Leave or Stay: Inequality of the Poverty Alleviation Resettlement Program

Dingwei Guo*, Yuan Zhao†, and Xiyan Li‡ §

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Abstract

This paper examines how large-scale poverty alleviation resettlement unevenly affects household labor allocation and income in rural China. Focusing on the Poverty Alleviation Resettlement Program (PARP), which relocates poor households from remote villages to peri-urban communities, we outline a framework with two competing mechanisms: a last-mile channel that lowers migration costs and encourages urban work, and a home-attachment channel that improves amenities and raises the utility and cost of staying home. Using panel data from a study county in Shaanxi (2014–2022) and a difference-in-differences design, we find that PARP raises total income and migrant wages by shifting labor from farming to urban employment. Yet gains are highly unequal. Labor-abundant households benefit substantially, while labor-constrained households face stagnant or declining earnings as higher living costs limit migration. These findings show that resettlement can unintentionally widen inequality, underscoring the role of household labor endowments in shaping program impacts.

Keywords: temporary work migration, quality of life, natural experiment

JEL Codes: J61, Q15, R52

*Jinhe Center for Economic Research, Xi'an Jiaotong University, Xi'an, China. Email: dwguo123@outlook.com

†Corresponding author. Jinhe Center for Economic Research, Xi'an Jiaotong University, Xi'an, China. Email: zhaoyuan@xjtu.edu.cn. Address: Jinhe Center for Economic Research, Xi'an Jiaotong University. Xian'ning West Road 28, Xi'an, Shaanxi, China, 710049.

‡China Center for Energy Economics Research, School of Economics, Xiamen University, Xiamen, China. Email: lixiyan2023@163.com

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1 Introduction

The urban–rural gap in developing countries is marked by stark disparities in income and access to amenities, trapping rural households in intergenerational poverty through disadvantaged neighborhoods, limited education, and scarce job opportunities. Rural families rely heavily on temporary or seasonal migration as an important income-generating strategy. They typically send household members to cities during agricultural slack seasons to work in labor-intensive sectors. This feature arises because permanent relocation is constrained by high costs, institutional barriers, and credit constraints that limit the permanent reallocation of labor to urban sectors (Chang et al., 2011; de Janvry et al., 2015; Cai, 2020; Fried and Lagakos, 2021; Jia et al., 2023).

Given this working feature, in 2016, China launched the nationwide Poverty Alleviation Resettlement Program (PARP) as a key component of its Anti-Poverty Campaign. The program targets impoverished rural households, aiming to help them escape resource constraints and disadvantaged environments. Participants receive free, newly built housing and one-time relocation compensation. Resettlement sites are strategically located between urban centers and original villages, providing better access to electricity, clean water, education, and healthcare. Their proximity to cities and improved transportation networks also facilitate seasonal migration and urban employment, increasing household income and breaking the intergenerational transmission of poverty through enhanced neighborhood effects.

While several studies have evaluated the impacts of PARP on resettled households, their findings point to substantial heterogeneity and raise concerns about the external validity of PARP effect estimates. For example, Zhang et al. (2023a) studied resettlement in Xin County of China, and documented a significant increase in non-farming employment, attributing the effect to reduced switching costs to urban sectors. In contrast, Qiu et al. (2024) examined 98 resettled villages across China and found a decline in migrant labor participation, emphasizing the program's benefits for rural left-behind family members. Motivated by these contradictory findings, this paper revisits how PARP reshapes household labor allocation between farming and migrant work. We argue that these divergent responses in the literature arise from two distinct yet interrelated mechanisms, which in turn induce unequal impacts of the program across households with different labor characteristics.

Specifically, we develop a conceptual framework that integrates two potential channels, the *last-*

mile channel and the *home-attachment channel*, to uncover how PARP induces heterogeneous labor responses and implicit inequality. The last-mile channel captures the reduction in commuting and migration costs when households are relocated closer to urban centers, which promotes temporary migration. This mechanism resembles the effects observed in studies where policy interventions offset migration expenses (Bryan et al., 2014; Hidrobo et al., 2022; Zhang et al., 2023a). In contrast, the home-attachment channel reflects the role of improved living conditions in strengthening households' attachment to resettlement sites. Enhanced housing, utilities, and community amenities can reduce the incentives for migration through three potential pathways:

First, improved amenities in resettlement areas increase the attractiveness of staying home, weakening the push factors that drive households to send migrant workers and leading to greater labor retention in local farming. Past research supports this view by proposing a cost-benefit framework of household migration decisions that incorporates amenity values (Qiu et al., 2024; Chang et al., 2011; Diamond, 2016). Second, improved living conditions raise household expenses, such as rising electricity and water bills, which may incentivize migration to generate additional income in order to offset the increased living costs (Juhn and Potter, 2006; Perez-Arce and Prados, 2021). Third, these higher living costs may on the other hand constrain households' financial capacity to cover migration-related expenses, which disrupts the positive effects of reduced migration costs brought by the last-mile channel (Abramitzky, 2009; Beegle et al., 2011).

The interaction between the two channels produces unequal impacts on the working activities and income patterns of resettled households with different labor endowments. Specifically, lower migration costs encourage migrant work in a homogeneous form across households. However, the home-attachment channel varies by household labor characteristics. By excluding competing pathways of self-financing and social networks, we argue that improved living conditions exert stronger negative effects on migrant work among households with less family labor as the crowding-out effect of higher living costs on funds available for migration becomes more pronounced.

Under this framework, we use a sample of rural households from a county[1] of Shaanxi Province in China covering the year 2014 to 2022 to estimate the treatment effect of PARP, with resettlement occurring in 2018 within the study county. Our sample covers all the 328 impoverished villages in the study county. We exploit a difference-in-differences (DID) design to examine how resettlement

[1]Per data provider's requirement, we cannot share detailed geographic information of the treatment place.

affects the structure of household income and family labor allocation. Our results show that after resettlement, households experience a 12% increase in total income per capita, primarily driven by a 37% rise in urban wage income per capita from migrant work.

Consistent with the income change, households on average reallocate more family labor toward urban sectors, with resettlement inducing an additional 0.78 months of migrant work per capita within a year. Additionally, we examine whether resettled households change their choice of migrant work destinations. We find that the average distance to the destination province increases by 11%, driven by a shift of migrant work province toward more developed regions where the GDP per capita is about 10% higher.

The estimated treatment effects highlight the dominance of the positive effects of PARP on labor transitions toward urban sectors. To further examine how family labor characteristics shape the distinct effects of the two channels, we estimate the heterogeneous effects across households with different labor characteristics. The results indicate that after resettlement, migrant work activities increase more for households with a higher family labor share. A 1% increase in family labor share leads to an additional 1.49% rise in urban wage income and 0.02 more months of urban migrant work per year. Furthermore, households with a family labor share below approximately 40% experience declines in both wage income and migrant work duration, which is consistent with the predictions of the conceptualization framework and suggests stronger and more dominant negative effects from the improved living conditions among households with limited labor force.

This research contributes to three strands of literature. First, it relates to studies on liquidity constraints and migration. Existing work shows that reducing migration costs or providing small subsidies affects labor allocation and welfare, often in experimental settings with limited variation (Ludwig et al., 2013; Bryan et al., 2014; Chetty et al., 2016; Morten, 2019; Imbert and Papp, 2020; Cai, 2020; Combes et al., 2020; Piyapromdee, 2021; Fried and Lagakos, 2021; Nakamura et al., 2022; Hidrobo et al., 2022; Zhang et al., 2023a; Lagakos et al., 2023). We extend this literature by showing that infrastructure-based interventions not only reduce migration costs but also raise living costs, thereby shaping household decisions. Importantly, we link migration responses to labor endowments and show that financing frictions can mute or reverse gains even when physical barriers are reduced, with direct policy implications.

Second, this research contributes to the literature on the unintended distributional consequences

of poverty alleviation. While prior studies document inequality and spillovers in migration programs, including Moving to Opportunity (Ludwig et al., 2013; Bryan et al., 2014; Chetty et al., 2016; Duflo, 2017; Breza and Kinnan, 2021; Egger et al., 2022; Hidrobo et al., 2022; Altındağ and O'Connell, 2023; Filmer et al., 2023; Muralidharan et al., 2023; Banerjee et al., 2024), these effects are often under-emphasized. We contribute to the literature on program evaluation by showing how policy design can generate divergence within the poor. This is a subtle but important point that resettlement systematically disadvantages certain groups. That adds a policy design critique missing from most migration studies which usually emphasize average effects.

The final related strand concerns the role of land in rural areas in developing countries (Wu et al., 2017; Noack et al., 2018; Mueller, 2022; Rathi, 2022; Noack and Costello, 2024). We formalize how reliance on farming income creates a paradox: it stabilizes survival but also slows down structural transformation. By showing that resettlement shifts households closer to markets but leaves this financing role intact, we contribute to debates on why rural transformation is sticky even with big policy pushes given the roles of land over rural areas.

The structure of this paper is as follows. In Section 2, we introduce PARP and the working patterns in rural China to motivate the pathways of PARP effects. Section 3 proposes the conceptualization framework of family labor allocation decisions to implicate empirical strategies. Section 4 presents data collection, descriptive statistics, and empirical strategy. Section 5 shows the empirical results. Section 6 concludes.

2 PARP and Background

2.1 PARP in China

The Poverty Alleviation Resettlement Program (PARP) in China was initiated in 2010 and unfolded in two stages. The first stage (2010–2015) consisted of small-scale, decentralized relocation efforts led by local governments, primarily targeting households exposed to environmental risks such as natural disasters, and remained fragmented and limited in scale. In 2016, the central government launched a nationwide PARP as part of the Anti-Poverty Campaign, marking a shift to a centrally coordinated policy with standardized eligibility, unified funding, and systematic implementation. Local governments were responsible for execution, typically providing subsidized or free housing in

newly constructed resettlement sites along with one-time relocation subsidies. By 2020, nearly 10 million people had been resettled, making it one of the largest state-led resettlement programs in history.

Our study focuses on Shaanxi Province in central China, where resettlement programs also evolved in two stages. In the first stage (2010–2015), local governments implemented two major initiatives concentrated in the northern and southern regions, primarily relocating households exposed to natural disaster risks. Starting in 2016, resettlement was scaled up to a province-wide program covering multiple counties and targeting impoverished households with poor living conditions and limited employment opportunities. Implementation was organized at the county level, with households relocated within their original counties to newly constructed resettlement sites.

Due to data access constraints, our analysis focuses on a single county. This county is one of the largest resettlement areas in Shaanxi, accounting for about 35% of all resettled households after 2016, and is thus broadly representative of the large-scale program. Located in central Shaanxi, it differs from the first-stage initiatives concentrated in the northern and southern regions, which primarily targeted households exposed to natural disaster risks.

2.2 Timeline and Implementation

PARP in this study county was planned and initiated in late 2017. The selection process for participation began with identifying impoverished households registered in an official listing. In 2014, the threshold for qualification into this listing was set at a net annual per capita income of 2,736 CNY (Chinese Yuan). To determine the eligibility of PARP among households in this listing, in late 2017, the local government assessed the overall living conditions and specific characteristics of these households. Priority was given to households in disadvantaged neighborhoods and those with particularly poor living conditions. Following this evaluation, a list of eligible households was publicly released, and households were informed of their eligibility. Each eligible household was given the option to participate in the program. Finally, 1,069 households were deemed eligible at the end of 2017, and 1,020 of them were resettled through PARP in 2018. The details of this selection process are summarized in Figure 1.

Resettled households received a free house and a one-time compensation payment, typically covering relocation costs exceeding 2,000 CNY. While they could live in or rent out the new property,

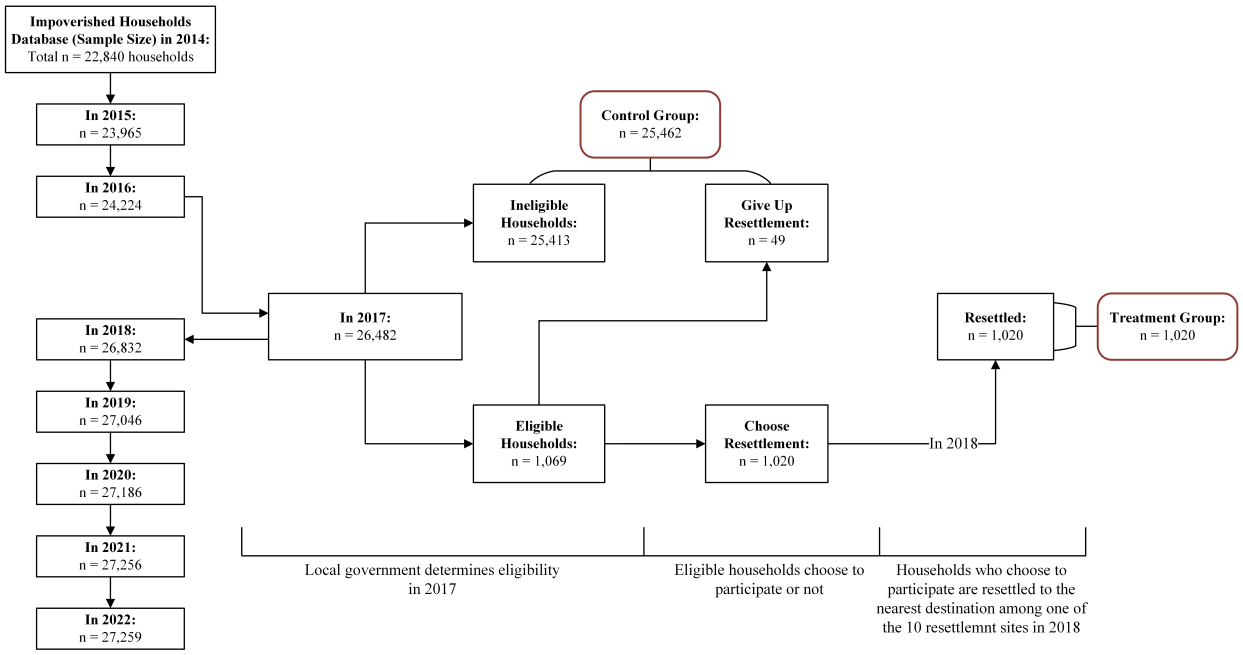


Figure 1: Selection into resettlement

Note: The left-hand flow reports the total number of households recorded in the database in each year. We use all households in the database for the main analysis; the annual increase reflects newly identified impoverished households whose characteristics were subsequently recorded. The right-hand flow shows the formation of the treatment and control groups following the 2017 eligibility assessment. All 1,069 eligible households were already recorded in 2014; thus, households added after 2014 belong to the control group.

resale was prohibited, and their original homes were reclaimed by the government. Due to capacity and cost considerations, 1,020 households were relocated to 10 sites within the county, with each household assigned to the nearest site relative to its original village. Figure 2 shows household locations before and after resettlement and their proximity to major transport networks. Most households were relocated from mountainous areas with limited access to transportation, especially railways, which are critical for migrant labor (Zhang et al., 2023b).

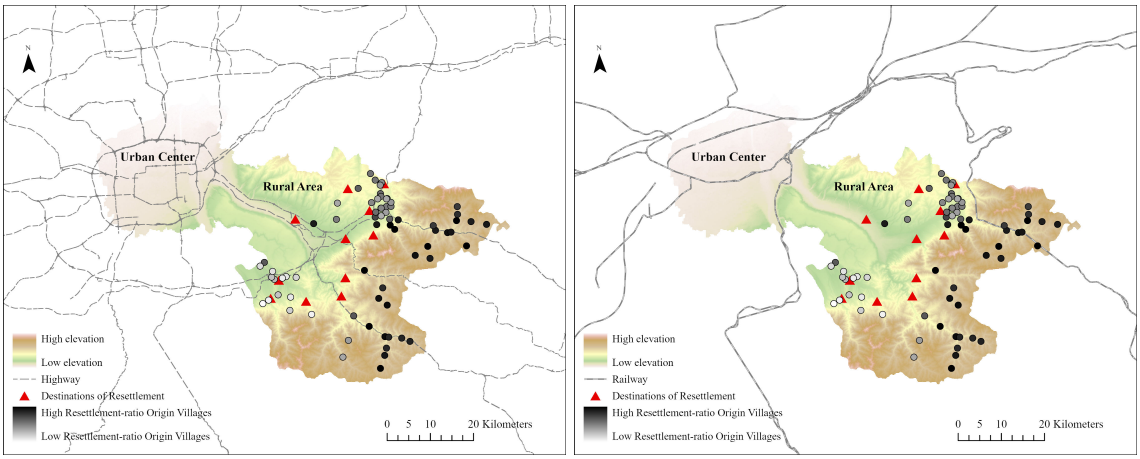


Figure 2: Distribution of resettlement locations

Note: This figure presents the distribution of origin villages for resettled households and their resettlement destinations, together with surrounding transportation networks. The two subfigures present the highways and railways, respectively.

2.3 Selection into Resettlement

Based on the process outlined in Figure 1, we define the control group as consisting of ineligible households and eligible but non-resettled households. The treatment group includes only households that were actually resettled. This definition allows us to identify the effect of resettlement by comparing households that relocated with those that did not.

Although our sample allows us to distinguish households across groups, the selection process may introduce biases that challenge identification. First, eligibility is determined by factors such as household labor endowments and living conditions, implying systematic differences between eligible and ineligible households. Second, conditional on eligibility, households may self-select into resettlement, such that those with more favorable expectations of PARP benefits are more likely to be resettled.

The ideal approach to identifying the treatment effects would require a full understanding of the eligibility criteria and the determinants of selection into resettlement. However, such information is not fully available in our setting. To address these concerns, we implement several strategies to mitigate potential biases and provide further discussion in Section 4.3.

2.4 Farmland Tenure and Temporary Migration in China

Rural households in China typically choose to allocate the family labor into both farming and non-farming sectors. They make labor-allocation decisions under a land-tenure system that differs substantially from private land ownership. Rural land is collectively owned, and households do not legally own either farmland or homesteads. Instead, they hold land-use rights allocated by village collectives under local administrative arrangements. In practice, farmland allocation often follows locally egalitarian principles, with household size and village-level land availability serving as key determinants of the amount of land assigned to each household.

Under PARP, households that move away from their original villages may experience adjustments to, or the reclamation of, their previous land-use rights by local authorities. They may also be assigned use rights to farmland near the resettlement sites. However, because these allocations remain subject to local land constraints and broadly egalitarian allocation rules, resettlement is unlikely to generate a substantial or systematic increase in households' access to cultivated land. PARP therefore differs from land-redistribution resettlement programs in other countries that explicitly expand beneficiaries' access to agricultural land. For example, resettlement programs in Malawi relocated households to areas with greater farmland availability and provided support for land acquisition and agricultural development (Mueller et al., 2014; Mendola and Simtowe, 2015). Such programs may increase the expected returns to farming by expanding cultivated landholdings and strengthening households' agricultural production capacity.[2]

Rural land regulations also create a high opportunity cost of permanent migration. Farmland left idle for more than three years may be reclaimed and redistributed, which reinforces temporary and seasonal labor mobility to work in cities as an important income-generation strategy, since the earnings from non-farming sectors are generally higher than that from farming (Meng,

[2]We also provide empirical evidence to rule out substantial changes in access to farmland after resettlement in Section 5.1 and Appendix Table C1.

2012; Adamopoulos et al., 2022; Cheng et al., 2022; Yang and Loopmans, 2023). Migrant workers tend to concentrate in cities that offer employment opportunities, particularly in manufacturing, construction, catering, and residential services.[3] These jobs rely primarily on physical ability and basic work discipline, instead of advanced formal education or occupation-specific skills (Fu and Gabriel, 2012; Meng et al., 2013; Wang and Chen, 2019). These workers typically live in cities for months and return home during the farming season or at the end of the year (Eggleston et al., 2018; Combes et al., 2020; Li et al., 2024). As indicated in Table 1, the total duration of yearly migrant work within a family in our sample is around four months per year. Such migrant labor plays a significant role in household livelihoods, contributing approximately 60% of total household income.

3 Conceptual Framework

3.1 Empirical Findings from the Literature

The PARP literature has primarily focused on the program's effects on migrant work, which has become one of the most important income-generating activities for rural households. In our sample, the share of migrant-work income in total household income increased from approximately 30% in 2014 to nearly 60% in 2022. The existing literature offers two contrasting views on how resettlement affects households' decisions to send migrant workers. On the one hand, Zhang et al. (2023a) show that relocation promotes migration by reducing geographic isolation and improving access to transportation networks and urban labor markets. This lowers migration costs and facilitates non-farm employment, making households more likely to send workers. This mechanism aligns with a broad literature showing that reduced migration frictions increase labor mobility (Ludwig et al., 2013; Bryan et al., 2014; Chetty et al., 2016; Morten, 2019; Imbert and Papp, 2020; Cai, 2020).

On the other hand, Qiu et al. (2024) find that resettlement reduces migrant work activities, particularly among households with dependents. They attribute this finding to improved living conditions, as resettled households receive newly built housing and better access to local public services, increasing the value of staying at home and weakening migration incentives. This channel is consistent with related studies showing that improved local conditions can reduce out-migration

[3]Data according to the national migrant-worker reports published by the National Bureau of Statistics of China.

(Imbert and Papp, 2020; Lagakos et al., 2023; Baseler et al., 2025).

We do not interpret these findings as contradictory. We instead view them as capturing two mechanisms embedded in the same policy, whose relative importance may depend on the specific settings of the program. In environments where relocation substantially lowers migration costs, the mobility channel is likely to dominate, leading to increased migrant work activities. In contrast, when relocation mainly improves local living conditions, the home-attachment channel may become more salient and reduce migration.

3.2 The Two Channels

Our paper builds on and reconciles these two perspectives by proposing a unified conceptual framework in which PARP simultaneously affects household labor allocation through two interacting channels, and where the relative strength of these channels depends on household constraints.

First, we argue that resettlement reduces migration barriers (hereafter the *last-mile channel*). By relocating households closer to transportation networks and urban labor markets, PARP lowers both monetary and time costs of accessing non-farm employment, such as commuting costs. As a result, households face a higher effective return to migrant work. As discussed in Section 2.4, migrant work in China generally does not require advanced formal education or occupation-specific skills. Accordingly, the two related papers (Zhang et al., 2023a; Qiu et al., 2024) and our study abstract from the costs of occupation-specific skill acquisition and instead focus on migration-related barriers.[4] In the absence of other constraints, this channel predicts an increase in labor reallocation toward urban sectors, higher participation in migrant work, and rising wage income, consistent with the mechanism emphasized by Zhang et al. (2023a).

Second, resettlement also operates through the *home-attachment channel* by fundamentally changing local living conditions. Newly constructed housing, together with improved access to water, electricity, education, and healthcare, raises the quality of life in resettlement sites. This affects household decisions through two conceptually distinct pathways. The most direct pathway is that better amenities increase the utility of staying at home, thereby weakening the incentive to send family members away for migrant work, as emphasized by Qiu et al. (2024).

[4]We provide empirical evidence to support this argument by showing that migrant work activity in our sample is not significantly associated with household education levels. See Section 5.2 and Appendix Table C2 for details.

In addition, we extend this channel by highlighting the role of household budget constraints strengthened by higher living costs. Improved housing and local services are often accompanied by higher recurring expenses, such as utility payments and daily consumption costs, which increase the household's financial burden. This generates two opposing forces. On the one hand, because migration typically requires upfront expenditures (Bryan et al., 2014; Zhang et al., 2023a), including transportation and initial living costs in destination cities, higher local expenses may tighten liquidity constraints and reduce the resources available to finance migration. On the other hand, higher living costs may increase the need for cash income and thus strengthen incentives to send migrant workers since the returns to non-farm migrant work are generally much higher than the earnings from farm sectors (Chen et al., 2019a,b).

Which force dominates depends on the extent of household liquidity constraints. For households facing binding liquidity constraints, even if higher living costs increase the need to generate additional income, they may be unable to respond by sending migrant workers due to the upfront costs required for migration. In this case, the incentive force is effectively muted, and the home-attachment channel operates primarily through increased utility of staying at home and tightened budget constraints, thereby reducing migration.

By contrast, for households that are less financially constrained, both forces are effective, as these households retain the capacity to finance migration, allowing them to respond to increased income needs by reallocating labor toward higher-return migrant work activities. As a result, the income incentive can offset, or even dominate, the forces discouraging migration.

3.3 The Role of Labor Endowments and Unequal Effects

Following the argument above, resettlement is likely to generate heterogeneous effects across households facing different constraint conditions. In our setting, we argue that these constraints are fundamentally linked to household labor endowments.

To understand why labor endowment plays a central role, it is useful to examine how migration decisions are made. Migration is inherently a labor allocation decision. Sending migrant workers requires reallocating labor away from home production and household responsibilities. At the same time, migration also involves upfront costs (Bryan et al., 2014; Hidrobo et al., 2022), which are often financed through income generated at home, such as agricultural production (Rana and Qaim,

2024).[5] [6]

As a result, both the ability to spare labor for migration and the ability to finance migration are closely linked to the labor endowments within the household, since labor-rich households are more capable of generating income at home, particularly through agricultural production which in rural China remains highly dependent on household labor endowments (Xu et al., 2025; Yang and Ji, 2026). This makes labor endowment a natural and central dimension along which the two channels interact.

Under this framework, households with different labor endowments respond differently to resettlement. For labor-scarce households, constraints are more likely to bind, so the home-attachment channel dominates. Higher utility of staying and tighter liquidity constraints limit their ability to respond to increased income needs, making migrant work activities less likely to rise and possibly decline.

In contrast, for labor-rich households, constraints are less binding and income pressure from higher living costs becomes more important. These households can reallocate labor toward migrant work, so the income incentive can offset or even dominate the opposing forces from home attachment and liquidity constraints. As a result, migrant work activities are more likely to increase among labor-rich households after resettlement. We formalize this mechanism in Appendix B, building on the model developed by Mckenzie and Rapoport (2007) and Qiu et al. (2024), and incorporating key elements from Cai (2020).

This discussion leads to three empirical predictions, which guide the empirical analysis in the following sections:

Because the last-mile and home-attachment channels have opposite implications for migrant work, the average effect of PARP on migrant work is theoretically ambiguous. If the last-mile channel dominates, reduced migration costs should increase migrant work activities. If the home-attachment channel dominates, improved local living conditions and tighter liquidity constraints should reduce migrant work activities. Therefore, the average effect depends on which channel dominates in the full sample.

[5] We do not exclude the possibility that migration may also be financed through informal credit or social networks (Phan, 2012; Manchin and Orazbayev, 2018). However, for impoverished households, these alternative financing channels may be limited due to constrained borrowing capacity and weaker network support. We provide further discussion and robustness checks in Section 5.2.

[6] We also appreciate the anonymous referees for this discussion.

Prediction 1 *The average effects on migrant work depend on the dominant channel.*

Prediction 1 summarizes the potential average or net effects of PARP among the full sample. The relative importance of the two channels should vary with household labor endowments. Labor-abundant households are better able to absorb increased living costs and overcome liquidity constraints, so the last-mile channel is more likely to dominate for these households. We therefore expect larger gains in migrant work activities as household labor endowments increase.

Prediction 2 *The effects on migrant work activities are expected to increase with household labor endowments.*

To examine Prediction 2, we use the household labor-force share as the main measure of household labor endowments. For robustness, we also consider the share of family members without severe or chronic illness and the share of children and elderly members as alternative measures in the empirical analysis.

A related implication concerns farming income. Since agricultural production provides a relatively stable source of subsistence income and liquidity for financing migration, households may need to maintain a basic level of farming income even after resettlement. At the same time, because the returns to labor are generally lower in farming than in migrant work (in Table 1, three months of migrant work per year contribute to 60% of total household income), and because access to farmland remains constrained by local land allocation arrangements, households are unlikely to substantially increase agricultural input or expand farming income. This leads to the following prediction:

Prediction 3 *Farming income is expected to remain relatively stable after resettlement.*

This framework suggests that the conflicting findings in the literature reflect different realizations of the same policy. PARP can both promote and discourage migration, depending on which channel dominates at the household level, leading to heterogeneous and potentially unequal effects. In our study county, resettlement typically moved households from geographically isolated mountainous villages to sites with substantially better access to transport networks and peri-urban labor markets as in Figure 2, making the last-mile channel particularly salient on average. At the same

time, because beneficiaries were also highly disadvantaged households with limited labor endowments, improved housing and higher recurring living costs could tighten liquidity constraints for some families, making the home-attachment channel more likely to dominate among labor-scarce households. This county therefore provides a setting in which both mechanisms are simultaneously relevant, allowing us to explain not only average gains but also why those gains are distributed unequally.

4 Data and Empirical Strategy

4.1 Data Collection

We use data on officially registered impoverished households. This database, established under the Anti-Poverty Campaign in 2014, records detailed demographic and socioeconomic information collected through annual household surveys by local governments. It is central to our analysis, as PARP eligibility is determined based on this registry. The dataset has also been widely used in studies of China's anti-poverty policies (Zhang et al., 2023a; Qiu et al., 2024).

Several key features of the database merit attention. First, the classification of impoverished households is primarily based on per capita income, supplemented by indicators such as asset ownership. Households below this threshold qualify for poverty-related assistance. Second, once a household is added to the registry, its information continues to be collected even if it no longer meets the poverty standard in subsequent years. Due to data access regulations, we use observations from 2014 to 2022 in this study.

To estimate the treatment effect, we only use data on households residing in the study county in our analysis, covering all the approximately 26,000 households in each year in the database. The dynamics of sample size by years are shown in Figure 1. We obtained lists of the 1,069 eligible households and the 1,020 resettled households from the local government and matched them to the database of impoverished households using unique government-issued ID numbers. Because PARP eligibility is determined from within this administrative registry, both eligible and resettled households are subsets of households already contained in the database. As a result, this matching step only serves to identify treatment status within the existing sample. We also generate distance-related variables of interest by leveraging household addresses and an API from online map services.

A concern with this large database is missing observations for some demographic variables, likely due to inconsistent data collection across local governments and years. We retain all available observations, so the estimation sample varies across specifications. This does not threaten identification as long as missingness is not systematically related to treatment or outcomes. As discussed in Section 5.4, missing rates are low, with no systematic differences between full and reduced samples, and results remain robust when restricting to complete cases.

4.2 Descriptive Statistics and Variables

The selection of households into PARP participation may generate substantial imbalance between the treatment and control groups. In this section, we examine the selection results by comparing key pre-treatment characteristics across the two groups. Table 1 reports descriptive statistics for the period 2014–2017, corresponding to the pre-treatment years. In addition, Appendix Table A1 reports descriptive statistics for 2017, the year in which eligibility was determined, to address concerns that the pooled statistics in Table 1 may obscure underlying time trends, such as the upward trend in income variables in years.

The demographic variables are grouped into five main categories. We conduct group-wise mean comparisons using t-tests. Specifically, Column (4) reports the t-statistics from the two-sample t-test by comparing resettled households (treatment group) with all unresettled households (control group) for each variable, while Column (5) reports the t-statistics by comparing resettled households with eligible but not resettled households.

As shown in Table 1, Column (4) indicates that most variables differ significantly between the treatment and control groups. Prior to resettlement, treatment households were generally more disadvantaged, exhibiting lower income levels, a greater reliance on government assistance, fewer working-age members, and poorer access to basic services. These patterns are consistent with the targeting criteria of PARP, which prioritizes the most disadvantaged households, but they may raise concerns about the validity of treatment effect identification.

By contrast, Column (5) shows that, among eligible households, we do not observe significant differences in demographic characteristics between resettled and eligible but non-resettled households, likely due to the very high take-up rate of the program.

Table 1: Descriptive statistics (2014 to 2017 average)

Variables	Resettled households (Treatment group) (1)	Eligible but not resettled (Control group) (2)	Ineligible (3)	Resettled vs. control group t-statistics (4)	Resettled vs. eligible not resettled t-statistics (5)
Income per capita (outcome variables)					
Total income	4210.35	4175.20	4650.82	-8.21***	0.11
	(2800.07)	(4200.00)	(1801.25)		
Wage income	2185.44	2140.11	2820.61	-12.90***	0.10
	(2512.00)	(3921.81)	(1702.16)		
Farming income	565.10	552.62	610.31	-1.62	0.07
	(851.39)	(1311.07)	(601.92)		
Transfer income	1505.61	1540.89	1250.40	4.92***	-0.15
	(1951.39)	(2912.04)	(1406.15)		
Working activities (outcome variables)					
Average migrant work (months)	2.85	2.78	4.05	-18.74***	0.14
	(3.10)	(4.40)	(2.10)		
Farming expenditure per capita	125.60	126.80	120.30	0.95	-0.05
	(210.00)	(300.00)	(150.00)		
Household (PSM covariates)					
Arable land per capita (km^2)	0.11	0.11	0.12	-14.90***	0.00
	(0.02)	(0.03)	(0.01)		
Family average age	47.20	47.60	46.50	1.75*	-0.10
	(15.50)	(22.02)	(11.15)		
Family size	3.10	3.05	3.40	-8.10***	0.12
	(1.40)	(2.02)	(1.03)		
Number of children (¡16)	0.40	0.41	0.36	1.70*	-0.08
	(0.61)	(0.97)	(0.38)		
Number of elderly (¿60)	0.58	0.60	0.62	-0.95	-0.12
	(0.65)	(0.92)	(0.45)		
Number of labor force	1.95	1.90	2.25	-8.70***	0.14
	(1.21)	(1.72)	(0.93)		
Number of illiteracy	0.30	0.32	0.18	6.40***	-0.14
	(0.55)	(0.80)	(0.35)		
Number of unhealthy	0.65	0.68	0.58	3.10***	-0.12
	(0.71)	(1.05)	(0.51)		
Living conditions (outcome variables)					
Access to tap water	0.52	0.50	0.60	-13.20***	0.72
	(0.18)	(0.28)	(0.10)		
Access to internet	0.75	0.72	0.88	-22.10***	0.55
	(0.25)	(0.35)	(0.12)		
Dist. to primary school	7.95	8.30	5.90	11.30***	-0.30
	(5.10)	(7.12)	(3.05)		
Dist. to middle school	8.60	8.85	6.40	12.00***	-0.25
	(4.50)	(6.21)	(3.11)		
Dist. to high school	21.60	22.20	17.10	12.80***	-0.28
	(9.02)	(12.51)	(6.23)		
Dist. to clinic	10.12	10.41	6.81	22.12***	-0.30
	(3.71)	(5.24)	(2.13)		
Dist. to hospital	21.71	22.62	16.41	10.07***	-0.26
	(14.14)	(19.25)	(8.51)		
Dist. to city center	26.51	27.66	19.82	13.10***	-0.35
	(14.42)	(21.16)	(9.51)		
Number of households (2017)	1,020	49	25,413		
F-statistics for joint mean differences				88.6***	0.81

Note: Column (4) compares treatment households with all non-treated households (eligible and ineligible combined). Column (5) compares treatment households with eligible but not resettled households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$ for the t-tests.

We further conduct a joint significance test of these differences using an F-test.[7] The results, reported in the last row of the table, are consistent with the t-tests. We find significant joint differences only for the comparison between the treatment and the full control group in Column (4), but not within the eligible sample in Column (5).

Appendix Table A2 reports descriptive statistics for households classified as low-income in the Shaanxi statistical yearbook 2017. Overall, we find no large differences between the two datasets, which confirms the representativeness of our sample.

4.3 Empirical Strategy

We exploit the 2018 treatment timing in a difference-in-differences (DID) design to identify the treatment effects of PARP on income and work activities:

$$Y_{it} = \beta \times Resettled_{it} + \theta_t + \eta_i + \psi_v \times t + \varepsilon_{it} \tag{1}$$

where Y_{it} denotes the outcome variable related to income or work activities for household i in year t. θ_t and η_i control for year fixed effect and household fixed effect, respectively. $\psi_v \times t$ controls for omitted variables that follow village-specific linear time trends. $Resettled_{it} = 0, 1$ indicates whether household i has been **resettled** in year t. We do not include additional covariates because the fixed effects and time trend are supposed to effectively absorb the endogeneity on (un)observables. For justification, we also provide estimates with covariates in Appendix E with Appendix Table E1 to E4 Column (1). In general, we do not find large differences in estimates with control variables compared with our main estimates in the following sections.

Following Predictions 1 and 2 in Section 3.3, our primary outcomes of interest are migrant-work activities. We measure these activities using household migrant-work income, migrant-work duration, and migrant-work destination choices, including whether workers move to more economically advanced areas. For these outcomes, the sign of β is theoretically ambiguous and depends on which channel dominates. However, the effect is expected to be larger among households with greater labor endowments.

Following Prediction 3, we also examine household farming income and farming expenditure.

[7] This test is conducted through regressing the group indicator on all the variables and jointly testing whether the coefficient estimates equal to zero.

For these farming-related outcomes, we expect β to be small, as the conceptual framework does not predict a substantial change in farming income after resettlement.

We also employ an event-study approach to estimate the relative treatment effect in each year following the regression to justify the parallel trend assumption:

$$Y_{it} = \sum_{t \neq -1} \beta_t \times D_{it} + \theta_t + \eta_i + \psi_v \times t + \varepsilon_{it} \tag{2}$$

where D_{it} is an indicator equal to one if household i is treated and the observation is in event time t relative to 2018 (the baseline year), and zero otherwise. Insignificant estimates for β_t in the pre-treatment periods support the parallel trends assumption.

This design faces two challenges. First, treatment is determined through two stages: eligibility and actual resettlement, as we noted in Section 2.3. Using $Resettled_{it}$ as the treatment indicator ignores selection from eligibility into resettlement, so the estimated β may capture not only the average treatment effect on the treated (ATT) of resettlement, but also unobserved factors affecting both resettlement and outcomes.[8]

Second, eligibility criteria are not fully observed, so both selection stages may correlate with pre-treatment characteristics and outcome trends. This raises concerns about parallel trends, as treated and control households may differ in unobserved ways that generate differential trends not absorbed by fixed effects. Although the event-study in (2) provides a pre-trend check, Table 1 shows that treated households are more disadvantaged in income, work, and resources. If these disadvantages persist and imply lower growth, the DID estimates may be downward biased if selection is not properly addressed.

4.4 Validity of Identification and Robustness

We provide additional arguments to address these concerns. In this program, 1,069 households were deemed eligible at the end of 2017, of which 1,020 were resettled in 2018, implying a take-up rate above 95 percent. This near-universal compliance alleviates concerns about second-stage selection. In particular, when almost all eligible households are resettled, the treatment indicator $Resettled_{it}$

[8] In addition, implementation errors may lead to some ineligible households being resettled, introducing "defiers" and violating monotonicity. However, we find no evidence of such defiers in our sample or in the policy implementation.

closely approximates the eligibility status. As a result, the DID estimates based on $Resettled_{it}$ largely reflect the effect of resettlement with limited bias from endogenous take-up.

Second, we re-estimate the specification (1) by replacing the treatment indicator with the eligibility status:

$$Y_{it} = \beta \times Eligible_{it} + \theta_t + \eta_i + \psi_v \times t + \varepsilon_{it} \tag{3}$$

where $Eligible_{it}$ equals one if household i has been designated as eligible for the program by year t, and zero otherwise.

The corresponding coefficient identifies the intention-to-treat (ITT) effect of the policy, capturing the impact of being assigned eligibility regardless of actual take-up. Comparing the ITT estimates with our baseline results provides a useful robustness check. Given the high compliance rate documented above, the ITT effect should be close in magnitude to the estimated effect of actual resettlement. The corresponding estimates are reported in Appendix E with Appendix Table E1 to E4.

Third, we further assess selection by restricting the sample in two ways. First, we exclude eligible but non-resettled households, comparing resettled households with those never eligible. Second, we restrict the sample to eligible households only, comparing resettled households with eligible non-movers. The latter approach may be less reliable due to the small number of eligible but non-resettled households. We should not expect to find large differences in the estimated effects across these specifications.

Finally, to address concerns that selection bias may lead to violations of the parallel trends assumption and a downward bias, we implement a propensity score matching (PSM) procedure to construct a more comparable control group. Prior to matching, we estimate a logit model of the resettlement decision and find that several pre-treatment household characteristics are significant predictors of treatment status, especially variables related to family labor endowments, as shown in Appendix Table D1. Motivated by these results, we match households based on pre-treatment characteristics, including average age, household size, number of labor force participants, number of unhealthy members, number of illiterate members, number of children, number of seniors, and arable land per capita. Since PARP targeted households with poor livelihood conditions, these pre-treatment characteristics are useful for constructing a more comparable control group. Matching

on variables related to labor characteristics and land resources helps remove poorly comparable control households with substantially stronger livelihood capacity (Rosenbuam and Rubin, 1983; Dehejia and Wahba, 2002).

Given the large number of households in the control group, we adopt a 5:1 nearest-neighbor matching ratio (five control households to one treated household). The matching results, reported in Appendix Figure D1, show substantial improvement in covariate balance between treated and control groups. The matching procedure restricts the sample to observations with sufficient overlap, removing poorly comparable controls. Although it cannot fully address selection on unobservables, it mitigates bias to the extent that selection is correlated with observed characteristics. In our context, the program targets households with poor livelihood conditions, which are closely related to observable variables such as labor endowments and production capacity. To the extent that these are correlated with unobserved determinants, matching can partially account for selection on unobservables. If downward bias arises from selection, improved comparability under PSM-DID should lead to larger estimated effects. In the main analysis below, we present the DID estimates using the matched sample alongside the standard DID results.

5 Empirical Results

In this section, we begin the empirical analysis by examining the effects of PARP on household income and working activities since they serve as the most direct measure of living conditions. Next, we explore the mechanisms through which PARP influences income by analyzing adjustments in family labor allocation toward migrant work. Specifically, building on the conceptual framework, we investigate the last-mile and home-attachment channel by presenting the heterogeneous effects of PARP based on household labor force. Then we assess how these differences in labor characteristics shape the unequal policy impacts, highlighting variations in income gains and losses following resettlement. Finally, we conclude this section with a discussion of robustness checks.

5.1 Impacts on Income and Labor Allocation

We begin by examining income changes among resettled households following PARP implementation. Table 2 reports the results from the DID estimations. Column (1) presents the baseline

estimates from regression (1). Panel A shows that, on average, PARP increases total household income by 12% after resettlement. Panel B indicates that this growth is primarily driven by migrant wage income, which rises by 37%. Following Prediction 1, this result suggests that the last-mile channel dominates on average, as resettled households earn substantially more income from migrant work.

Table 2: Treatment effects of PARP on income

	Baseline DID (1)	PSM-DID (2)
Panel A: dependent variable: total income per capita (log)		
$Resettled_{it}$	0.12***	0.19***
	(0.02)	(0.05)
R-squared	0.75	0.85
N	227,144	50,661
Panel B: dependent variable: wage income per capita (log)		
$Resettled_{it}$	0.37***	0.48***
	(0.08)	(0.10)
R-squared	0.81	0.87
N	228,793	51,084
Panel C: dependent variable: farming income per capita (log)		
$Resettled_{it}$	-0.05	-0.10
	(0.08)	(0.09)
R-squared	0.79	0.82
N	227,574	50,776
Panel D: dependent variable: transfer income per capita (log)		
$Resettled_{it}$	-0.16***	-0.20***
	(0.05)	(0.04)
R-squared	0.81	0.85
N	227,274	51,200
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓

Note: Column (1) uses unmatched DID specification. Column (2) uses PSM-DID. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Panel C shows that farming income is not significantly changed, declining slightly by 5%. This finding is consistent with Prediction 3 from the conceptual framework. Panel D reports a 16% reduction in transfer income, suggesting that resettled households have become less dependent on poverty-related aid and increasingly rely on more sustainable income sources, particularly urban employment. Figure 3 presents the event-study estimates, showing that the coefficients in the pre-treatment periods are not statistically different from zero, providing evidence for the pre-treatment

parallel trends. The post-treatment coefficients are consistent with the DID estimates for each outcome variable, which supports our DID estimates.

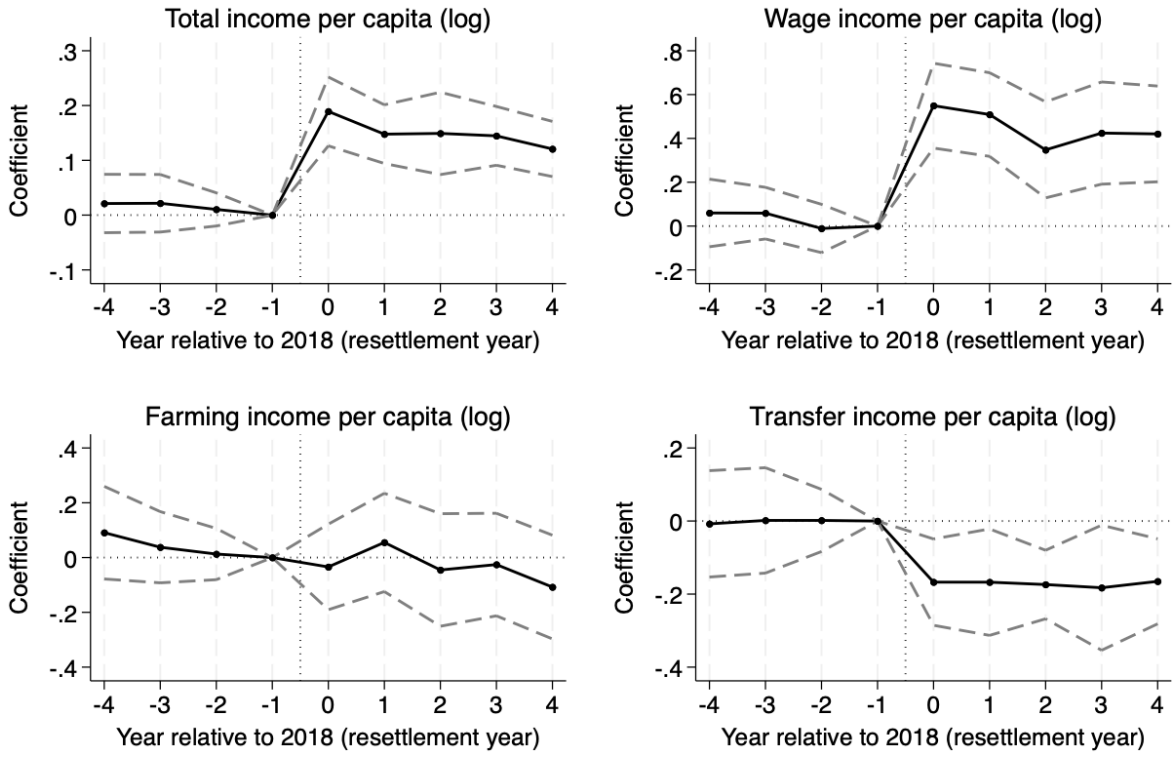


Figure 3: Event study on income variables

Note: This figure reports the event-study estimates corresponding to Table 2. 2017 is omitted as the base year. The solid line shows the point estimates, and the dotted lines indicate the 95% confidence intervals.

Column (2) reports the PSM-DID estimates, which show larger effects on total and wage income and more negative effects on farming and transfer income. This pattern is consistent with improved comparability, suggesting that baseline estimates may be biased toward zero.

In particular, the differences across income components align with the program's targeting mechanism. Households selected into resettlement are likely those that would have remained more reliant on agricultural production and transfer income, and less engaged in migrant work in the absence of the program. By improving balance on these pre-treatment characteristics, PSM-DID reveals stronger gains in wage income and a sharper decline in transfer dependence.

Alternative specifications using $Eligible_{it}$, excluding eligible but non-resettled households, or

restricting to eligible households in Appendix Table E1 yield similar estimates, consistent with the high take-up rate.

We next examine the effects of PARP on household labor allocation, as reported in Table 3. The estimated changes in labor patterns are consistent with the observed income effects. Panels A to C employ three measures of migrant work to capture PARP's impact on household employment activities. Panel A shows that resettlement increases the average duration of urban employment by 0.78 months per year. In Panels B and C, we restrict the sample to migrant workers and use two alternative outcomes related to migration destinations. The motivation is that reduced migration costs should enable resettled households to seek employment in more developed regions outside Shaanxi Province (typically in southeastern China) rather than remaining confined to local urban markets (Chang et al., 2011; Meng, 2012).

Table 3: Treatment effects of PARP on working activities

	Baseline DID (1)	PSM-DID (2)
Panel A: dependent variable: average length of urban migrant work per year		
$Resettled_{it}$	0.78***	0.92***
	(0.15)	(0.18)
R-squared	0.81	0.87
N	227,198	50,693
Panel B: dependent variable: distance between home and work province (log)		
$Resettled_{it}$	0.11***	0.14***
	(0.03)	(0.04)
R-squared	0.81	0.85
N	109,197	30,652
Panel C: dependent variable: GDP per capita of migrant work destination province (log)		
$Resettled_{it}$	0.10***	0.11***
	(0.02)	(0.02)
R-squared	0.81	0.84
N	109,634	28,524
Panel D: dependent variable: farming expenditures per capita (log)		
$Resettled_{it}$	-0.12*	-0.14*
	(0.07)	(0.08)
R-squared	0.81	0.82
N	227,249	50,820
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓

Note: Column (1) uses unmatched DID specification. Column (2) uses PSM-DID. In Panel B and C we exclude households with zero urban working months. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Specifically, in Panel B, where the outcome is the distance between the home village and the migrant work province, we find that PARP increases this distance by 11% (approximately 112 kilometers). In Panel C, using the GDP per capita of the destination province as a proxy for economic conditions, the results show a 10% increase, indicating that resettled households are shifting toward employment in more developed areas. The evidence from Panel A to C further supports Prediction 1: the last-mile channel appears to dominate on average, as resettled households engage in longer migrant work and move toward more economically developed destinations. Finally, Panel D examines farming-related expenditure, which declines modestly by 12% after resettlement (not statistically significant). The result is consistent with the insignificant decline in farming income in Table 2 and further supports Prediction 3, which suggests that farming income remains relatively stable after resettlement. To distinguish PARP from land-oriented resettlement programs in other countries (Mueller et al., 2014; Mendola and Simtowe, 2015), we use the DID specification to examine changes in the amount of farmland operated by rural households. Consistent with China's rural land-tenure arrangements, we find no evidence that resettlement led to a substantial change in household farmland holdings, as reported in Appendix Table C1.

We include estimates of event-study approaches in Figure 4. The insignificant estimates in the pre-treatment periods support the pre-treatment parallel trend. And the post-treatment event-study coefficients are consistent with the DID estimates. Column (2) presents the estimates of PSM-DID, where similar to Table 2, the baseline DID underestimate the treatment effects on the working activities. Also, the three alternative specifications in Appendix Table E2 do not lead to large changes in the estimates of treatment effects.

We might be surprised by the significant returns to family labor adjustment following PARP. Despite a modest decline in farming activities (around 5%) and a small increase in urban migrant work length (0.78 months), both wage and total income are largely increased by 10% to 40%. This pattern suggests that disadvantaged locations may have led households to allocate labor less efficiently between farm and non-farm activities before resettlement. This finding aligns with the broader literature on productivity gains from reduced migration costs and improved infrastructure (Bryan et al., 2014; Bryan and Morten, 2019; Hidrobo et al., 2022).

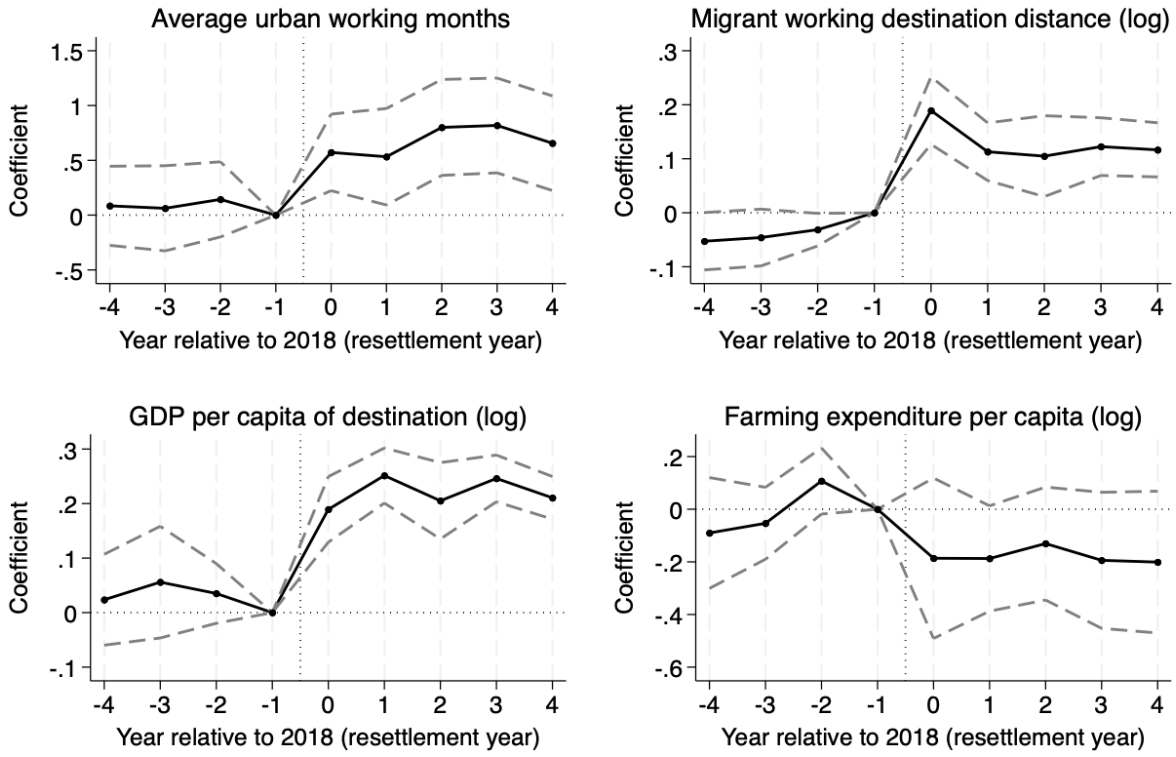


Figure 4: Event study on working activities

Note: This figure reports the event-study estimates corresponding to Table 3. 2017 is omitted as the base year. The solid line shows the point estimates, and the dotted lines indicate the 95% confidence intervals.

5.2 Discussions on Liquidity Constraints

In this section, we provide empirical evidence on the last-mile and home-attachment channels through heterogeneity analysis. Before we proceed, we first present changes in migration costs and living conditions after resettlement using baseline DID estimates in Table 4 to justify the underlying assumptions of the two channels. Since PARP was designed to resettle households away from disadvantaged neighborhoods, it is unsurprising that migration costs (approximated by the distance from home to urban areas) are reduced and living conditions (approximated by the distance to educational institutions or medical facilities and access to safe water or stable electricity) are improved. Figure 5 and 6 plot the coefficient estimates of the event-study approach, suggesting the parallel pre-trend as we observe the insignificant estimates in the pre-treatment periods. The magnitude of post-treatment estimates is also consistent with that of the DID estimates. The three alternative specifications in Appendix Table E3 and E4 do not induce large changes in the estimates.

Our conceptual framework relies on the key assumption that household labor endowments are closely linked to liquidity constraints. To examine this assumption, we analyze the relationship between household labor endowments and migrant work under pre-treatment conditions, highlighting how labor endowments shape migration decisions through their role in relaxing liquidity constraints. To isolate the role of migration costs and living conditions, we regress the duration of migrant work on household labor characteristics while controlling for urban distance and local living conditions. Observations from 2018 onward are excluded to remove potential resettlement effects. We consider the share of labor force, the share of healthy members, and the share of kids under 16 and senior members over 60 as proxies for family labor characteristics. Labor endowment is measured by the labor share to account for household size and dependency structure and better capture effective labor availability for migration decisions. The results in Figure 7 reveal a positive association between household labor force share and migrant work activities, suggesting the model's assumption that households with larger labor endowments are better able to overcome liquidity constraints and participate in urban employment. These results support the reasoning behind Prediction 2, suggesting that family labor plays an important role in sending migrant workers.

To support the argument in Section 3.2 that migrant work in our setting does not primarily

Table 4: Change of migration cost and living conditions after PARP

	Baseline DID	PSM-DID	Baseline DID	PSM-DID
	(1)	(2)	(3)	(4)
Dependent variable:	Access to tap water		Access to internet	
$Resettled_{it}$	0.53***	0.59***	0.15***	0.18***
	(0.12)	(0.14)	(0.03)	(0.04)
R-squared	0.24	0.26	0.23	0.25
N	227,065	50,693	226,536	50,214
Dependent variable:	Dist. to urban center		Dist. to primary school	
$Resettled_{it}$	-10.34***	-11.20***	-2.41***	-2.70***
	(2.56)	(2.90)	(0.72)	(0.85)
R-squared	0.67	0.69	0.75	0.77
N	227,365	30,652	227,365	30,115
Dependent variable:	Dist. to middle school		Dist. to high school	
$Resettled_{it}$	-2.57***	-2.80***	-10.52***	-11.40***
	(0.67)	(0.80)	(2.45)	(2.80)
R-squared	0.59	0.61	0.62	0.64
N	227,365	29,884	227,365	29,447
Dependent variable:	Dist. to clinic		Dist. to hospital	
$Resettled_{it}$	-3.82***	-4.20***	-2.31***	-2.60***
	(0.82)	(0.95)	(0.69)	(0.80)
R-squared	0.74	0.75	0.69	0.71
N	227,365	30,201	227,365	29,980
Year FE	✓		✓	
Household FE	✓		✓	
Village-specific trend	✓		✓	

Note: Each pair of columns reports the baseline DID and PSM-DID estimates for the corresponding outcome. The PSM-DID sample is smaller because treated households are matched to comparable controls before estimation. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the village level.

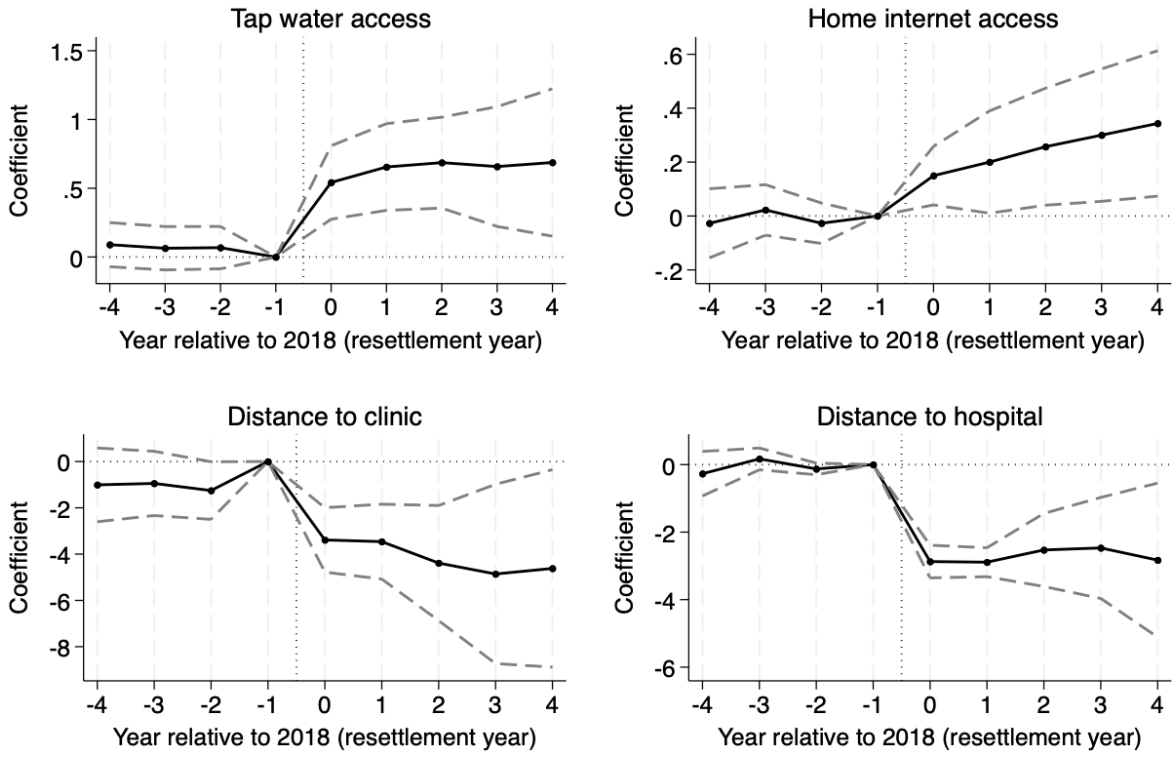


Figure 5: Event study on access to utilities

Note: This figure reports the event-study estimates corresponding to Table 4. 2017 is omitted as the base year. The solid line shows the point estimates, and the dotted lines indicate the 95% confidence intervals.

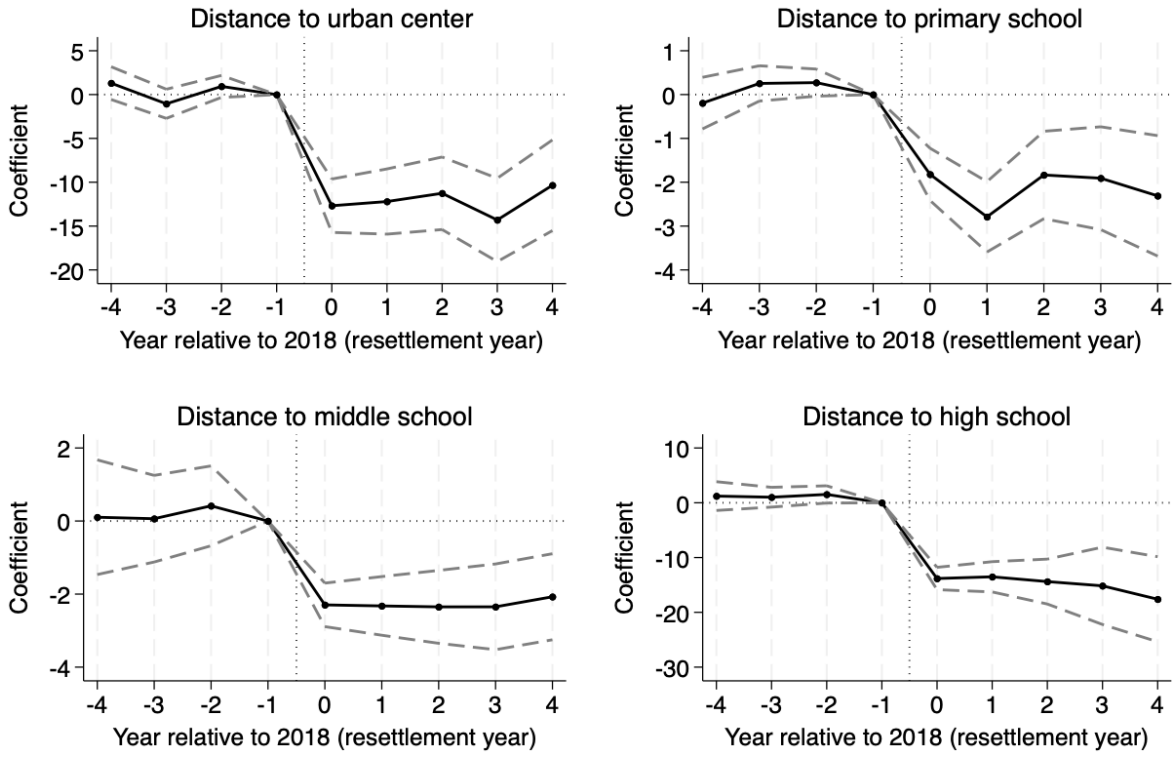


Figure 6: Event study on distance to closest facility

Note: This figure reports the event-study estimates corresponding to Table 4. 2017 is omitted as the base year. The solid line shows the point estimates, and the dotted lines indicate the 95% confidence intervals.

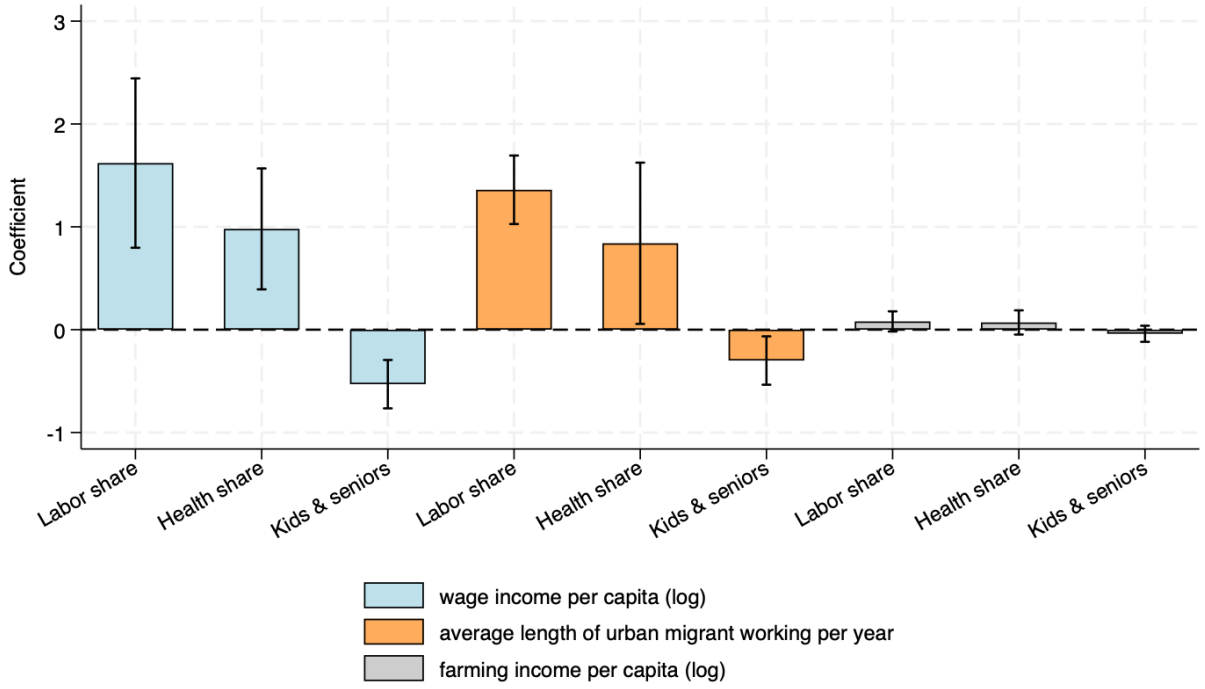


Figure 7: Relations of household labor characteristics and working patterns

Note: This figure plots the coefficients from regressions of work-related outcomes on household labor characteristics. Colors distinguish the outcome variables. The labels on the horizontal axis denote the labor-characteristic regressors. We use only pre-treatment samples only and control for variables categorized in living conditions in Table 1, year and household FEs, and village-specific time trends.

depend on advanced occupation-specific skills, Appendix Table C2 reports regressions of migrant work activities on household education levels using the pre-treatment sample. We find no significant association between household education and migrant work activities, suggesting that a lack of advanced skills is unlikely to be a major barrier to migrant work in our sample.

However, as emphasized in the conceptual framework in Section 3.3, alternative mechanisms may confound this relationship. To further validate the assumption above, we conduct additional robustness checks to rule out competing channels that could also account for the positive relationship between labor endowments and migrant work documented above.

In Appendix Table C3 Column (2), we control for a household borrowing indicator to capture access to external financing. The coefficient on borrowing is insignificant, while the coefficient on labor force share remains stable and significant, suggesting that the relationship is not driven by credit access. Column (3) and (4) further control for village migration networks, proxied by the village migrant work share and the share of workers migrating to the same destination province, following Munshi (2003). These variables capture information diffusion and coordinated migration that may both lower migration costs and correlate with labor endowments. The coefficient on labor force share remains largely unchanged, suggesting that the result is not driven by network effects.

In Figure 7, we also include the regression estimates of farming income on labor endowments. Interestingly, farming income per capita does not vary systematically with household labor characteristics, while migrant work and wage income increase significantly with labor endowment. This result suggests that the advantage of labor-rich households does not primarily come from expanding agricultural earnings. Instead, they are better able to reallocate labor toward migrant work without substantially altering income from home production.

This evidence highlights a key feature of household behavior that farming income is relatively rigid and serves as a liquidity base. As a result, households cannot fully reallocate labor to migration despite higher returns. For labor-scarce households, maintaining baseline home production tightens liquidity constraints and limits their ability to finance migration. In contrast, labor-rich households, with surplus labor beyond this requirement, are better able to exploit migration opportunities. This underscores the role of home production as a key liquidity source, consistent with the literature on land as insurance (Noack et al., 2018; Rathi, 2022).

5.3 Heterogeneity by Family Labor Characteristics

Building on the link between labor endowments and liquidity constraints, we examine heterogeneous PARP effects by household labor characteristics, focusing on how family labor shapes the relative strength of the last-mile and home-attachment channels. Table 5 reports results by labor share, number of healthy members, and number of fragile members (children and seniors) who are not likely to work. In Column (1), Panels A and B show that PARP's effects on wage income and migrant work increase with labor share. A 1% rise in labor share is associated with a 1.5% increase in per capita wage income and 0.019 additional months of migrant work. When labor share falls below about 37% (0.55/1.49), wage income declines after resettlement. These results follow Prediction 2.

This finding is informative about the underlying mechanisms. In the absence of liquidity constraints, higher post-resettlement living costs would raise incentives to migrate. The negative coefficient on the non-interaction term instead suggests these households cannot act on this incentive. Limited labor makes it difficult to maintain home production while financing migration, so the home-attachment channel dominates the last-mile effect, reducing migration. In contrast, the positive interaction term indicates that labor-abundant households can overcome these constraints and respond to improved migration conditions, consistent with our framework. Column (2) and (3), using alternative measures of labor resources, yield similar results.

To further substantiate this interpretation, we repeat the regression by interacting the labor force measure with $Resettled_{it}$ while additionally controlling for variables related to living conditions. These controls capture changes in local amenities and costs associated with resettlement and therefore help absorb the home-attachment channel. As a result, an important source of heterogeneity between labor-scarce and labor-abundant households is attenuated, and the remaining variation more closely reflects the last-mile channel.

The results, reported in Column (4), are consistent with this interpretation. The coefficient on the non-interaction term becomes close to zero and insignificant, indicating that once the home-attachment channel is accounted for, resettlement no longer reduces migration for these labor-scarce households. At the same time, the interaction term remains positive, although somewhat attenuated, suggesting that variation driven by the home-attachment channel is largely absorbed. As a result, the overall degree of heterogeneity across households is substantially reduced. This high-

Table 5: Heterogeneous effects of PARP by labor force demographics

	Labor share (1)	Health share (2)	Kids & seniors share (3)	Labor share (4)
Panel A: dependent variable: wage income per capita (log)				
$Resettled_{it}$	-0.55***	-0.29**	0.31***	0.15
	(0.13)	(0.13)	(0.11)	(0.15)
$Resettled_{it} \times LaborCharacteristics_{it}$	1.49***	0.97**	-1.25***	1.10**
	(0.17)	(0.47)	(0.48)	(0.52)
R-squared	0.85	0.86	0.83	0.91
N	227,125	226,925	226,896	226,927
Panel B: dependent variable: average length of urban migrant work per year				
$Resettled_{it}$	-0.38**	-0.73***	0.45***	0.03
	(0.16)	(0.25)	(0.19)	(0.02)
$Resettled_{it} \times LaborCharacteristics_{it}$	1.87***	1.58***	-1.37***	1.31***
	(0.26)	(0.32)	(0.52)	(0.44)
R-squared	0.86	0.87	0.83	0.90
N	227,068	226,857	226,864	226,827
Panel C: dependent variable: distance between home and migrant work destination				
$Resettled_{it}$	-0.05**	-0.07**	0.15***	-0.01
	(0.02)	(0.03)	(0.05)	(0.01)
$Resettled_{it} \times LaborCharacteristics_{it}$	0.21**	0.25**	-0.06**	0.17***
	(0.09)	(0.10)	(0.03)	(0.05)
R-squared	0.83	0.87	0.86	0.92
N	108,202	107,932	108,325	108,014
Panel D: dependent variable: GDP per capita of migrant work destination province				
$Resettled_{it}$	-0.03**	-0.04**	0.17***	0.01
	(0.01)	(0.02)	(0.05)	(0.01)
$Resettled_{it} \times LaborCharacteristics_{it}$	0.14***	0.15**	-0.10***	0.10**
	(0.05)	(0.06)	(0.04)	(0.05)
R-squared	0.83	0.86	0.87	0.90
N	108,937	108,652	107,932	108,157
Control for living conditions	×	×	×	✓
Time FE	✓	✓	✓	✓
Household FE	✓	✓	✓	✓
Village-specific trend	✓	✓	✓	✓

Note: Column (1) to (3) use share of labor force, share of healthy members, and share of kids and seniors to measure the labor characteristics, respectively for the interaction term. Column (4) repeats Column (1) but additionally controls for living condition variables. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

lights the central role of changes in living conditions in generating unequal effects across households, as we noted in Prediction 2.

As supplementary evidence on PARP's effects on labor allocation, we re-estimate Table 5 using an indicator for sending migrant workers as the outcome, capturing extensive-margin responses. Results in Table 6 Panel A largely mirror those for the intensive margin, except for the insignificant effect among households with low labor share. One explanation is government aid: households without migrants are more likely to receive transfers that help relax liquidity constraints. Consistent with this, adding transfer income as a control in Column (4) and (5) yields results similar to the intensive-margin estimates. Panel B reports heterogeneous effects on farming income. As discussed in Section 5.2, the lack of variation by labor endowment is unsurprising, as agricultural production serves as a relatively rigid source of subsistence and liquidity rather than a margin of adjustment.

Table 6: Heterogeneous effects of PARP on extensive margins and farming income

	Labor share (1)	Health share (2)	Kids & seniors share (3)	Labor share (4)	Labor share (5)
Panel A: dependent variable: whether send out migrant workers					
$Resettled_{it}$	-0.03*	-0.09***	0.04***	-0.10***	0.02
	(0.02)	(0.03)	(0.01)	(0.03)	(0.02)
$Resettled_{it} \times LaborCharacteristics_{it}$	0.17***	0.10**	-0.08***	0.20***	0.17***
	(0.03)	(0.04)	(0.03)	(0.05)	(0.04)
R-squared	0.21	0.32	0.25	0.35	0.30
N	226,452	226,875	227,172	225,425	226,953
Panel B: dependent variable: farming income per capita (log)					
$Resettled_{it}$	-0.03*	-0.09*	0.04		
	(0.02)	(0.06)	(0.03)		
$Resettled_{it} \times LaborCharacteristics_{it}$	0.07	0.10*	-0.08*		
	(0.06)	(0.06)	(0.05)		
R-squared	0.86	0.87	0.85		
N	226,932	227,065	227,172		
Control for transfer income	×	×	×	✓	✓
Control for living conditions	×	×	×	×	✓
Time FE	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓
Village-specific trend	✓	✓	✓	✓	✓

Note: Column (1) to (3) use share of labor force, share of healthy members, and share of kids and seniors to measure the labor characteristics, respectively for the interaction term. Column (4) repeats Column (1) but additionally controls for transfer income per capita. Column (5) further controls for living condition variables. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Our findings carry important policy implications. While resettlement encourages a shift toward

urban employment, the increase in migrant work remains modest (0.78 months on average). This slow adjustment reflects persistent liquidity and institutional constraints within the farming sector. Farming, though providing stable income, also limits migration by serving as the main source of liquidity for migration-related expenses. Hence, resettlement alone cannot fully eliminate household vulnerability. The post-PARP income gains remain fragile, as they depend heavily on the availability of family labor for urban work. Any shocks that substantially reduce household labor capacity may push families back into poverty due to their inability to finance migration costs. In Appendix F, we include more discussions on the post-treatment vulnerability and unequal effects among the resettled households.

5.4 Robustness Checks

We conduct several robustness checks to ensure the validity of the causal relationships described above.

Missing observations. Some demographic variables are missing, likely due to differences in interview standards across local governments. Appendix Table G1 reports low missing rates, with the highest below 3%. It also compares the full sample with a reduced sample that drops any observation with missing values in any variable, showing no substantial differences except in household age and size. Re-estimating the DID models on the reduced sample (Appendix Table G2) yields similar results.

Anticipation of program implementation. Households may have anticipated PARP and misreported conditions to influence selection, especially given similar resettlement programs in earlier years in Shaanxi. To address this, we manually advance the treatment timing to pre-2018 years as fake treatment timings. The resulting estimates, shown in Appendix Figure G1, are insignificant, suggesting that anticipation is not a major concern.

Spillover effects from surrounding counties. As noted in Section 2, similar resettlement programs were implemented in other counties after 2016. Spillovers through local labor market adjustments may bias identification if they differentially affect treated and control households within our county. To address this, we exploit within-county spatial variation using two approaches. First, we interact treatment with distance to county borders as a proxy for exposure to neighboring counties, given that resettlement sites are typically located within the county. Second, we exclude

households near county boundaries. Results in Appendix Table G3 to G6 show no significant heterogeneity by border proximity, and excluding these households does not materially change the estimates, suggesting that spillovers do not drive our results.

Confounding effects from earlier resettlement programs. As discussed in Section 2, several resettlement programs were implemented before 2018. We argue these are unlikely to bias our results. First, our data confirm that none of the sampled households were relocated prior to the focal program, so the estimates are not driven by direct exposure. Earlier programs may still generate indirect effects, for example by shaping local expectations or economic conditions. Although detailed data on historical relocation sites are unavailable, we exploit variation in likely exposure based on policy targeting. Since earlier programs focused on disaster-prone mountainous areas, we classify households closer to these regions, particularly in the southeastern part of the county (Figure 2), as more exposed.

We then replicate the previous approaches by interacting treatment with this exposure measure and excluding these households. Results in Appendix Table G7 to G10 show no systematic heterogeneity, and excluding these households does not materially change the estimates, suggesting that earlier programs do not confound our results.

Confounding anti-poverty policies. As PARP is part of a broader anti-poverty campaign, other concurrent policies may affect outcomes. We focus on credit support and industrial support programs, which are most likely to generate similar effects. Credit support programs provide improved access to small-scale financial services for impoverished households, facilitating borrowing for production and migration-related expenses. Industrial support programs aim to increase local income opportunities by promoting agro-processing activities, for example through government-supported establishment of agricultural processing facilities in villages, which raise the value added of farm products and enhance household earnings.

Appendix Table G11 shows that overlap with these programs is below 6 percent, indicating limited exposure. We further control for participation and exclude affected households. Results in Appendix Table G12 and G13 remain similar, suggesting that our estimates are not driven by concurrent policies.

6 Conclusions

This paper investigates how reduced migration costs (last-mile channel) and improved living conditions (home-attachment channel) jointly shape rural households' labor allocation decisions under China's Poverty Alleviation Resettlement Program (PARP). Our theoretical framework highlights how these two channels operate in opposite directions, and how their relative strength depends on household labor endowments.

Using a difference-in-differences design with rich panel data on all registered impoverished households in the study county from 2014 to 2022, we find that resettlement significantly increases household income, particularly wage income from migrant work, through a reallocation of labor away from farming toward urban employment. However, these gains are highly uneven. Households with abundant family labor resources benefit substantially from resettlement, while those with limited labor force see stagnant or even declining earnings.

Our findings demonstrate that while PARP effectively enhances income for some households, it can unintentionally exacerbate inequality among the poor by failing to account for heterogeneity in labor endowments. This has important policy implications. Resettlement alone is insufficient to ensure poverty alleviation for labor-constrained households, as their ability to leverage improved access to urban work is limited by liquidity constraints. Future poverty alleviation policies should therefore integrate complementary measures to enable vulnerable households with limited labor resources to benefit from resettlement opportunities.

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Appendix

A Supplementary Descriptive Statistics

Table A1: Descriptive statistics in 2017

Variables	Resettled households (Treatment group) (1)	Eligible but not resettled (Control group) (2)	Ineligible (3)	Resettled vs. control group t-statistics (4)	Resettled vs. eligible not resettled t-statistics (5)
Income per capita (outcome variables)					
Total income	5756.57	5688.20	6281.23	-7.83***	0.09
	(3118.95)	(5019.25)	(2115.04)		
Wage income	2948.36	2890.15	3749.80	-12.23***	0.08
	(3006.83)	(4795.26)	(1985.26)		
Farming income	777.46	765.30	826.50	-1.53	0.05
	(1011.47)	(1605.26)	(703.25)		
Transfer income	2040.76	2105.88	1709.68	4.38***	-0.13
	(2338.37)	(3596.25)	(1713.05)		
Working activities (outcome variables)					
Average migrant work (months)	3.57	3.48	5.12	-20.47***	0.12
	(3.68)	(5.18)	(2.41)		
Farming expenditure per capita	165.72	167.21	159.27	0.80	-0.03
	(253.95)	(359.58)	(181.02)		
Household (PSM covariates)					
Arable land per capita (km^2)	0.11	0.11	0.12	-15.63***	0.00
	(0.02)	(0.04)	(0.01)		
Family average age	48.13	48.55	47.14	1.86*	-0.12
	(16.81)	(23.72)	(12.11)		
Family size	3.14	3.10	3.47	-8.54***	0.13
	(1.53)	(2.20)	(1.10)		
Number of children (¡16)	0.42	0.43	0.38	1.89*	-0.07
	(0.67)	(0.95)	(0.45)		
Number of elderly (¿60)	0.61	0.62	0.62	-0.89	-0.14
	(0.71)	(1.10)	(0.48)		
Number of labor force	1.99	1.96	2.36	-9.25***	0.15
	(1.28)	(1.85)	(0.95)		
Number of illiteracy	0.32	0.34	0.20	6.95***	-0.16
	(0.61)	(0.85)	(0.40)		
Number of unhealthy	0.70	0.72	0.62	3.37***	-0.13
	(0.75)	(1.05)	(0.55)		
Living conditions (outcome variables)					
Access to tap water	0.58	0.55	0.67	-14.05***	0.69
	(0.21)	(0.25)	(0.12)		
Access to internet	0.81	0.78	0.95	-24.71***	0.52
	(0.28)	(0.40)	(0.15)		
Dist. to primary school	7.56	7.91	5.62	11.69***	-0.32
	(5.26)	(7.41)	(3.22)		
Dist. to middle school	8.18	8.25	6.11	12.43***	-0.23
	(4.68)	(6.61)	(3.05)		
Dist. to high school	20.56	21.10	16.26	13.21***	-0.29
	(9.24)	(12.89)	(6.52)		
Dist. to clinic	9.64	9.90	6.48	23.84***	-0.32
	(3.94)	(5.62)	(2.21)		
Dist. to hospital	20.68	21.57	15.59	10.45***	-0.27
	(14.81)	(21.11)	(9.25)		
Dist. to city center	27.07	28.20	20.04	13.49***	-0.36
	(15.28)	(21.92)	(11.02)		
Number of households	1,020	49	25,413		
F-statistics for joint mean differences				96.4***	0.74

Note: Column (4) compares treatment households with all non-treated households (eligible and ineligible combined). Column (5) compares treatment households with eligible but not resettled households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$ for the t-tests.

Table A2: Descriptive statistics compared with Shaanxi 2017 statistical yearbook

	Shaanxi 2017 statistical yearbook (1)	Diff. to impoverished sample 2017 (2)
Total income	6205.40	-101.78
Wage income	2935.80	-784.36
Farming income	1254.50	429.82
Transfer income	1770.40	48.45
Household size	3.60	0.15
Number of labor force	2.30	-0.04

Note: Income is annual per capita. Column (2) reports the difference between sample in this study and low-income households from Shaanxi 2017 statistical yearbook.

B Theoretical Framework

In this section, we present a model following the pattern of migrant work in China with the presence of migration cost and living conditions to quantify the last-mile channel and home-attachment channel based on Qiu et al. (2024). The implications of the model results will motivate our empirical analysis.

B.1 Model Set Up

Following Mckenzie and Rapoport (2007), consider a household with N family members. The share of labor force is l, which excludes kids, seniors, and disabled members who cannot work. Suppose for each family member, their time endowment is 1, which can be regarded as a year. We assume that for family members who do not work, such as kids or seniors, they spend all the time to enjoy the life at home. On the contrary family labor force will input all the time to working in either agriculture sector or urban sector. On average, each labor force will contribute proportion m of their labor endowment to urban sectors. We assume the sources of income consist of farming income and urban migrant work income only. The farming income follows a quadratic form (Qiu et al., 2024):

$$Farming\ income = ANl(1-m) - \frac{B\left[Nl(1-m)\right]^2}{2} \tag{4}$$

where A is the TFP of farming for each unit of farming labor supply. B is the cost parameter of farming activities followed by a quadratic form. The household farming labor supply is $Nl(1-m)$.

For migrant work earning,

$$Migrant\ working\ income\ (wage\ income) = Nlm(w-\bar{c}) \tag{5}$$

where w is wage level of urban working for each worker. $\bar{c}$ is the migration cost such as transportation costs and living costs in urban. Assume $w-\bar{c} \gg A-BNl(1-m)$ for any m to reflect the fact that migrant work delivers much higher returns to labor supply.

Households' utility level depends on household consumption level c and living conditions of the family at home, denoted by q:

$$u(c,m) = c + \delta qN(1-lm) \tag{6}$$

where $\delta qN(1-lm)$ measures the total level of home enjoyment of family members left at home. q is the home amenity level (or living conditions). We add $\delta > 0$ with the aim to translate $qN(1-lm)$ into monetary-equivalent utility measurement.

We should note that c here includes the inessential goods only and does not involve goods used for basic living subsistence. The basic living subsistence is typically referred to stuffs that guarantee household survival, which cannot be actively chosen by households as the subsistence is biologically determined. Therefore, we regard c and q as perfect substitutes since both of them are used to give extra improvement of family living quality.

B.2 Timeline of Decisions and Household Constraints

Households choose the optimal level of labor allocation and consumption level to maximize the utility given the following budget constraints of households.

$$\underbrace{ANl(1-m) - \frac{B\left[Nl(1-m)\right]^2}{2}}_{\text{Farming income}} + \underbrace{Nlm(w-\bar{c})}_{\text{Wage income}} = \underbrace{c}_{\text{Consumption}} + \underbrace{kqN(1-lm)}_{\text{Amenity costs}} + \underbrace{I \cdot N}_{\text{Living subsistence}} \tag{7}$$

where k is the cost of per unit amenity (such as electricity bills). I is the personal subsistence spending. It is important to point out that amenity costs only include the home period of family members, while we account all the expenditures during urban working period for $\bar{c}$.

However, constraint (7) alone cannot fully describe the household's decision-making process, as it overlooks the financing needs associated with sending migrant workers. As illustrated in Figure B1, we conceptualize the household labor allocation process as consisting of two episodes. The first episode corresponds to the farming season, during which migrant workers remain (or return) home for agricultural activities, with all crops harvested at the end of the period. Farming income from this episode is used to meet subsistence and amenity needs and to finance upcoming migration costs such as transportation and urban living expenses. The second episode represents the non-farming season, when households send members to cities and incur migration costs upfront. Wage income is received at the end of this episode, while family members remaining at home generate no income during this period.

Following this structure, we can formulate the liquidity constraint for the first episode as follows.

This setting is consistent with past literature (Bryan et al., 2014; Cai, 2020)

$$ANl(1-m) - \frac{B\left[Nl(1-m)\right]^2}{2} \geq Nlm\bar{c} + I \cdot N + kqN(1-lm) \tag{8}$$

where assume that $Al - BNl^2/2 \geq I + kq$ to assure when $m = 0$, farming income is enough for basic living requirements.

To account the role of financing requirement, we consider two types of households with binding finance constraint or not as follows:

Definition 1 *Constrained households:*

Constrained households are those whose liquidity constraint (8) is binding.

Definition 2 *Unconstrained households:*

Unconstrained households are those whose liquidity constraint (8) is not binding.

When households are not constrained by the costs in (8), mathematically, such households should have relatively high labor share l on the LHS to generate enough stable farming income even if m is high[9]. These households are able to achieve the most efficient labor allocation between farming and urban sectors that maximizes utility only constrained by the budget constraint. However, for households that are constrained by the costs (i.e. binding (8)), they mathematically should have lower level of l. Therefore, their choice of m is restricted to lower levels since their priority is to struggle with basic living requirements (pay for q and I) by this limited available labor share l instead of generating income through the costly migrant work. An example is helpful to understand the pattern. A family with one abled adult and two kids is not able to migrate for urban working if the RHS of (8) with even $m = 0$ must be satisfied through the farming income from at least one full-time farming worker. In other words, this family is too struggling with life to send out migrant workers. However, when one kid is replaced by an adult, the family might choose to send out one adult for migrant work since now they can afford the cost requirements just by the adult at home and begin to explore more ways to generation income by sending the extra adult.

[9]It is natural in (8) to consider the variation of the other parameters, which, however, are not likely to be heterogeneous.

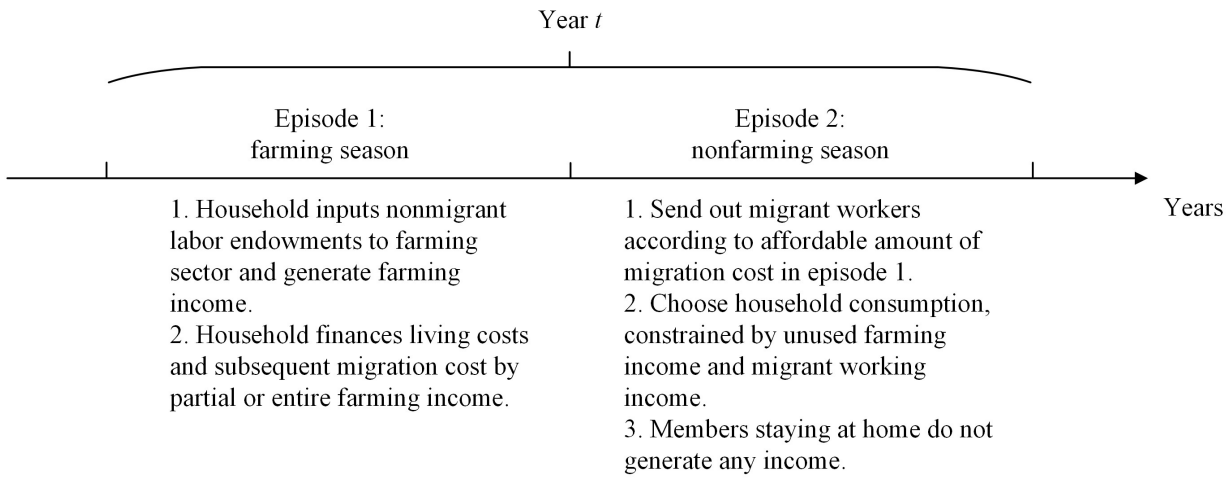


Figure B1: Timeline of household decision in the theoretical model

B.3 Model Implications

The solutions to the utility maximization problem with respect to m and c deliver the following results:

Proposition 1 *For households constrained by liquidity constraint:*

The optimal choice of m^* is directly derived by holding constraint (8) binding. And it follows that $\partial m^*/\partial \bar{c} < 0$ (last-mile channel) and $\partial m^*/\partial q < 0$ (home-attachment channel).

For empirical implications, among constrained households, a reduction in migrant work costs increases the migration rate for resettled households. However, resettlement also improves living conditions, which in turn reduces the migration rate.

Proposition 1 highlights the choice of households who are constrained by financing requirements in response to the change of migration cost and living conditions brought by resettlement. A crucial feature for such group of households is that they will use up all the farming income to fulfill the living costs (i.e. IN and kNq) and migration cost (i.e. $Nlm\bar{c}$), with living cost fulfillment prioritized. In this sense, migration rate is determined by the per unit migration cost c and the available net farming income by deducting living costs from the total farming income. Therefore, given I, k, and q, the last-mile channel must encourage migrant work by decreasing the per unit migration cost c since the available migrant work funds are not affected by this channel. In terms of home attachment channel, the improvement of living conditions will directly increase the living cost and,

as a result, crowd out the available funds for financing labor migration, thereby decreasing the migration rate.

Proposition 2 *For households unconstrained by financing requirements:*

$\partial m^*/\partial \bar{c} < 0$ (last-mile channel). $\partial m^*/\partial q < 0$ if $\delta > k$, and $\partial m^*/\partial q > 0$ if $\delta < k$ (home-attachment channel).

For empirical implications, a reduction in migration costs after resettlement encourages a higher migration rate among households that are not constrained by financing requirements—typically those with a greater household labor endowments. But the effect of living condition changes depends on the relative magnitude of home enjoyment (δ) and amenity costs (k).

The implication of Proposition 2 is different from the case for constrained households. For last-mile channel, reduced migration costs encourages migration by increasing the net returns from migrant work instead of relaxing the finance pressure. The home attachment channel is more complicated. First, unconstrained households include the enjoyment at home (i.e. $qN(1-lm)$) into the decision of migrant work, where improved living condition should prevent the it for home enjoyment loss. Second, improved living conditions will increase the living cost, which turns to decrease the consumption level in the utility. In this sense, to make up the loss of consumption, households should choose higher migration rate for more earnings. In this sense, there is a trade off for unconstrained households between the positive and negative effects of improved living conditions. If the living conditions are very poor before resettlement, then households will be more sensitive to the improvement of living conditions, which presents the dominant pattern of enjoyment at home from better living conditions. In other words, improved living conditions in this case induce a decrease of migrant work activities. However, if the living conditions before resettlement is relatively great, households are getting more sensitive to the change of living conditions after resettlement as the diminishing marginal utility to consumption, which induces an increase of migration rate with the improvement of living conditions in this case.

Proposition 3 *Sensitivity:*

Use subscripts un for unconstrained households and con for constrained. PARP induces a negative change of $\bar{c}$ and a positive change of q. Then following Proposition 1 and 2, $\Delta\bar{c} < 0$ leads to $\Delta m_{con} > 0$ and $\Delta m_{un} > 0$. However, $\Delta q > 0$ implies $\Delta m_{con} < 0$ but Δm_{un} being undetermined.

From an empirical perspective, resettlement is therefore expected to exert a more positive average effect on migration among households with a larger labor force, as the home-attachment channel weakens or may even reverse among unconstrained households. In contrast, households with a limited labor force are more likely to experience a less positive or even negative impact of PARP on migrant labor supply, as the home-attachment channel unambiguously discourages migration in their case.

Proposition 3 highlights the heterogeneous results regarding the two channels. First, for the last-mile channel, in general, constrained households should be less sensitive to the change of migration compared with unconstrained households as they are facing the living cost pressure, which is the largest factor that prevents the migrant work activities. In this sense, the pure change of migration cost is not enough for them to get rid of the case with insufficient credits for migrant work activities, which finally inhibits the increase of migration rate. However, it is different for unconstrained households since reduced migration cost directly determines the net return of migrant work. When households are resettled away from a disadvantaged neighborhoods with high migration costs, they should be very incentivized to increase migrant work activities. Second, in terms of home attachment channel, unconstrained households should overall be less sensitive to the improvement of living conditions given Proposition 3 as the existence of the two contradictory effects, while constrained households will decrease the migration rate more as their available funds for migrant work activities is even lower.

B.4 Proof of Propositions

We first write down optimization problem:

$$\max_{c,m} \; c + \delta q N(1 - lm) \tag{9}$$

$$s.t.\ ANl(1-m) - \frac{B\left[Nl(1-m)\right]^2}{2} + Nlm(w - \bar{c}) = c + kNq(1 - lm) + IN \tag{10}$$

$$ANl(1-m) - \frac{B\left[Nl(1-m)\right]^2}{2} \geq Nlm\bar{c} + IN + kNq(1 - lm) \tag{11}$$

We denote the the Lagrangian multipliers of constraint (10) and (11) by λ_1 and λ_2, respectively.

First, consider the unconstrained households whose financing requirement (11) is not binding. In this case, $\lambda_2 = 0$. FOCs of Lagrangian deliver the optimal choice of m:

$$m_{un} = \frac{Nl(w - \bar{c}) - (ANl - BN^2l^2) + (k - \delta)qNl}{BN^2l^2} \tag{12}$$

Second, for the binding case, the choice of m is from (11):

$$m_{con} = \frac{-g_1(\bar{c}, q) + \sqrt{[g_1(\bar{c}, q)]^2 + 8BN^2l^2g_2(q)}}{2BN^2l^2} \tag{13}$$

where $g_1(\bar{c}) = 2Nl\bar{c} + 2ANl - 2BN^2l^2 - 2kNql \geq 0$ and $g_2(q) = 2ANl - BN^2l^2 - 2kqN - 2IN \geq 0$ as we assume $Al - BNl^2/2 \geq I + kq$ to assure farming income is enough to support the whole family.

The change of m_{un} and m_{con} with respect to $\bar{c}$ to q is immediate by taking derivatives, which is summarized by Proposition 1 and 2.

C Additional Results

Table C1: PARP Effect on Household Farmland

Dependent variable: household farmland area	
Household labor force share	0.004
	(0.010)
Village FE	✓
Year FE	✓
Village-specific time trend	✓
Observations	226,897
R-squared	0.703

Notes: This table reports the change of household farmland area using the DID specification. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Table C2: Correlations between Migrant Work and Household Education

Panel A: dependent variable: wage income per capita (log)	
Household illiterate members share	0.007
	(0.009)
Observations	97,263
R-squared	0.657
Panel B: dependent variable: length of migrant work per year	
Household illiterate members share	0.009
	(0.012)
Observations	97,178
R-squared	0.668
Living conditions	✓
Year FE	✓
Household FE	✓
Village-specific time trend	✓

Notes: This table reports the coefficients from regressions of migrant work activities on share of illiterate members of each household. We use only pre-treatment samples only to present the relations between household education levels and migrant work without the impact of PARP. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Table C3: Household labor characteristics, borrowing behavior, and social networks

	Baseline	Borrowing	Village migrant working share	Village same destination share
	(1)	(2)	(3)	(4)
Dependent variable: average length of migrant work per year				
Household labor force share	1.420***	1.391***	1.284***	1.230***
	(0.207)	(0.210)	(0.225)	(0.225)
Household borrowing indicator		0.060		
		(0.045)		
Village migrant work share			0.087*	
			(0.049)	
Share working in same province				0.094*
				(0.054)
Year FE	✓	✓	✓	✓
Household FE	✓	✓	✓	✓
Village-specific time trend	✓	✓	✓	✓
Observations	96,899	52,138	96,899	96,899
R-squared	0.18	0.22	0.21	0.20

Notes: Column (1) is the most left orange bar in Figure 7. Borrowing indicator is defined as whether the household has any borrowing behavior from any other households or from financial institutions. Village migrant work share is defined as the proportion of households in the same village that engage in migrant work. Share working in the same province is the proportion of migrants in the same village who migrate to the same destination province as household i. We only use the sample in pre-treatment periods. The sample size in Column (2) is smaller due to the missing observations for the borrowing indicator. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

D Propensity Score Matching

Table D1: Determinants of resettlement

Dependent variable:	Resettled
Average age	-0.021*
	(0.011)
Household size	0.085***
	(0.018)
Labor force number	0.132***
	(0.025)
Unhealthy number	0.097**
	(0.041)
Illiteracy number	0.076**
	(0.034)
Number of children	0.054**
	(0.023)
Number of seniors	0.118***
	(0.036)
Arable land per capita	-0.287*
	(0.152)
Observations	25,789
Pseudo R-squared	0.18

Notes: The dependent variable is an indicator equal to one if the household is resettled, which is a time-invariant treatment indicator. All covariates are measured using characteristics in 2017. * : $p < 0.1$; ** : $p < 0.05$; *** : $p < 0.01$. Standard errors are clustered at the village level.

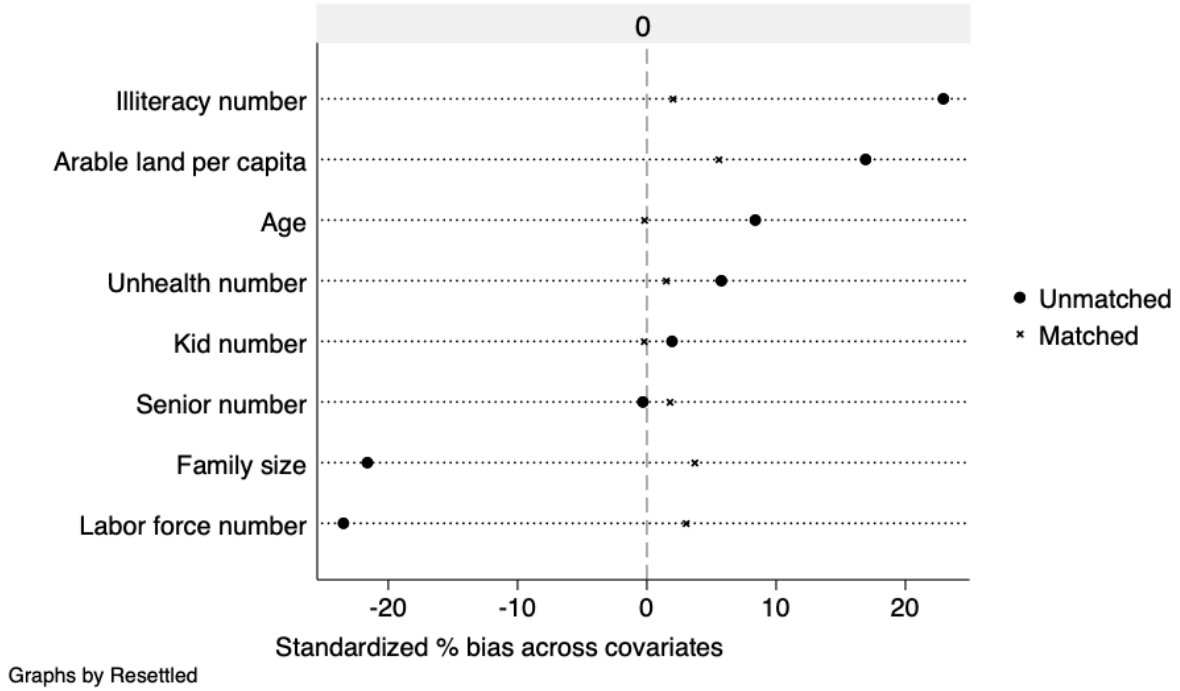


Figure D1: PSM bias reduction

Note: This figure reports the bias reduction between unmatched and matched groups using PSM for DID estimation. We apply observations in 2017 only and use 5:1 matching.

E Alternative Regression Specifications

Table E1: Treatment effects of PARP on income: alternative specifications

	With controls	ITT	Drop eligible but not resettled	Use eligible households only
	(1)	(2)	(3)	(4)
Panel A: dependent variable: total income per capita (log)				
$Eligible_{it}$		0.11***		
		(0.02)		
$Resettled_{it}$	0.12***		0.13***	0.12**
	(0.03)		(0.03)	(0.05)
R-squared	0.80	0.74	0.75	0.71
N	226,403	227,144	226,508	8,432
Panel B: dependent variable: wage income per capita (log)				
$Eligible_{it}$		0.35***		
		(0.08)		
$Resettled_{it}$	0.37***		0.38***	0.36**
	(0.08)		(0.08)	(0.15)
R-squared	0.85	0.80	0.81	0.77
N	227,825	228,793	228,142	8,615
Panel C: dependent variable: farming income per capita (log)				
$Eligible_{it}$		-0.04		
		(0.08)		
$Resettled_{it}$	-0.06		-0.06	-0.05
	(0.08)		(0.08)	(0.12)
R-squared	0.84	0.79	0.79	0.75
N	226,934	227,574	226,921	8,507
Panel D: dependent variable: transfer income per capita (log)				
$Eligible_{it}$		-0.16***		
		(0.05)		
$Resettled_{it}$	-0.16***		-0.17***	-0.16**
	(0.05)		(0.05)	(0.08)
R-squared	0.86	0.80	0.81	0.77
N	226,528	227,274	226,628	8,421
Year FE	✓	✓	✓	✓
Household FE	✓	✓	✓	✓
Village-specific time trend	✓	✓	✓	✓
Add control variables	✓	×	×	×

Note: Column (1) augments the baseline specification by including time-varying household controls used for PSM. Column (2) reports the ITT estimates by replacing the treatment indicator with eligibility status. Column (3) uses the resettlement indicator but excludes eligible households that were not resettled. Column (4) uses the resettlement indicator and restricts the sample to eligible households only. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Table E2: Treatment effects of PARP on working activities: alternative specifications

	With controls	ITT	Drop eligible but not resettled	Use eligible households only
	(1)	(2)	(3)	(4)
Panel A: dependent variable: average length of urban migrant work per year				
$Eligible_{it}$		0.76***		
		(0.15)		
$Resettled_{it}$	0.79***		0.80***	0.79**
	(0.16)		(0.16)	(0.30)
R-squared	0.85	0.80	0.81	0.78
N	226,834	227,198	226,542	8,517
Panel B: dependent variable: distance between home and work province (log)				
$Eligible_{it}$		0.10***		
		(0.03)		
$Resettled_{it}$	0.11***		0.12***	0.11**
	(0.03)		(0.03)	(0.06)
R-squared	0.86	0.80	0.81	0.77
N	108,635	109,197	108,864	4,612
Panel C: dependent variable: GDP per capita of migrant work destination province (log)				
$Eligible_{it}$		0.09***		
		(0.02)		
$Resettled_{it}$	0.09***		0.10***	0.09**
	(0.02)		(0.02)	(0.04)
R-squared	0.85	0.80	0.81	0.78
N	108,925	109,634	109,287	4,731
Panel D: dependent variable: farming expenditures per capita (log)				
$Eligible_{it}$		-0.11*		
		(0.07)		
$Resettled_{it}$	-0.12*		-0.13*	-0.12
	(0.07)		(0.07)	(0.10)
R-squared	0.85	0.80	0.81	0.77
N	226,835	227,249	226,603	8,436
Year FE	✓	✓	✓	✓
Household FE	✓	✓	✓	✓
Village-specific time trend	✓	✓	✓	✓
Add control variables	✓	×	×	×

Note: Column (1) augments the baseline specification by including time-varying household controls used for PSM. Column (2) reports the ITT estimates by replacing the treatment indicator with eligibility status. Column (3) uses the resettlement indicator but excludes eligible households that were not resettled. Column (4) uses the resettlement indicator and restricts the sample to eligible households only. In Panel B and C, we exclude household-year observations with zero urban migrant work months. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Table E3: Change of migration cost and living conditions after PARP: alternative specifications

	With controls	
	(1)	(2)
Dependent variable:	Access to tap water	Access to internet
$Resettled_{it}$	0.53***	0.15***
	(0.13)	(0.03)
R-squared	0.28	0.27
N	225,842	225,311
Dependent variable:	Dist. to urban center	Dist. to primary school
$Resettled_{it}$	-10.32***	-2.40***
	(2.58)	(0.72)
R-squared	0.71	0.78
N	225,129	225,129
Dependent variable:	Dist. to middle school	Dist. to high school
$Resettled_{it}$	-2.55***	-10.52***
	(0.66)	(2.44)
R-squared	0.63	0.66
N	225,129	225,129
Dependent variable:	Dist. to clinic	Dist. to hospital
$Resettled_{it}$	-3.78***	-2.30***
	(0.81)	(0.68)
R-squared	0.77	0.72
N	225,129	225,129
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓
Add control variables	✓	✓

Note: This table reports estimates from the baseline specification augmented with time-varying household controls used for PSM. Each column corresponds to a different outcome variable. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Table E4: Change of migration cost and living conditions after PARP : alternative specifications

	ITT	Drop eligible but not resettled	Use eligible households only	ITT	Drop eligible but not resettled	Use eligible households only
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:		Access to tap water			Access to internet	
$Eligible_{it}$	0.50***			0.13***		
	(0.13)			(0.03)		
$Resettled_{it}$		0.54***	0.52**		0.16***	0.15**
		(0.13)	(0.21)		(0.03)	(0.06)
R-squared	0.23	0.24	0.21	0.22	0.23	0.20
N	227,065	226,418	8,401	226,536	225,901	8,356
Dependent variable:		Dist. to urban center			Dist. to primary school	
$Eligible_{it}$	-9.88***			-2.23***		
	(2.74)			(0.77)		
$Resettled_{it}$		-10.47***	-10.21**		-2.44***	-2.36**
		(2.61)	(4.18)		(0.73)	(1.12)
R-squared	0.66	0.67	0.63	0.74	0.75	0.71
N	227,365	226,711	8,462	227,365	226,711	8,462
Dependent variable:		Dist. to middle school			Dist. to high school	
$Eligible_{it}$	-2.38***			-10.03***		
	(0.71)			(2.63)		
$Resettled_{it}$		-2.60***	-2.48**		-10.68***	-10.31**
		(0.68)	(1.09)		(2.48)	(4.05)
R-squared	0.58	0.59	0.56	0.61	0.62	0.59
N	227,365	226,711	8,462	227,365	226,711	8,462
Dependent variable:		Dist. to clinic			Dist. to hospital	
$Eligible_{it}$	-3.55***			-2.12***		
	(0.89)			(0.74)		
$Resettled_{it}$		-3.86***	-3.70**		-2.35***	-2.24**
		(0.83)	(1.42)		(0.70)	(1.05)
R-squared	0.73	0.74	0.70	0.68	0.69	0.65
N	227,365	226,711	8,462	227,365	226,711	8,462
Year FE	✓	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓	✓
Village-specific time trend	✓	✓	✓	✓	✓	✓
Add control variables	×	×	×	×	×	×

Note: Column (1) and (4) report the ITT estimate using eligibility status, Column (2) and (5) use the resettlement indicator after dropping eligible households that were not resettled, and Column (3) and (6) use the resettlement indicator while restricting the sample to eligible households only. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

F Divergence in Poverty Alleviation

The main findings show empirical evidence of the two channels influencing migrant worker decisions after resettlement through the heterogeneous analysis. The results might be discouraging, as they reveal a decline in family income levels after resettlement among households with fewer available family labor. Such divergence in poverty alleviation is further illustrated by comparing the income levels of resettled households with those of unresettled households living near the resettlement destinations.

Since the ten resettlement destinations are located across ten different sites, we repeat the following regression for each site j:

$$Y_{ijt} = \beta_j^D \times Resettled_{ij} + \beta_j^L \times Resettled_{ij} \times L_{ijt} + \theta_t + \psi_v \times t + \varepsilon_{ijt} \tag{14}$$

where L_{it}^j denotes household labor characteristics. For each site j, we restrict the sample to post-2018 observations and households located within 15 km of the destination. Accordingly, $D_{it}^j = 1$ for households resettled to site j, and $D_{it}^j = 0$ for non-resettled households within the same radius. We focus on the post-treatment sample (after 2018), controlling for year fixed effects and time trends but omitting household fixed effects since $Resettled_{ij}$ is time-invariant. We further apply PSM to construct matched pairs. Appendix Figure F2) shows the balanced comparison pairs after matching. Importantly, this specification does not estimate treatment effects; instead, it compares post-treatment differences between similar resettled and unresettled households to illustrate how the two channels generate unequal outcomes through labor reallocation.

The estimation results are presented in Appendix Table F1, while Appendix Figure F1 illustrates the predicted values of wage income per capita for both resettled and unresettled households in regression (14) across sites. In Appendix Figure F1, resettled households experience significantly lower wage income than their unresettled counterparts living nearby after resettlement. However, this income gap narrows among households with a larger labor force, which presents the divergence in PARP's effects on poverty alleviation.

Although PARP aims to promote income generation by facilitating migrant labor, some households fail to benefit or even experience a decline in earnings. More importantly, the most impov-

erished households, often with disadvantaged labor characteristics, are the ones most negatively affected after resettlement. This divergence in poverty alleviation likely arises from improved living conditions, which simultaneously enhance home enjoyment while increasing financial pressure due to higher living costs. In this sense, PARP proves ineffective in addressing poverty among households with limited labor force as they struggle to generate higher income from urban sectors.

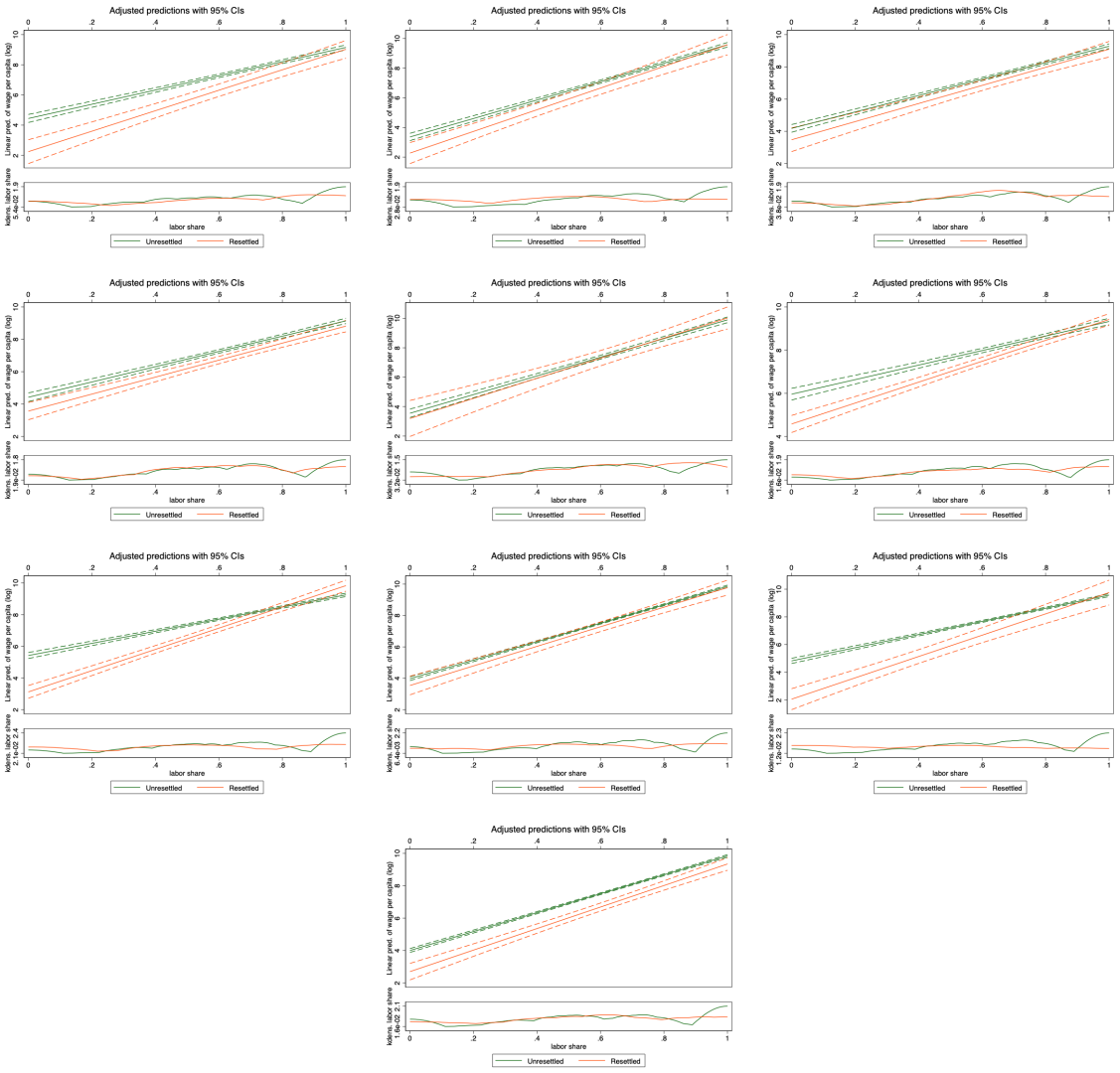


Figure F1: Comparison of wage income per capita between resettled and local households

Note: This figure uses observations from 2018 onward and applies PSM to construct matched household pairs for the regression. Each subfigure corresponds to a resettlement site that accommodates resettled households.

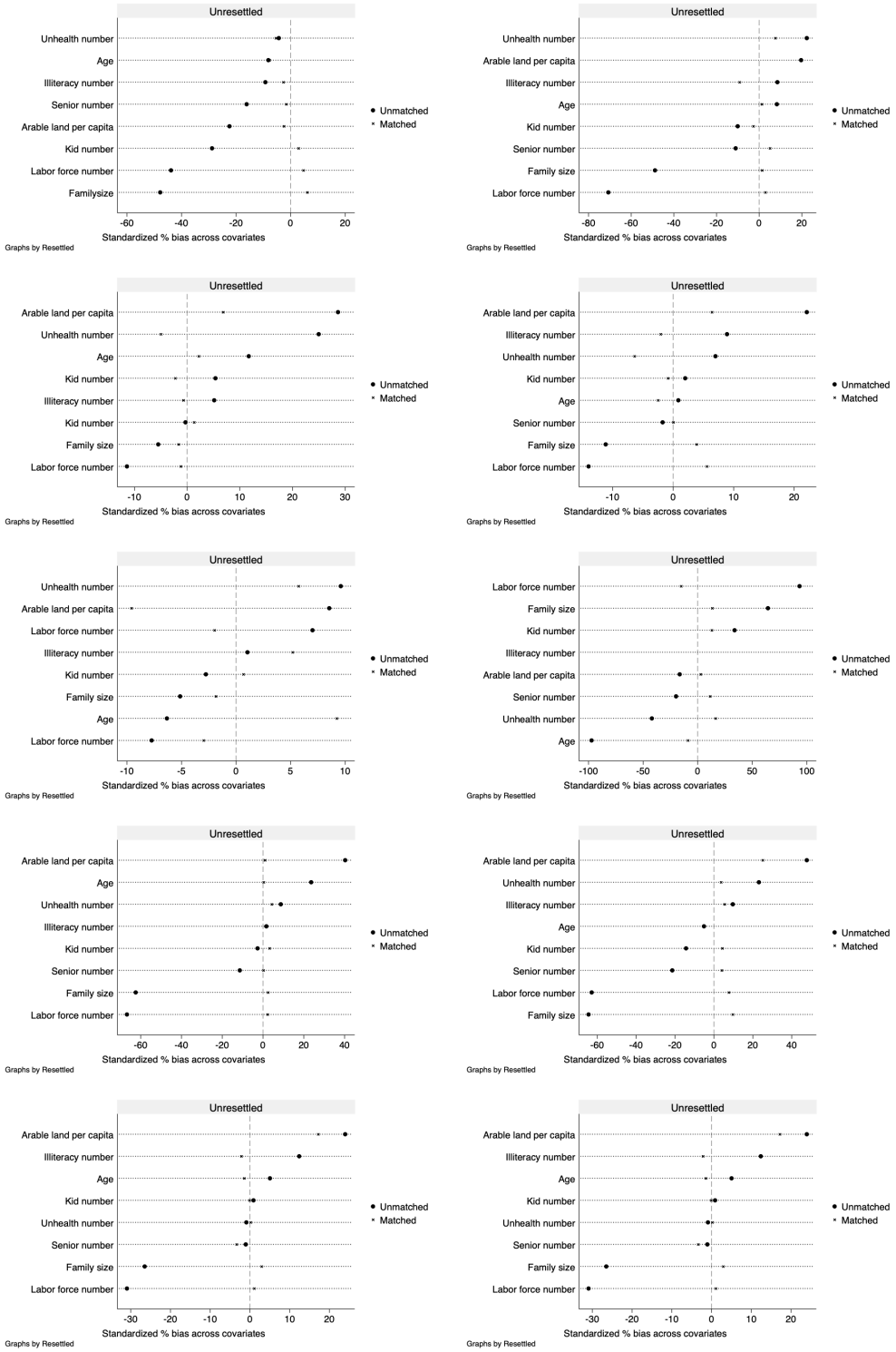


Figure F2: PSM bias reduction

Note: This figure reports the bias reduction between unmatched and matched groups using PSM for comparison of households around resettlement sites. We apply observations in 2018 only and use 5:1 matching.

Table F1: Wage income comparison after resettlement

$Site_j$	$Resettled_{it}^j$ (1)	$Resettled_{it}^j L_{it}^j$ (2)	Observation
Dependent variable: wage income per capita			
1	-0.57***	0.71***	711
	(0.09)	(0.11)	
2	-0.49***	0.67***	628
	(0.07)	(0.11)	
3	-0.45**	0.64***	744
	(0.15)	(0.11)	
4	-0.33***	0.19***	418
	(0.04)	(0.01)	
5	-0.37***	0.51***	878
	(0.09)	(0.09)	
6	-0.40	0.62	347
	(0.28)	(0.27)	
7	-0.30***	0.46***	792
	(0.08)	(0.04)	
8	-0.42***	0.65***	1,210
	(0.07)	(0.08)	
9	-0.37***	0.61***	1,564
	(0.08)	(0.11)	
10	-0.60***	0.80***	1,065
	(0.08)	(0.06)	
Year FE		✓	
Household FE		×	
Village-specific time trend		✓	

Note: This table reports the estimations of equation (14). The first column is the site number. Column (1) reports β_j^D for all the 10 sites. Column (2) reports β_j^L for sites. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

G Robustness Checks

G.1 Missing Variables

Table G1: Missing rate and descriptive statistics comparison

Variables	Missing rate (%)		Diff. between full sample and reduced sample in 2017	
	Full sample	Reduced sample	Control group	Treatment group
Total income	1.958	0	132.34*	127.71*
Wage income	0.223	0	71.79*	37.06
Farming income	0.748	0	12.47	28.10
Transfer income	0.014	0	48.08*	62.55
Length of migr. working	2.823	0	-0.12*	-0.16
Farming expenditures	0.674	0	4.56*	6.52
Destination of migrant work	1.02	0	N/A	N/A
Average household age	1.049	0.001	6.44***	6.22***
Household size	0	0	-0.14***	-0.20***
Num. of kids	0.972	0	-0.05	-0.07
Num. of seniors	0.972	0	0.00	0.02
Num. of labor force	0.972	0	0.00	-0.04
Num. of illiteracy	0.972	0	-0.00	-0.01
Num. of unhealthy members	0.972	0	0.01*	-0.01
Tap water	0.721	0.025	0.01	0.00
Stable electricity	0.012	0	0.00	-0.02
Distance to closest primary school	0	0	0	0
Distance to closest middle school	0	0	0	0
Distance to closest high school	0	0	0	0
Distance to closest clinic	0	0	0	0
Distance to closet hospital	0	0	0	0
Distance to closet urban center	0	0	0	0
Number of observations	232,229	165,313		

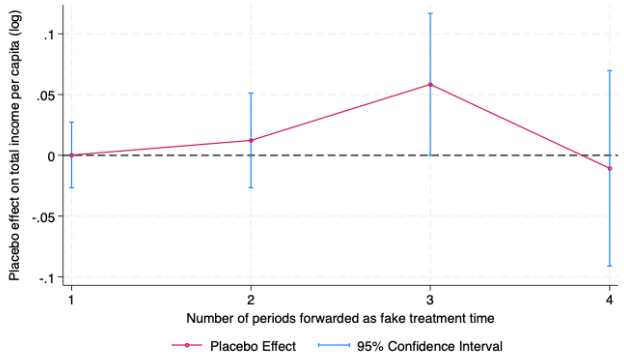
Note: This table reports the missing rate of observations for certain variables. The column under "full sample" represents the sample involves all the observations. The column under "reduced sample" corresponds to the sample by dropping households with missing total income, wage income, farming income, transfer income, total length of migrant work, farming expenditure, or destination of migrant work. The columns under control group and treatment group show the difference between full sample and reduced sample for each group, respectively. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$ for t-test of group difference.

Table G2: Treatment effects of PARP with reduced sample

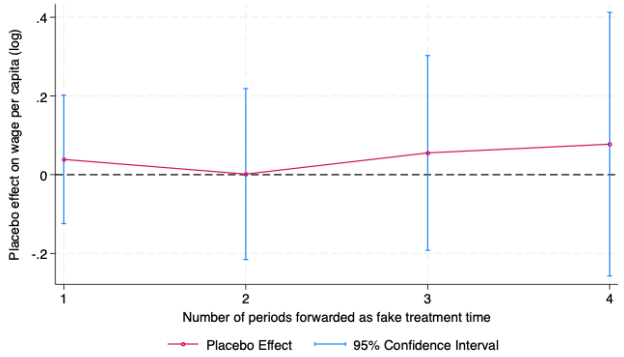
	Baseline DID (1)	PSM-DID (2)
Panel A: dependent variable: total income per capita (log)		
$Resettled_{it}$	0.12***	0.13***
	(0.04)	(0.04)
R-squared	0.86	0.86
Panel B: dependent variable: wage income per capita (log)		
$Resettled_{it}$	0.38***	0.46***
	(0.10)	(0.09)
R-squared	0.79	0.80
Panel C: dependent variable: farming income per capita (log)		
$Resettled_{it}$	-0.05	-0.01
	(0.05)	(0.01)
R-squared	0.81	0.86
Panel D: dependent variable: transfer income per capita (log)		
$Resettled_{it}$	-0.19***	-0.25***
	(0.03)	(0.05)
R-squared	0.82	0.83
Panel E: dependent variable: average length of urban migrant work per year		
$Resettled_{it}$	0.69***	0.82***
	(0.17)	(0.07)
R-squared	0.83	0.83
Panel F: dependent variable: distance between home and work province (log)		
$Resettled_{it}$	0.12***	0.16***
	(0.02)	(0.05)
R-squared	0.78	0.79
Panel G: dependent variable: GDP per capita of migrant work destination province (log)		
$Resettled_{it}$	0.12***	0.16***
	(0.03)	(0.05)
R-squared	0.79	0.83
Panel H: dependent variable: farming expenditures per capita (log)		
$Resettled_{it}$	-0.14*	0.02
	(0.09)	(0.02)
R-squared	0.73	0.82
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓
N	165,226	40,960

Note: This table repeats the estimation of PARP effects on income and working activities using DID and PSM-DID on the reduced sample, which excludes households without observed values for total income, wage income, farming income, transfer income, total length of migrant work, farming investment, or destination of migrant work. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

G.2 Anticipation



(a) Placebo test on wage income per capita (log)



(b) Placebo test on farming income per capita (log)

Figure G1: Placebo test by forwarding treatment timing

Note: This figure reports the placebo test results by manually forwarding treatment timing to 2017, 2016, 2015, or 2014, respectively. The points estimates and 5% confidence intervals are drawn.

G.3 Spillovers from Surrounding Counties

Table G3: Spillover effects from surrounding counties on income: as control

	5km	10km
	(1)	(2)
Panel A: dependent variable: total income per capita (log)		
$Resettled_{it}$	0.12***	0.12***
	(0.02)	(0.02)
$Resettled_{it} \times 1(\text{Dist} \leq 5\text{km})$	-0.01	
	(0.03)	
$Resettled_{it} \times 1(\text{Dist} \leq 10\text{km})$		-0.02
		(0.02)
R-squared	0.75	0.75
N	227,144	227,144
Panel B: dependent variable: wage income per capita (log)		
$Resettled_{it}$	0.37***	0.36***
	(0.08)	(0.08)
$Resettled_{it} \times 1(\text{Dist} \leq 5\text{km})$	0.04	
	(0.11)	
$Resettled_{it} \times 1(\text{Dist} \leq 10\text{km})$		0.03
		(0.09)
R-squared	0.81	0.81
N	228,793	228,793
Panel C: dependent variable: farming income per capita (log)		
$Resettled_{it}$	-0.05	-0.05
	(0.08)	(0.08)
$Resettled_{it} \times 1(\text{Dist} \leq 5\text{km})$	0.02	
	(0.10)	
$Resettled_{it} \times 1(\text{Dist} \leq 10\text{km})$		0.01
		(0.09)
R-squared	0.79	0.79
N	227,574	227,574
Panel D: dependent variable: transfer income per capita (log)		
$Resettled_{it}$	-0.16***	-0.15***
	(0.05)	(0.05)
$Resettled_{it} \times 1(\text{Dist} \leq 5\text{km})$	-0.03	
	(0.06)	
$Resettled_{it} \times 1(\text{Dist} \leq 10\text{km})$		-0.02
		(0.05)
R-squared	0.81	0.81
N	227,274	227,274
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓

Note: Column (1) uses an indicator for households located within 5 km of the county border. Column (2) uses an indicator for households located within 10 km. The interaction terms capture whether treatment effects differ for households more exposed to potential spillovers from surrounding counties. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Table G4: Spillover effects from surrounding counties on working activities: as control

	5km	10km
	(1)	(2)
Panel A: dependent variable: average length of urban migrant work per year		
$Resettled_{it}$	0.78***	0.77***
	(0.15)	(0.15)
$Resettled_{it} \times 1(\text{Dist} \leq 5\text{km})$	0.06	
	(0.20)	
$Resettled_{it} \times 1(\text{Dist} \leq 10\text{km})$		0.08
		(0.18)
R-squared	0.81	0.81
N	227,198	227,198
Panel B: dependent variable: distance between home and work province (log)		
$Resettled_{it}$	0.11***	0.11***
	(0.03)	(0.03)
$Resettled_{it} \times 1(\text{Dist} \leq 5\text{km})$	0.01	
	(0.04)	
$Resettled_{it} \times 1(\text{Dist} \leq 10\text{km})$		0.00
		(0.04)
R-squared	0.81	0.81
N	109,197	109,197
Panel C: dependent variable: GDP per capita of migrant work destination province (log)		
$Resettled_{it}$	0.10***	0.10***
	(0.02)	(0.02)
$Resettled_{it} \times 1(\text{Dist} \leq 5\text{km})$	0.02	
	(0.03)	
$Resettled_{it} \times 1(\text{Dist} \leq 10\text{km})$		0.01
		(0.03)
R-squared	0.81	0.81
N	109,634	109,634
Panel D: dependent variable: farming expenditures per capita (log)		
$Resettled_{it}$	-0.12*	-0.12*
	(0.07)	(0.07)
$Resettled_{it} \times 1(\text{Dist} \leq 5\text{km})$	-0.03	
	(0.09)	
$Resettled_{it} \times 1(\text{Dist} \leq 10\text{km})$		-0.04
		(0.08)
R-squared	0.81	0.81
N	227,249	227,249
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓

Note: Column (1) uses an indicator for households located within 5 km of the county border, while Column (2) uses an indicator for households located within 10 km. The interaction terms capture whether treatment effects differ for households more exposed to potential spillovers from surrounding counties. In Panel B and C, the sample excludes households with zero urban working months. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Table G5: Spillover effects from surrounding counties on income: drop

	Drop within 5km (1)	Drop within 10km (2)
Panel A: dependent variable: total income per capita (log)		
$Resettled_{it}$	0.12***	0.11***
	(0.02)	(0.02)
R-squared	0.75	0.75
N	214,032	158,417
Panel B: dependent variable: wage income per capita (log)		
$Resettled_{it}$	0.36***	0.35***
	(0.08)	(0.08)
R-squared	0.81	0.81
N	215,488	159,926
Panel C: dependent variable: farming income per capita (log)		
$Resettled_{it}$	-0.05	-0.04
	(0.08)	(0.08)
R-squared	0.79	0.79
N	214,276	158,763
Panel D: dependent variable: transfer income per capita (log)		
$Resettled_{it}$	-0.15***	-0.15***
	(0.05)	(0.05)
R-squared	0.81	0.81
N	213,991	158,506
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓

Note: Column (1) excludes households located within 5 km of the county border, while Column (2) excludes households located within 10 km of the county border. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Table G6: Spillover effects from surrounding counties on working activities: drop

	Drop within 5km (1)	Drop within 10km (2)
Panel A: dependent variable: average length of urban migrant work per year		
$Resettled_{it}$	0.77***	0.76***
	(0.15)	(0.15)
R-squared	0.81	0.81
N	214,074	198,452
Panel B: dependent variable: distance between home and work province (log)		
$Resettled_{it}$	0.11***	0.10***
	(0.03)	(0.03)
R-squared	0.81	0.81
N	102,618	94,331
Panel C: dependent variable: GDP per capita of migrant work destination province (log)		
$Resettled_{it}$	0.10***	0.09***
	(0.02)	(0.02)
R-squared	0.81	0.81
N	103,044	94,706
Panel D: dependent variable: farming expenditures per capita (log)		
$Resettled_{it}$	-0.12*	-0.11*
	(0.07)	(0.07)
R-squared	0.81	0.81
N	214,118	198,501
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓

Note: Column (1) excludes households located within 5 km of the county border, while Column (2) excludes households located within 10 km of the county border. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

G.4 Confounding Effects from Past Resettlement

Table G7: Confounding effects from earlier resettlement programs on income: as control

	5km	10km
	(1)	(2)
Panel A: dependent variable: total income per capita (log)		
$Resettled_{it}$	0.12***	0.12***
	(0.02)	(0.02)
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 5km)	0.01	
	(0.03)	
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 10km)		0.00
		(0.03)
R-squared	0.75	0.75
N	227,144	227,144
Panel B: dependent variable: wage income per capita (log)		
$Resettled_{it}$	0.37***	0.36***
	(0.08)	(0.08)
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 5km)	0.05	
	(0.10)	
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 10km)		0.04
		(0.09)
R-squared	0.81	0.81
N	228,793	228,793
Panel C: dependent variable: farming income per capita (log)		
$Resettled_{it}$	-0.05	-0.05
	(0.08)	(0.08)
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 5km)	-0.02	
	(0.10)	
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 10km)		-0.01
		(0.09)
R-squared	0.79	0.79
N	227,574	227,574
Panel D: dependent variable: transfer income per capita (log)		
$Resettled_{it}$	-0.16***	-0.16***
	(0.05)	(0.05)
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 5km)	0.02	
	(0.06)	
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 10km)		0.01
		(0.05)
R-squared	0.81	0.81
N	227,274	227,274
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓

Note: Column (1) uses an indicator for households located within 5 km of mountainous areas. Column (2) uses the analogous indicator for households located within 10 km of mountainous areas. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Table G8: Confounding effects from earlier resettlement programs on working activities: as control

	5km	10km
	(1)	(2)
Panel A: dependent variable: average length of urban migrant work per year		
$Resettled_{it}$	0.78***	0.77***
	(0.15)	(0.15)
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 5km)	0.04	
	(0.19)	
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 10km)		0.05
		(0.17)
R-squared	0.81	0.81
N	227,198	227,198
Panel B: dependent variable: distance between home and work province (log)		
$Resettled_{it}$	0.11***	0.11***
	(0.03)	(0.03)
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 5km)	0.00	
	(0.04)	
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 10km)		0.01
		(0.04)
R-squared	0.81	0.81
N	109,197	109,197
Panel C: dependent variable: GDP per capita of migrant work destination province (log)		
$Resettled_{it}$	0.10***	0.10***
	(0.02)	(0.02)
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 5km)	0.01	
	(0.03)	
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 10km)		0.01
		(0.03)
R-squared	0.81	0.81
N	109,634	109,634
Panel D: dependent variable: farming expenditures per capita (log)		
$Resettled_{it}$	-0.12*	-0.12*
	(0.07)	(0.07)
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 5km)	-0.02	
	(0.08)	
$Resettled_{it} \times 1$(Dist. to mountain $\leq$ 10km)		-0.03
		(0.08)
R-squared	0.81	0.81
N	227,249	227,249
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓

Note: Column (1) uses an indicator for households located within 5 km of mountainous areas. Column (2) uses the analogous indicator for households located within 10 km of mountainous areas. In Panel B and C, the sample excludes households with zero urban working months. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Table G9: Confounding effects from earlier resettlement programs on income: drop

	Drop within 5km (1)	Drop within 10km (2)
Panel A: dependent variable: total income per capita (log)		
$Resettled_{it}$	0.11***	0.11***
	(0.02)	(0.02)
R-squared	0.75	0.75
N	213,845	197,962
Panel B: dependent variable: wage income per capita (log)		
$Resettled_{it}$	0.35***	0.34***
	(0.08)	(0.08)
R-squared	0.81	0.81
N	215,206	199,433
Panel C: dependent variable: farming income per capita (log)		
$Resettled_{it}$	-0.04	-0.04
	(0.08)	(0.08)
R-squared	0.79	0.79
N	214,061	198,328
Panel D: dependent variable: transfer income per capita (log)		
$Resettled_{it}$	-0.15***	-0.14***
	(0.05)	(0.05)
R-squared	0.81	0.81
N	213,772	198,107
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓

Note: Column (1) excludes households located within 5 km of mountainous areas. Column (2) excludes households located within 10 km. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

Table G10: Confounding effects from earlier resettlement programs on working activities: drop

	Drop within 5km (1)	Drop within 10km (2)
Panel A: dependent variable: average length of urban migrant work per year		
$Resettled_{it}$	0.76***	0.75***
	(0.15)	(0.15)
R-squared	0.81	0.81
N	213,889	197,994
Panel B: dependent variable: distance between home and work province (log)		
$Resettled_{it}$	0.10***	0.10***
	(0.03)	(0.03)
R-squared	0.81	0.81
N	102,407	93,872
Panel C: dependent variable: GDP per capita of migrant work destination province (log)		
$Resettled_{it}$	0.09***	0.09***
	(0.02)	(0.02)
R-squared	0.81	0.81
N	102,836	94,241
Panel D: dependent variable: farming expenditures per capita (log)		
$Resettled_{it}$	-0.11*	-0.11*
	(0.07)	(0.07)
R-squared	0.81	0.81
N	213,931	198,051
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓

Note: Column (1) excludes households located within 5 km of mountainous areas. Column (2) excludes households located within 10 km. Both columns estimate the baseline DID specification on the restricted samples. In Panels B and C, we further exclude households with zero urban working months. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.

G.5 Confounding Anti-Poverty Policies

Table G11: Household overlap with confounding anti-poverty policies

Year	Industry	Credit	Overlap
2013	0.0000	0.0000	0.0000
2014	0.0000	0.0000	0.0000
2015	0.0000	0.0000	0.0000
2016	0.0000	0.0000	0.0000
2017	0.0193	0.0000	0.0021
2018	0.0269	0.0425	0.0052
2019	0.0328	0.0507	0.0091

Note: This table reports the yearly share of households participating in concurrent anti-poverty policies, including industrial support and credit support programs. The overlap column indicates the share of households jointly covered by at least one of these policies.

Table G12: Confounding anti-poverty policies and income outcomes

	As controls	Drop sample
	(1)	(2)
Panel A: dependent variable: total income per capita (log)		
$Resettled_{it}$	0.12***	0.11***
	(0.02)	(0.02)
Credit $support_{it}$	0.03	
	(0.04)	
Industrial $support_{it}$	0.05*	
	(0.03)	
R-squared	0.76	0.75
N	227,144	213,902
Panel B: dependent variable: wage income per capita (log)		
$Resettled_{it}$	0.36***	0.35***
	(0.08)	(0.08)
Credit $support_{it}$	0.02	
	(0.06)	
Industrial $support_{it}$	0.08*	
	(0.05)	
R-squared	0.81	0.81
N	228,793	215,337
Panel C: dependent variable: farming income per capita (log)		
$Resettled_{it}$	-0.05	-0.04
	(0.08)	(0.08)
Credit $support_{it}$	0.01	
	(0.06)	
Industrial $support_{it}$	0.04	
	(0.05)	
R-squared	0.79	0.79
N	227,574	214,112
Panel D: dependent variable: transfer income per capita (log)		
$Resettled_{it}$	-0.15***	-0.15***
	(0.05)	(0.05)
Credit $support_{it}$	0.06*	
	(0.04)	
Industrial $support_{it}$	0.02	
	(0.03)	
R-squared	0.82	0.81
N	227,274	213,845
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓
Policy controls	✓	×

Note: Column (1) augments the baseline DID specification by including indicators for participation in concurrent anti-poverty policies, including financial support and industrial support programs. Column (2) excludes households participating in these policies. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. * : $p < 0.1$; ** : $p < 0.05$; *** : $p < 0.01$. Standard errors are clustered at the village level.

Table G13: Confounding anti-poverty policies and working activities

	As controls	Drop sample
	(1)	(2)
Panel A: dependent variable: average length of urban migrant work per year		
$Resettled_{it}$	0.77***	0.76***
	(0.15)	(0.15)
Credit $support_{it}$	0.05	
	(0.07)	
Industrial $support_{it}$	0.09*	
	(0.05)	
R-squared	0.82	0.81
N	227,198	213,956
Panel B: dependent variable: distance between home and work province (log)		
$Resettled_{it}$	0.11***	0.10***
	(0.03)	(0.03)
Credit $support_{it}$	0.01	
	(0.04)	
Industrial $support_{it}$	0.03	
	(0.03)	
R-squared	0.81	0.81
N	109,197	101,884
Panel C: dependent variable: GDP per capita of migrant work destination province (log)		
$Resettled_{it}$	0.10***	0.09***
	(0.02)	(0.02)
Credit $support_{it}$	0.01	
	(0.03)	
Industrial $support_{it}$	0.02	
	(0.03)	
R-squared	0.81	0.81
N	109,634	102,213
Panel D: dependent variable: farming expenditures per capita (log)		
$Resettled_{it}$	-0.12*	-0.11*
	(0.07)	(0.07)
Credit $support_{it}$	0.02	
	(0.05)	
Industrial $support_{it}$	0.04	
	(0.04)	
R-squared	0.82	0.81
N	227,249	214,018
Year FE	✓	✓
Household FE	✓	✓
Village-specific time trend	✓	✓
Policy controls	✓	×

Note: Column (1) augments the baseline DID specification by including indicators for participation in concurrent anti-poverty policies, including financial support and industrial support programs. Column (2) excludes households participating in these policies. The difference in the number of observations arises from zero weights assigned to some observations due to missing outcome variables for certain years and households. $^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Standard errors are clustered at the village level.