard error clustered at the county level is in parentheses. *Significant at 10% level, **significant at 5% level, ***significant at 1% level.

Next, we shift our outcome back to the Kakwani index to examine how the policy affected the distribution of income gains within each group. The coefficients across all columns in

Table 6 are negative, which indicates that after the policy, households in the top 50% of each category experienced less inequality in income gains relative to those in the bottom 50%. In other words, the distribution of benefits was more even within the upper half of each category compared to the lower half. However, the effect is only statistically significant for health and risk-tolerance, suggesting that differences in health status and risk preferences may play an important role in shaping inequality. In short, the policy appears to reduce inequality within the top halves of the groups, but the effect is not consistent across all dimensions.

Overall, our findings show a clear overall benefit from the policy, with aggregate income rising as reported in Table 2. However, analyses in this section show that the distribution of these gains was uneven. Households at the upper end of the spectrum captured a larger share of the benefits, while those at the lower end saw modest improvements. This pattern is consistent with the nature of the policy itself, which encouraged households to return and pursue entrepreneurial activities. Such opportunities naturally favored individuals with greater financial resources, stronger human capital, and the capacity to take on risk. As a result, while the policy succeeded in boosting average development, it also reinforced pre-existing inequalities by disproportionately rewarding better-endowed households.

Table 6: Mechanisms of Policy Impact on Household Inequality

	Dependent Variable: Kakwani Index			
	Income	**Education**	**Health**	**Risk Tolerance**
	(1)	(2)	(3)	(4)
Policy × Post × Characteristic	-0.016	-0.035	-0.065***	-0.064***
	(0.018)	(0.028)	(0.019)	(0.021)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	12,545	12,545	12,545	12,545
R^2	0.669	0.098	0.077	0.072

Note: This table presents CS estimator using household survey data, with Kakwani index as the outcome. All estimation includes time- and county-fixed effects. The robust standard error clustered at the county level is in parentheses. *Significant at 10% level, **significant at 5% level, ***significant at 1% level.

VII Conclusion

In this paper, we examined the distributional effects of the National Pilot Program for Returnee Entrepreneurship implemented in 2016, a large-scale initiative designed to stimulate rural economic development by encouraging return migration and entrepreneurial activity. Using a staggered difference-in-differences strategy combined with household inequality measures, we documented two central findings. First, the policy generated clear aggregate developmental gains across treated counties, reflecting its effectiveness in expanding economic opportunities. Second, the distribution of these gains was highly uneven. Households with stronger initial endowments such as higher education, better health, greater risk tolerance, or access to capital, were disproportionately able to capitalize on the initiatives provided by the policy, while more disadvantaged households benefited less consistently.

These heterogeneous results are logically consistent with the mechanism of policy design, where initiatives centered on entrepreneurship will favor individuals with the financial and human capital required to undertake risks and invest in new ventures. As a result, while the policy succeeded in raising average incomes, it simultaneously worsens the inequality at the lower end of the distribution, even as it reduced disparities among the higher-income groups.

Our findings highlight an important trade-off in policy design. Efforts to promote rural development and economic mobility through entrepreneurship can enhance overall welfare, but it may also risk widening inequality unless complemented by measures that improve access to resources, credit, and capacity-building. Future research should explore potential complementary policies that can help ensure these benefits are distributed more evenly across households with different income levels.

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