NBER WORKING PAPER SERIES

SOCIAL INSURANCE AND MIGRATION:
EVIDENCE FROM A NATION-WIDE INSTITUTIONAL REFORM IN CHINA

Fanghua Li
Chenyang Ji
Moshe Buchinsky

Working Paper 31819
http://www.nber.org/papers/w31819

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 2023

We thank Michael Keane, Steve Berry, Jean-Marc Robin, John Rust, Maria Guadalupe, Benjamin Marx, Xin Meng, Simon Grant, Bob Gregory, and Jane Zhang for their invaluable comments and suggestions. We also thank all conference and seminar participants at the 2021 AASLE, 3rd China Regional Economist Forum, 19th Quarterly Meeting of China Labor Economists Forum, Peking Economics and National School of Development, UNSW Economics, Georgetown University, INSEAD, Sciences Po and ANU Economics. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Social Insurance and Migration: Evidence from a Nation-Wide Institutional Reform in China
Fanghua Li, Chenyang Ji, and Moshe Buchinsky
NBER Working Paper No. 31819
October 2023
JEL No. J01,O10

ABSTRACT

In this paper we examine the causal relationship between formal social insurance and individuals' migration decisions. We exploit a quasi experimental design in rural China, under which county officials were assigned to a group of villages (i.e., treated villages) to serve as village supervisors (VSs) for the local leaders. We show that this led to reduced favoritism in welfare allocation by the local leaders, thereby increasing the efficacy in the formal social insurance in the treated villages. We use detailed geo-referenced administrative household-level data suited for a spatial regression discontinuity design (RDD) to obtain an average treatment effect (LATE) of the improved social insurance on migration. The apparent variation in the implementation of the reform across treated villages and heterogeneous impacts on different family clans make it possible to directly link changes in the efficacy of the insurance to migration choices. We find a large positive migration effect, of about 19%, for the young males and females. In turn, this led to a large boost in the average household's income in just two years.

Fanghua Li
Department of Economics
University of New South Wales
Sydney
New South Wales
Australia
fanghua.li@unsw.edu.au

Chenyang Ji
Renmin University of China
59 Zhongguancun St
Beijing 100086
China
jichenyang@ruc.edu.cn

Moshe Buchinsky
Department of Economics
9357 Bunche Hall
Box 851477
UCLA
Los Angeles, CA 90095-1477
and NBER
buchinsky@econ.ucla.edu

SOCIAL INSURANCE AND MIGRATION

Evidence from a Nation-Wide Institutional Reform in China

Fanghua Li
University of New South Wales

Chenyang Ji
Renmin University of China

Moshe Buchinsky
Sciences Po, UCLA, and NBER

Abstract

In this paper we examine the causal relationship between formal social insurance and individuals' migration decisions. We exploit a quasi experimental design in rural China, under which county officials were assigned to a group of villages (i.e., *treated* villages) to serve as village supervisors (VSs) for the local leaders. We show that this led to reduced favoritism in welfare allocation by the local leaders, thereby increasing the efficacy in the formal social insurance in the treated villages. We use detailed geo-referenced administrative household-level data suited for a spatial regression discontinuity design (RDD) to obtain an average treatment effect (LATE) of the improved social insurance on migration. The apparent variation in the implementation of the reform across treated villages and heterogeneous impacts on different family clans make it possible to directly link changes in the efficacy of the insurance to migration choices. We find a large positive migration effect, of about 19%, for the young males and females. In turn, this led to a large boost in the average household's income in just two years.

1 Introduction

In developing countries, despite the existence of social safety nets on paper, the practical distribution of available funds is often inefficient and biased towards individuals who have connections with the local elite, leaving many individuals without insurance protection from the government.[1] However, there has been relatively little discussion about the

[1]The welfare systems in developing countries are often well established at the central government level. Indeed, 119 developing countries have implemented at least one type of cash assistance program (see Honorati et al. (2015)). However, Chuhan-Pole (2016) documented that, due to failures to reach the needy population, foreign aid had only a small effect on poverty reduction. Thus, approximately half of all Africans still lived below the very low $1.25-a-day poverty line, despite the huge foreign aid devoted to alleviating this problem. Qian (2015) finds that the poorest 20% of countries received only 1.69–5.25% of total foreign aid in any given year. There is a substantial body of literature dedicated to quantifying the extent of corruption, with a special focus on developing countries (see Olken & Pande (2012) for a detailed review

potential distortions in individuals' choices that may arise from this inefficiency in the formal social welfare system.[2] The key goal of this study is to investigate this potentially important effect.

One potential link between the absence of formal social insurance and the behavioral distortion at the individual level is provided by Munshi & Rosenzweig (2016). In their research, they consider a dual economy setup in developing countries, in the spirit of A. V. Banerjee & Newman (1998). They demonstrate that well-functioning informal insurance in rural areas, facilitated by extended kin or caste members, disincentivizes rural workers from migrating from their home villages to urban areas where wages and opportunities are much better. This reluctance largely stems from the presence of significant uninsured migration risks. Furthermore, they deduce that effective formal social insurance through the use of a well-functioning government safety net can decrease this spatial labor misallocation by replacing the local village-based insurance with a broader country-wide system. It is important to note that our study focuses on the *risk-mitigating* nature of the social welfare system but not on the *benefit level*.[3]

Figures 1 a–b depict country-level measurements of government efficiency in relation to rural employment variables, utilizing data sourced from the World Bank. Figure 1a shows a significant negative relationship between a measure of government effectiveness and the percentage of male employment in the agricultural sector.[4] Figure 1b shows a significant positive correlation between the change in governance effectiveness and the flow of males transitioning out of the agricultural sector,signifying internal migration to other regions or sectors. These two stylized facts are consistent with the hypothesis positing a close link between the efficacy of formal social insurance and the magnitude of rural-urban migration. Nevertheless, establishing a causal relationship between the two factors is challenging at the country level. This is due to the problems of potential unobserved variable biases, the endogeneity of several factors, and broader national policy considerations at play.

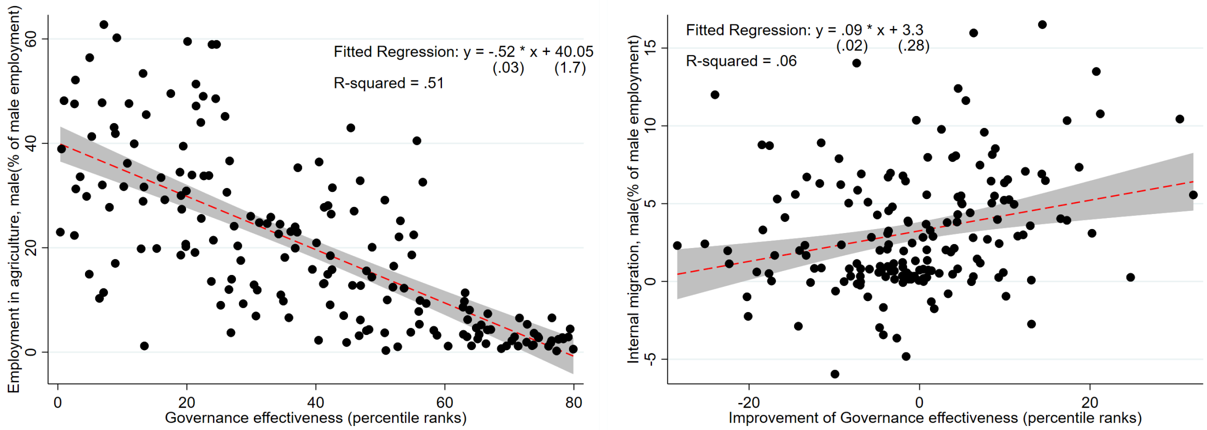
---

of the literature). Among them, several papers provide direct evidence on the magnitude of corruption in welfare allocation. For example, Reinikka & Svensson (2004), in exploring the special education block grant of Uganda, find a stunning leakage rate of 87% due to corruption. Olken (2006), meanwhile, studies the subsidized rice program in Indonesia and finds a sizable leakage rate of 18% due to corruption.

[2] The literature on the impact of governance failures due to corruption on firms highlights the presence of both direct costs (e.g., Svensson (2003)) and indirect costs resulting from behavioral distortion and the misallocation of investments (e.g., Samphantharak & Malesky (2008) and Sequeira & Djankov (2010)). In regards to households, there is evidence that corruption imposes direct costs, which disproportionately affect poor households due to their inability to pay bribes compared to richer households (e.g. Hunt & Laszlo (2006)).

[3] The impact of benefit *level* has been well studied using developed countries' data. It is almost a consensus (e.g. Card et al. (2015) and Gelber et al. (2017)) for developed countries that some public welfare programs like unemployment insurance and disability insurance can lead to a decrease in labor force participation due to the substitution effect and the income effect. Abundant studies evaluate such welfare programs in European countries and the United States (for a review, see **?**).

[4] Both of these variables are averaged over the period 2011–2021 to mitigate the impact of annual fluctuations



(a) Governance effectiveness and agri. emp. (b) Δ Governance effectiveness and -Δ agri. emp.

Note: The data on governance effectiveness come from The Worldwide Governance Indicators (WGI); see (Kaufmann et al., 2009). The proportion of males employed in the agriculture sector is derived from the World Bank country-level statistics.

Figure 1: **Governance Effectiveness and Employment in Agriculture**

In this study, we leverage the exogenous variation in the social insurance efficacy at the most granular administrative units, namely the village, to isolate the impact of the improved functional efficacy of formal insurance on the migration choices of rural workers. The variations in insurance efficacy that we utilize stem from an anti-corruption initiative implemented in China. We first assess the effectiveness of this program, and then examine the impact of this program on individual- and household-level choices. We then further exploit the village-level variation in the treatment intensity (namely the increase in the insurance efficacy) in order to establish a direct causal link between social insurance and individuals' migration choices.

In rural China, welfare programs are established and funded by the central and provincial governments. Due to limited information available to the central government about the rural population, a common phenomenon in developing countries (e.g., Ahmad et al. (1991)), the central government has to rely on locally elected officials (cadres) to distribute allocated funds. These elected village leaders play a role analogous to "claim examiners" (or "benefit specialists") in developed countries. Their endorsement becomes the sole criteria for rural residents to access any welfare benefits in China.[5] Consequently, a typical principal-agent conflict arises, leading to potential corruption and thus inefficiencies in the

[5]It is important to note that this is true across all welfare programs within the rural welfare system. Table A.2 of Appendix A lists all the welfare programs that are available in rural China, spanning from minimum living allowance to government job programs that offer low-skilled positions to those in need. It is important to understand that village leaders are limited to merely *endorsing* the eligibility of a particular household. The actual distribution of benefits is ultimately executed by the county government through direct bank transfers. Historically, village cadres were tasked with greater administrative duties, ranging from from tax collection to welfare distribution (e.g., (Martinez-Bravo et al., 2022)). However, with the cessation of agricultural taxes in 2006 and the growth of banking networks, many of these responsibilities were eliminated. Thus, cadres were relegated to handling basic administrative duties.

allocation of benefits from the welfare programs in villages, as extensively documented in the literature.[6] In response to this sub-optimal situation, the central government initiated a nationwide institutional reform in 2015 as part of a broader national anti-poverty campaign called the "Targeted Poverty Alleviation" (TPA) program.[7] Specifically, county officials, who were outsiders to the villages, were appointed to work full-time as village supervisors (VSs). These VSs were responsible for overseeing their respective villages' activities, working as on-site "claim examiners" together with the village cadres. Additionally, the VSs were required to conduct regular home visits to poor households in order to continually monitor and update their economic status and assess households' needs. The goal of this process was to alleviate any information asymmetry between the county government and the village cadres. This reform exemplifies a common approach to resolving the principal-agent conflict, which involves investing in qualified personnel, improving information collection and processing systems, and enhancing oversight of local officials' actions. In the literature on corruption reduction, this approach is often referred to as "top-down monitoring" (Olken & Pande, 2012).

One advantage of the reform under study is that it introduced village-level variation in the efficacy of formal insurance. It is worth emphasizing that the village represents the most granular level of any policy implementation. This is because villages lack the authority to enact policies of their own, meaning they take all directives issued by the central government as given. As village cadres can only determine welfare eligibility, their relatively limited authority, in comparison to larger administrative entities (such as counties or nations), simplifies the identification of the underlying mechanism. In addition, our paper is based on a comprehensive individual- and household-level administrative dataset from a representative county in China. The homogeneity in cultural, genetic, and geographical factors within our sample further helps to filter out the potential effect of unobservables, which is not typically the case in this line of literature.[8]

We begin by demonstrating the effectiveness of this upper government monitoring in terms of reducing corruption in villages and improving welfare allocation. Following the idea of Fisman (2001), we refer to corruption as the gap in benefit gap between similarly situated poor households who belong to the family clans of their respective village cadres ("favored") and those who are not part of these clans ("non-favored"). We find significant gaps between these two groups of households in the untreated villages (even after controlling for household-level characteristics). However, in treated villages, the gap completely disappears (i.e., those to whom VSs were assigned), which aligns with the findings from top-down monitoring studies, such as (Olken, 2007).

[6] For instance, Golan et al. (2017) demonstrates that the selection procedure for China's rural basic living allowance program (Dibao) has significant non-random inclusion and exclusion errors.

[7] See Guo et al. (2022) for a systematic review of the history of poverty alleviation and the TPA in rural China.

[8] For instance, Spolaore & Wacziarg (2013) downplays the role of formal institutions and emphasizes instead the importance of other determinants of economic performance, such as geographical features, informal cultural norms, and genetic and epidemiological traits.

Having established that the institutional reform did have an overall effect in equalizing the welfare allocation among families with and without clan connections, we next investigate how this affects villagers' migration decisions by comparing the rural households' migration choices in the treated and untreated villages. With the detailed administrative household-level data and geo-referenced data on the exact house location, we are able to exploit the variation in the location of families around arbitrary village borders to isolate and identify the impact of this institutional reform through a spatial regression discontinuity design (SRDD). The segmented and short village pair borders allow us to avoid the long-border problems as highlighted by Keele & Titiunik (2015) and Lehner (2019). That is, we compare individual households on both sides of the border that are in very close proximity to each other.

We then construct a border-level measurement of favoritism reduction and employ the heterogeneity test proposed by Hsu & Shen (2019) to establish a direct link between the change in the formal insurance efficacy and the increased migration flow. We also use village remoteness as a proxy for the village-level variation in formal insurance efficacy to further validate this direct link. Moreover, we examine and rule out other potential mechanisms that could explain the observed effect of the VSs, including improvements in infrastructure or economic growth in treated villages as a result of the VSs' involvement, as well as the possibility of the VSs serving as role models or job agency through their own networks.

We unveil several important findings. First, we find that spatial labor misallocation can be mitigated by improving the formal social safety net. Specifically, we find that rural-urban migration of the poor increased by almost 19% as a direct result of the treatment. This effect is especially strong for the groups of young males and females. Moreover, the increase in migration flow yielded a significant increase in household income of 1,723 CNY (36% of the annual minimum living standard) in two just years, thereby serving as the main driver of poverty alleviation in rural China from 2016 onward.

This study is closely related to several other strands of the literature. First, it contributes to the broader literature on the role of institutions, which has been recognized as a vital factor, especially in poverty alleviation (see Lopez (2004), Acemoglu et al. (2005), and Tebaldi & Elmslie (2008)). However, the endogenous formation of institutions makes it challenging to isolate their causal effects from other unobservable factors, such as cultural attributes and geographic features. Cross-country studies, such as those by Glaeser et al. (2004) and Nunn & Puga (2012), have attempted to explore the role of institutions using innovative instrumental variable methods, but concerns about omitted variables still exist. In addition, Miguel (2004) and Michalopoulos & Papaioannou (2014) introduce identification schemes that exploit border discontinuities in institutional arrangements, with Michalopoulos & Papaioannou (2014) being the closest in spirit to our study. However, their broad definition of institutions at the country level, including everything but ethnicity, limits one's ability to delve further into the mechanisms behind the effects of institutions.[9]

Our study contributes to the literature by focusing on institutional arrangements at the *local* level. The finding that top-down monitoring from higher levels of government reduces village cadres' elite capture is reminiscent to the results of Jia & Nie (2017), who discussed the question of re-centralization in autocracies. Their investigation into centralization and decentralization reforms in China demonstrates that decentralization increases the likelihood of collusion between regulators and firms, ultimately leading to a higher number of coal mine fatalities in the country. Additionally, our study enhances the understanding of the channels through which institutions can affect individuals' decisions and outcomes. We show that local corruption and inefficient formal social safety nets can lead to spatial misallocations of labor, thereby impeding attempts at poverty reduction. We also suggest some effective channels by which antipoverty policies can be further explored in other contexts.

Furthermore, our study contributes to the ongoing debate in the literature on rural-urban migration. The literature widely acknowledges the reluctance of workers to transition from rural to urban areas or from agriculture to non-agricultural sectors, despite the persistent productivity gap between these sectors and the underutilized wage gap (see Gollin et al. (2002), Restuccia et al. (2008), Vollrath (2009), Gollin et al. (2014), Gollin et al. (2014), and Munshi & Rosenzweig (2016) for recent evidence). Recent literature goes beyond the traditional view of ability sorting theory (e.g., Young (2013)) and posits that low mobility can be attributed to other factors, including migration costs (e.g., Bryan & Morten (2019) and Imbert & Papp (2020)) and liquidity constraints (Bryan et al., 2014). Fafchamps & Lund (2003), along with other researchers, offers behavioral explanations for this phenomenon, highlighting the significance of the uninsured migration costs. According to these perspectives, the observed wage gap represents a spatial labor misallocation. Thus, the relocation of workers out of less productive rural agricultural activities could lead to substantial improvements in welfare. Our unique setting allows us to provide some of the first evidence of the impact of improved formal insurance on individual decision-making.

It is important to note that our setup also enables us to identify the causal effects of internal temporary migration—an issue that has been hampered by difficult selection issues. This problem has been addressed in the literature through the use of various econometric methods, such as instrumental variables (e.g., Yang & Martinez (2006)), panel data (e.g., Beegle et al. (2011), or through the use of (quasi) experimental settings (e.g., Clemens (2010), McKenzie et al. (2010) that explore exogenous changes in conditions in the *destination* locations in order to study the effects of permanent international migration. We contribute to this line of literature by exploring changes in migration patterns induced by exogenous variations in the formal insurance levels offered at the *origin* villages in rural China.

[9] As noted by Rodrik (2000), Chong & Calder'on (2000), and Tebaldi & Elmslie (2008), this inherent imprecision in the definitions and measurements of institutions is one of the major concerns in the study of institutional impact.

The rest of the paper is organized as follows. In Sections 2 and 3, we introduce the institutional reform, outline the context of our study, and introduce the data. In Section 4, we explore the effects of the institutional reform on corruption reduction, revealing that corruption was significantly reduced. Based on this finding, in Section 5, we present SRDD-based estimates that quantify the impact of improved formal insurance on migration choices and family income. Section 6 then further establishes the causal link between these two factors by analyzing village-level corruption reduction and excluding other potential mechanisms. Finally, Section 7 offers a conclusion and discusses the generalizability of our findings.

2 Context and Background

2.1 Institutional Reform for Selected Villages

Many developing countries, including China, typically suffer from two related constraints that severely limit the implementation of any formal social security schemes, namely incomplete information and inadequate administration (Ahmad et al. (1991)). In rural China, welfare programs are established and funded by the central and provincial governments. Due to the lack of proper information, the centralized information infrastructure, like the tax system and urban social welfare systems, tends to bypass the vast majority of the rural community. This lack of coverage is especially pronounced for the most impoverished, who are in the greatest need of social safety nets.[10] In this context, village leaders, who are familiar with the economic conditions of the locals, take on roles analogous to ”claim examiners” or ”benefit specialists” in developed countries. Thus, their endorsement becomes, to a large extent, the sole criteria for the rural residents to be able to have access to the welfare benefits in China.[11] These village heads (cadres) are elected by the villagers, and come typically from the local elites.[12] These elected cadres are not civil servants and there are no term-limits on their appointments. Thus, they typically stay in their positions for a long time. Due to the absence of cross-validation mechanisms, limited oversight from higher authorities, and a lack of incentives or penalties for misconduct, these village cadres are well positioned to misuse their administrative authority, ultimately resulting in

[10] According to the national survey conducted by the Bureau of Statistics, only 15% of rural workers in China work under full insurance coverage through the firms that employ them in 2011. In addition, with the cessation of agricultural taxes in 2006, the central government lost its last direct avenue of information regarding the rural poor.

[11] It is essential to recognize that while autonomy within village communities is common in developing regions including India, Southeast Asia, and Africa, the extent of autonomy granted to villages varies significantly. (Myerson, 2017). For instance, India has a long history of traditional and decentralised forms of democratic local rural governance, most commonly through *Gram Sabhas* (village councils) (Datta, 2019). In India, village heads often embark on their political careers at the local level, whereas in China, village heads are not recognized as formal civil servants and lack a clear pathway to higher administrative tiers.

[12] There are usually two village cadres in a village: the Party Secretary (*zhishu*) and the Village Chairman (*zhuren*). See Schubert (2002) for a review of village elections and Martinez-Bravo et al. (2022) for a further discussion of the election results. For a review of essays on village governance, see Alpermann (2003).

a malfunctioning formal insurance system on a practical level.

At the end of 2015, the central Chinese government launched the ”Targeted Poverty Alleviation” (TPA) campaign in an attempt to enhance the operational efficiency of the existing social welfare system. The TPA is not a standalone new welfare scheme, as it is sometimes perceived to be. Rather, it was established in order to revamp the existing system. Its goals are to accurately identify, continuously track, and gather data on the poorest rural households, subsequently being able to offer them suitable programs tailored to their needs from the available pool.[13] This campaign started with extensive on-site interviews, combined with rigorous verification and auditing procedures, to identify the poorest rural families and to construct a comprehensive household document with which to initialize the information system, namely the Poor Household Registration and Management System of China (PHRMS) (elaborated further in Section 2.3).[14] Each poor household registered in this system was assigned a county official, similar to a social worker in terms of their role, who would regularly visit and update their information to ensure data accuracy.[15]

Following this, rural China has witnessed significant institutional reform. The key element of this reform was the introduction of *”village supervisors”* (VSs). These are officials from the county or upper government who spent most of their workweek (five days and four nights) in assigned villages, overseeing all administrative activities in collaboration with locally-elected village cadres. This is analogous to having on-site claim examiners sent by the government.

These VSs were additionally assisted by a dedicated work team from the same county department, led by department heads or their deputies, who visited these villages regularly. Their responsibilities included engaging in evening discussions with villagers to gather updated information and making impromptu visits to assess the performance of the VS and offer support. They essentially served as ”audit specialists” who helped the ”claim examiners” (the VSs) by periodically reviewing the situation. The specialized work teams also scrutinize cadres’ actions for any potential corrupt shifting (Olken, 2007). This revised system marked the starting point of a centralized information infrastructure covering the most impoverished rural population, thereby granting upper-tier government direct informational access and circumventing the traditional exclusive reliance on village cadres.

This institutional reform, given its pilot nature and limited manpower, was initially rolled out only in selected villages, termed as *”poor villages”* (*”pinkuncun”*). From 2018 onward, certain features of the reform began trickling down to other villages (”not-poor villages”),

[13] This is similar to the *Ultra-Poor Graduation Program* proposed by A. Banerjee et al. (2015), which was also designed to identify the most needed first and then provide a ”big push”, or a basket of welfare programs tailored to their needs.

[14] To be specific, the Leading Group Office of Poverty Alleviation and Development, a department of the central government of China, holds and manages this system. The system contains 29 million households and approximately 90 million individuals, covering 17% of the overall rural population.

[15] (Zhang et al., 2021) provides a comprehensive overview of these ”social workers” and examines their impact on information gathering. On average, each one oversees 2-3 poor households included in the system.

albeit in a diluted manner (e.g., only one VS was assigned without the additional dedicated village team). Moreover, the competency of VSs in the "not-poor" villages was notably inferior to that of the "*poor villages*", as measured by the importance of their government positions, affiliated government tiers etc.[16] Given the time required to change the behavior of the residents, we take the 2016–2018 period as the *treatment* period for the selected villages.

2.2 The (Quasi-) Randomness of the Village Selection

According to official Chinese documents, the criteria for selecting the treated villages (namely "poor villages") for this institutional reform are somewhat obscure. While the treatment was designated to "poor villages", the label's conception traces back to 2001. Prior to the TPA, China's efforts to reduce poverty largely centered around allocating financial funds to villages that were designated "poor" villages.[17] Due to the absence of direct individual-level information, the designation process was solely delegated to the county governments. The counties typically rotated the "poor village" designation amongst all villages to ensure resource equity at the county level.[18] Consequently, by 2014, the characteristics of those villages designated as "poor" villages closely resembled the characteristics of those designated as "not-poor" villages.

We compare "poor" and "not-poor" villages in 2013 across multiple dimensions—natural resources, geographic location, infrastructure development, village cadre characteristics, and economic activities—in Appendix D, Table D.2 (the data used is described in detail below). We categorize villages based on their 2014 "poor" status, which determined their eligibility to benefit from the institutional reform. The F-test statistics indicate, however, that there are negligible differences between treated ("poor") and untreated ("not-poor") villages. To further scrutinize the rotation process described above, we group all villages into four groups based on their designations from 2010 onward and test whether the characteristics of the groups are the same.[19] Indeed, the F-test statistics reveal that these designations, as well as the timings of the designation, are not correlated with any of the villages' attributes.

[16]Typically, those serving the "*poor villages*" held higher positions within their departments and often came from higher governmental divisions including the prefectural, provincial, or central government. For a comparison of the supervisors' and cadres' age and education, please refer to Appendix A, Figure A.2. We also provide information about the education level of the VSs, their government positions, the quality of their departments, and the rankings of the paired town officials for both the "poor" and "not-poor" villages in Appendix A, Figure A.3.

[17]See detailed introduction about poverty alleviation in China in Y. Liu et al. (2018).

[18]The county governments are responsible for assessing and designating "poor villages." As Yuan (2019) notes, the initial selection of poor villages depends on village characteristics. However, the county rotated the "poor village" title every 2 years or so. In official documents, this procedure is referred to as *"One plan, two years implementation, checking results one village at a time, pushing forward in batches."*

[19]The four groups are as follows: (a) villages never labeled as "poor"; (b) villages labeled as "poor" pre-2014 but designated as "not-poor" in 2014; (c) villages labeled as "poor" in 2014, making them eligible for the institutional reform before they switched to "not-poor" prior to 2015; and (d) villages labeled as "poor" in 2014 but relabeled as "not-poor" between 2016 and 2018, also making them eligible for the reform.

Even though our findings indicate no association between treatments and a multitude of observable factors, concerns may remain about the potential influence of unobservable village attributes. To address this, we employ an SRDD in order to analyze individuals' choices. This approach exploits the continuity of various factors—cultural aspects, geographical conditions, infrastructure accessibility, and more—around some exogenously positioned boundaries of the villages. Moreover, it is unlikely that the mere labeling of a village as "poor" would correlate with any of the individual traits just adjacent to these boundaries. We provide a detailed discussion of the identification below in Section 4.1.

3 Data

3.1 Sample County and Data Source

We undertook a large-scale data combination exercise constructing a household-level and an individual-level dataset of villagers from one representative county in Henan province. For context, Figure B.1. and Table B.1 of Appendix B show the distribution of various economic and demographic indicators among all Chinese counties. Notably, our sample county is a representative county: Its economic development level (per capita GDP), urbanization rate (the ratio of people in the agricultural industry to the total population), the population density (capita per square meter), and the education level (average years of education) all align closely with the median value across Chinese counties.[20] The evidence provided above suggests that our sample county is indeed representative of China.

The advantage of using data from this county is that we have access to several administrative datasets that are crucial for assessing various aspects of individuals over their life cycle. Thus, to take full advantage of individual-level, household-level, as well as village-level variation, we merge the available administrative data from several sources. This section provides an overview of the data sources and variable definitions used in our analysis.

As noted above, the core dataset combines annual rounds of the individual and household censuses in the period 2014–2019 from the Poor Household Registration and Management System of China (PHRMS). The data include detailed information on household characteristics including household assets, annual income (with a breakdown of income sources), family members' demographic information, and labor supply details. We were provided by the government of our sample county with the universe of these longitudinal micro data for the whole county. There are around 12,000 poor households in our sample county (approximately 15% of the rural population in this county) for a total of 45,000 poor individuals, located in 173 villages, 73 of which were designated in 2014 as "poor" villages, while the other 100 were always designated as "not-poor" villages.

[20]We calculate the distribution of per capita GDP for all Chinese counties based on statistical data from the China County Statistics Yearbook 2013.

In order to measure the connection between the cadres and the individuals in their villages, we obtained the village cadre rosters from 2015 to 2019. This enabled us to create a predetermined and objective proxy for unobserved connectedness between households and corresponding village cadres using their surnames and generation names, as detailed in Section 4.

The location of households and their proximity to village boundaries play a crucial role in the SRDD employed in this research. The county administration maintains a comprehensive database of land GIS details for every land parcel, containing the owner's identification details. This enables us to link the geographical data of land parcels with the households listed in the PHRMS. As a result, we successfully determined the locations of over 85% of these households. Additionally, we sourced data on village boundaries from the Institute of Geographic Sciences and Natural Resources Research (CAS).

Furthermore, we obtained information on paved roads from the *Bigemap*, a professional GIS map provider in China. Combining road GIS information and household location information enablees us to measure road accessibility for each household in the sample for the years 2013, 2015, and 2018. In addition, we obtained monthly non-residential electricity usage account data from the State Grid from 2015 onward and aggregated it by village as a measurement of villages' economic activities.[21] Additional village-level data, such as information on village cadres, VSs, and natural resources, are sourced from the PHRMS.

Table 1 presents descriptive statistics for our dataset. In Panel A, we provide descriptive statistics for household-level variables, while in Panel B, we provide statistics for individual-level data. Our dataset spans the period from 2014 to 2020 and includes records from a total of 11,997 households, amounting to approximately 82,000 observations. During our sample time frame, the average yearly household income is roughly 6,000 CNY, with salary earnings being the primary source of income. Remarkably, income from farming only contributes an average of 400 CNY annually, which is a mere 6.7% of the total family income. This aligns with the understanding that farming is both challenging and not particularly lucrative in this predominantly hilly county.

Our focus in Panel B of Table 1, our focus shifts to the working-age demographic. The data reveal that 55.2% of this group are male, the average age is 44 years, and the educational attainment level is generally low. Nearly half (47.6%) of these individuals engaged in paid employment during our observation period. A significant majority of them (73.1%) opted for jobs outside the county, at least temporarily. The village-level descriptive statistics are provided in Appendix D, Table D.1. The villages are generally relatively small, with an average population of around 1,400 individuals, or 400 households.

[21] State Grid is the only electricity company in North China; hence, this data covers all electricity usage of all types. For more details, please refer to the company's official website: `http://www.sgcc.com.cn/ywlm/index.shtml`

Table 1: **Descriptive Statistics of the Main Data Set**

Variable	Sources	Observations		Mean	St. Dev.
		Total	Unique		
Panel A: Family structure					
# of children under 5	PHRMS	82,275	11,997	0.16	0.44
# of children 6-15	PHRMS	82,275	11,997	0.52	0.74
# of people over 65	PHRMS	82,275	11,997	0.46	0.68
# of all family members	PHRMS	82,275	11,997	3.44	1.54
Family income					
per capita salary (k)	PHRMS	82,275	11,997	5.60	5.82
per capita farming income (k)	PHRMS	82,275	11,997	0.41	1.38
per capita self-earned income (k)	PHRMS	82,201	11,997	6.02	5.85
per capita cash transfer (k)	PHRMS	82,201	11,997	1.81	2.24
Accessibility to infrastructure					
Distance to paved roads in 2018 (m)	Land, GIS	71,763	11,730	133.45	884.61
Change of distance 2015-18 (m)	Land, GIS	71,763	11,730	503.91	876.30
Panel B: Individual-level data					
Individual characteristics					
Age	PHRMS	217,844	36,485	44.05	16.49
Male dummy	PHRMS	217,844	36,485	0.55	0.50
Elementary school degree	PHRMS	191,827	36,485	0.34	0.47
Middle school degree	PHRMS	191,827	36,485	0.45	0.50
High school degree	PHRMS	191,827	36,485	0.07	0.25
3-year college and above	PHRMS	191,827	36,485	0.05	0.22
Employment and migration choices					
I(employ) = 1	PHRMS	217,844	36,485	0.48	0.50
I(work in town) = 1	PHRMS	217,844	36,485	0.09	0.29
I(migrate) = 1	PHRMS	217,844	36,485	0.35	0.49
Month	PHRMS	217,820	36,485	3.68	4.30

3.2 Sample Selection Problem

Despite the fact that the main source of information in our study is restricted to poor households, this does not significantly hinder our study's objectives. Our primary aim is to isolate the causal effect of the social safety net on the migration decisions of the rural population. Given that the poor population is the primary beneficiary of the available social welfare programs, the group of people included in our analysis is most relevant for our question.

A potential concern regarding sample selection is the accuracy of the selection process into the PHRMS. In Appendix C, we provide the details of the poor household designation procedure. The initial step involves village cadres nominating households as "poor". Several subsequent verification stages ensure that the households marked as poor do legitimately fall into that category and that no genuinely poor households were overlooked. This verification procedure combines community-based methods (e.g., Alatas et al. (2012)) with self-targeting (e.g., Alatas et al. (2016)), both of which are conducted by county officials on-site. Additionally, the process incorporates cross-verification of several data sources, or proxy means testing (e.g. Alatas et al. (2012)), which is overseen by both the provincial and central government. This entire identification process lasted for over a year, and, following the verification stages, out of the 29 million households initially nominated by village cadres, 9.29 million were reclassified as "not-poor" and subsequently removed from the system. Concurrently, an additional 8.07 million households were integrated into the system based on further assessments.[22]

In our sample county, an initial selection in 2014 identified 11,810 households. However, subsequent verification unearthed discrepancies. The county officials, in their November 2015 on-site review, found that 18.2% of these households had been inaccurately designated. Later, in December 2015, a proxy means test conducted by the central government identified another 3.7% as erroneous inclusions. To understand the patterns of these selections, we apply a multinomial Logit regression analysis using the roster data from each round. The results, displayed in Appendix C, Table C.1, indicated a propensity for households connected to village cadres to be mistakenly included in the initial selection. This suggests a degree of favoritism in the primary nomination process. However, the subsequent verification seemed effective in correcting these initial oversights.

These results suggest that the finalized poor household list exposed to the institutional reform under study primarily captured the poorest households. Even if there was a modest bias towards families with local ties in the PHRMS, our estimations would still provide a lower bound for the impact of reducing corruption (in the form of favoritism). Nevertheless, we limit our primary analysis to the poorest 10% of households in each village, out of the 15% poorest covered by this dataset. This approach helps us to exclude families with slightly higher incomes where favoritism may have a greater influence and also addresses potential inconsistencies in the selection procedures across villages.

4 The Institutional Reform and Corruption—Within-Clan Favoritism

In this section, we treat the institutional reform as a quasi-experiment to quantify the corruption level without the reform, and explore whether the assigned VSs reduced the corruption in the treated villages.

4.1 Measurement and regression setup

Following the approach of Fisman (2001), we measure corruption by determining the gap between the benefits received by similarly qualified poor households who belong to the

[22]Source: https://baijiahao.baidu.com/s?id=1691278301138105841&wfr=spider&for=pc

family clans of the corresponding village cadres ("favored") and those who are not part of these clans ("non-favored"). To generate a measure of family clan connectedness, we obtained the village cadre rosters from 2015 to 2019, together with each cadre's education level, age, and gender.[23] This makes it possible to create an objective proxy for the unobserved connectedness between households and the corresponding village cadres using their surnames. The role of family ties is well documented in the literature, e.g., Ashraf & Bandiera (2017). Moreover, family clan favoritism has been recorded in various Asian countries, including Vietnam (see Do et al. (2017)), India (Khalil et al. (2021)), and China (Kung & Zhou (2021)). The use of family networks as a measure of connectedness is particularly suitable in our context. Specifically, we define this measure for family i at time t as a binary variable that takes the value 1 if any member n within family i shares the same surname with the village cadres in the corresponding village at the same time:

$$Connection_{it} = \begin{cases} 1 & \text{if } \exists n \in i : \text{surname}_{nit} = \text{cadre\_surname}_i \text{ at time } t \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

We examine the factors that determine the probability that one receives welfare from a program k (i.e., $Pr(\text{program}^k = 1)$) and the amount of money that one receives from that program (i.e., program^k), for welfare program k. To be specific, we consider the following two regressions:

$$Pr(\text{program}_{it}^k = 1) = \alpha_0 + \alpha_1 \text{Connection}_{\text{it}} + \alpha_2 \text{Connection}_{\text{it}} * T_{it} + \mathbf{X}_{it}'\mathbf{\Theta} + \varepsilon_{it}, \tag{2}$$

$$\text{program}_{it}^k = \beta_0 + \beta_1 \text{Connection}_{\text{it}} + \beta_2 \text{Connection}_{\text{it}} * T_{it} + \mathbf{X}_{it}'\mathbf{\Lambda} + \epsilon_{it}, \tag{3}$$

where the dummy variable $T_{it} = \{0,1\}$ denotes whether household i is in a treated village and thus is subject to the institutional reform. The dependent variable $\mathbf{I}(program_{it}^k)$ in equation (2) denotes whether an individual is a beneficiary of program k. Given its binary nature, we utilize the probit regression model for estimation. In equation (3), the dependent variable $program_{it}^k$ is the annual monetary assistance received by the individual through program k. Hence, we employ the Tobit regression model for estimation to account for the left-censored data. In order to account for income-based eligibility, we include several family-level characteristics in our control $\mathbf{X}$—(i) per capita self-earned income, (ii) family size and the number of dependent members, and (iii) distance to the nearest paved road and proximity to the village center—to control for accessibility to essential infrastructure and earning potential.

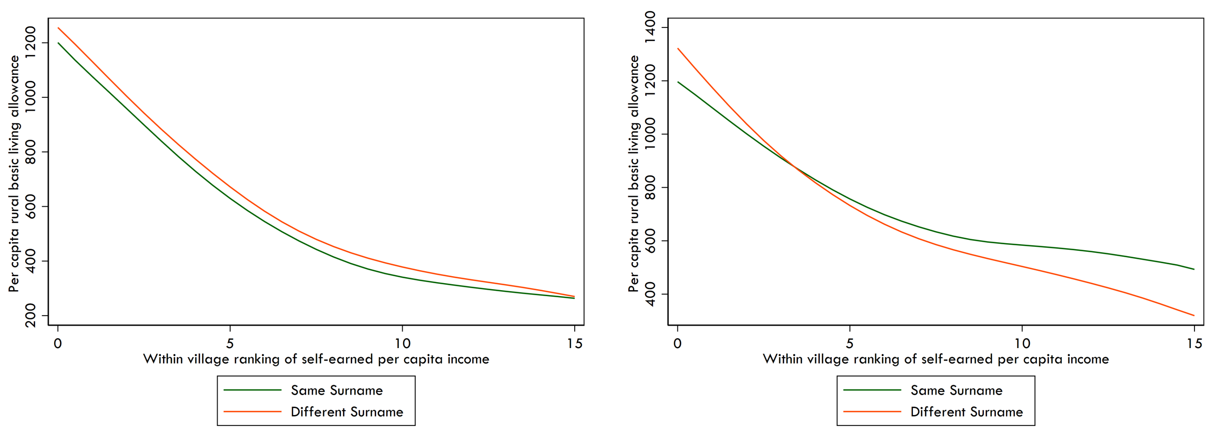
With this setting, the coefficient on *Connection* captures the excess advantage in access to welfare programs and the financial support received by connected families, with income-based eligibility controlled. Additionally, the coefficient of the cross-term between *Connection* and the village treatment dummy variable T captures the reduction in favoritism in treated villages.

[23] Village cadre elections in the sample county are held every three years (December of 2014, April of 2018, and December of 2020). Historically, village cadres tend to be re-elected for more than one term. Among the 260 village cadres who were in office in 2016, only 6.4% were newly elected, while over 55% were in office for more than two terms.

4.2 Results and Robustness Checks

In Table 2, we report the results for the two most important welfare programs, namely the ”Basic Living Allowances” (BLA) program, in Panel A, and the ”Government Job” program, in Panel B. [24]

The BLA is the largest nationwide UCT program that provides cash supplements to households with an income below specified thresholds, covering 346 million individual beneficiaries in 2019.[25] In our sample, 49.9% of the poor families were covered by this program, receiving an average of 3,189 CNY per year. Figure 2 provides graphical illustration of the treated villages (Figure 2a) and non-treated villages (Figure 2b). From this, we can see that there is a clear difference between the treated and non-treated villages. In the treated villages, the ”favored” groups and ”non-favored” groups are comparable at all levels of per capita self-earned income, while in the untreated villages, the ”favored” group has significant advantages, especially at the the higher self-earned income distribution, where the room for cadre maneuvering is larger.



(a) Treated Villages

(b) Non-Treated Villages

Figure 2: **Basic Living Allowance for Treated and Non-Treated Villages by Connection to Village Cadres**

Columns 1–3 in Table 2 present the probit regression results for the BLA program. In column 1, no additional controls are included, while columns 2–3 progressively introduce more control variables to control for the eligibility. The coefficient of *Connection* is

[24]We summarize all the welfare programs available for rural residents in China during the period of 2017–2019 in Table A.2 of Appendix A. There are a total of ten programs, with five being universal programs established well before the TPA period and available to rural Hukou holders. The remaining five programs are exclusively designed for the poor households identified through the TPA campaign. As shown in Table A.2, the other programs either have too high a coverage rate (of around 90%) or too low a coverage rate (2%).

[25]Source: The Ministry of Civil Affairs: https://baijiahao.baidu.com/s?id=1640359132428684354&wfr=spider&for=pc.

consistently positively significant across various model specifications, indicating major corruption in the form of favoritism in program allocations. When all control variables are included, the marginal effect is approximately 14%, implying that households from the same family clans as cadres are 14% more likely to receive welfare transfers from this program, provided they have similar income levels, earning potential, and family demographics. Meanwhile, the coefficient of $Connection * T$ is significantly negative with a magnitude comparable to that of the favoritism captured by $Connection$. This suggests a complete reduction in favoritism under the new institutional regime in treated villages, which aligns with the graphical evidence presented in Figure 2.

The Tobit regression results in columns 4–6 further corroborate this finding. They reveal that the favoritism in access to BLA translates to a 143 CNY advantage in the amount received by households from the same family clans as the village cadre compared to their counterparts.

To demonstrate the prevalence of favoritism across all welfare allocations, we extend our analysis to the second-largest welfare insurance program and present the results in Panel B of Table 2. Initiated in 2016, the "Government Job" Program is designed to assist poor households that have experienced job losses by providing employment opportunities within the village, such as street cleaning and other low-skilled positions. In our sample, 18.5% of families received cash transfers through this program, with an average income of 5,880 CNY. This program follows the same pattern as that observed for the BLA Program, whereby households in the same clan are 5% more likely to be included in the program and receive, on average, 89 CNY more from this program relative to those not from the clan. As before, we see that this favoritism disappears altogether in the treated villages.[26]

For robustness, we employ two alternative measures of connectedness. Firstly, Table E.2 of Appendix E, we utilize the number of family members who share the same surname as village cadres as an indicator of connectedness. The results for this measure show that for each additional family member sharing the cadre's surname, there is a 6.2% increase in the likelihood of receiving BLA. These findings align with our baseline results, where the average number of family members sharing the same surname in connected families is 2.3.

We further introduce a third measurement by delving into traditional Chinese naming conventions. Traditional Chinese names, particularly male names, are made up of three components: the family name, a generational name, and a given name, with the generational name denoting the individual's position within the clan's generational sequence (Z. Li & Lawson (2002)). Thus, we define a family's connectedness based on whether any family member shares not only the same surname but also the same generational name with the cadres. This definition introduces additional variation in measuring connectedness because a typical Chinese village consists of only a few family clans. Thus, using a

[26] In Table E.1 of Appendix E, we provide additional results by year. We find that these reform treatment effects are significant in 2017, one year after the start of the treatment, and continues to be so in the following two years.

Table 2: **Favoritism and Reform Effect on Favoritism Reduction**

	$Pr(\text{program}_{it}^{k} = 1)$			Program^{k}		
Panel A —Rural Basic Living Allowance Program:						
Connection×T	-0.499***	-0.429***	-0.472***	-577.962***	-372.268***	-382.722***
	(0.022)	(0.023)	(0.025)	(22.177)	(20.534)	(20.909)
Connection	0.357***	0.329***	0.355***	351.965***	263.581***	270.480***
	(0.020)	(0.020)	(0.023)	(19.676)	(18.240)	(18.640)
Income	No	Yes	Yes	No	Yes	Yes
Demographics	No	Yes	Yes	No	Yes	Yes
Distance	No	No	Yes	No	No	Yes
Obs	35,712	35,647	30,311	35,712	35,647	30,311
Panel B —Government Job Program:						
Connection×T	-0.349***	-0.315***	-0.315***	-1,116.463***	-870.767***	-843.395***
	(0.026)	(0.027)	(0.029)	(86.146)	(84.584)	(87.508)
Connection	0.260***	0.232***	0.213***	814.915***	657.144***	579.942***
	(0.023)	(0.023)	(0.025)	(74.412)	(73.025)	(76.560)
Income	No	Yes	Yes	No	Yes	Yes
Demographics	No	Yes	Yes	No	Yes	Yes
Distance	No	No	Yes	No	No	Yes
Obs	35,712	35,647	30,311	35,712	35,647	30,311

Notes: This table presents the impact of family elite connectedness on welfare access and allocation. In Panel A, we report the results for Basic Living Allowance (BLA) program and in Panel B, for Government Job program. For each program, we report the probit results of program access in the first four columns (with *Connection* measured with only shared ancestry in the first two and also same generation in the last two columns) and program amount allocated in the last four columns. For each regression and *Connection* measurement, we report the results without any controls first and then with controls, including per capita self-earned income, family size and number of dependent members, distance to the nearest paved road and to the village center.

binary variable based solely on surnames might be too coarse.[27] The results of this exercise, which are presented in Appendix E, Table E.2, do not change much relative to the results presented in Table 2. Overall, the results are robust across the various measurements of connectedness used and model specifications.

4.3 Corruption shifting

The last robustness check we perform considers the possibility of the existence of corruption shifting, as discussed in the literature on top-down monitoring. As noted by (Olken, 2010), the dilemma here is that the individuals assigned to oversee and enforce penalties may also be susceptible to corruption, implying that top-down monitoring might merely shift the corruption from lower-level officials to the auditors. In our case, the performance of the VSs is scrutinized by the village cadres, the work team members, and even the villagers. Also, the penalties for engaging in corrupt practices are substantial, since their prospects of earning promotions depend on their VS performance. Additionally, there is a clear distinction between the welfare allocation in our study and the infrastructure projects investigated by Olken (2007), with it being far more challenging to audit the latter because of its complex procedures. Furthermore, the fact that neither the VSs nor the cadres handle money transfers directly makes it more difficult for them to obtain direct payment from the villagers.

We conduct additional analysis in which we examine whether families sharing the same surnames with the assigned VSs gain any advantage in welfare allocation. The findings presented in Appendix E, Table E.3 reveal no significant positive advantage for this group; in fact, some estimates for the VS-clan dummy variable are even negative.

5 Reform Effects on Households' Decisions and Outcomes

We devote this section to examining the impact of the institutional reform on households' decision-making and outcomes.

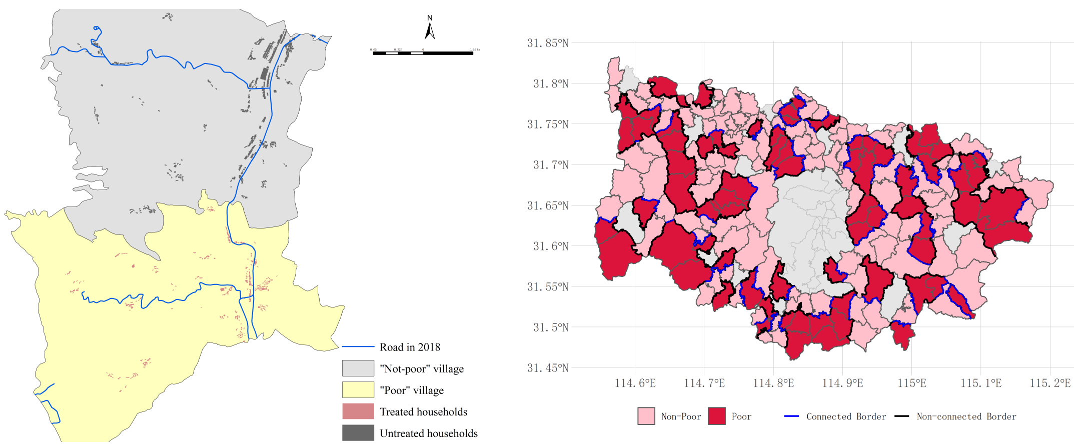
5.1 Identification strategy

Our research is based on a quasi-experimental design that allows us to isolate the causal impact of institutional reform. Through the use of geo-referenced administrative household data, we can precisely locate each household within the county. This granularity in the data enables us to exploit the variations in this reform and to estimate the causal impact by comparing households situated on either side of the border separating villages with differing institutions.

More specifically, we focus on pairs of adjacent villages, with "poor" villages on one

[27] In our sample, 44% of poor families feature at least one member who shares the same surname as village cadres.

side and ”not-poor” villages on the other, as illustrated in graph (a) of Figure 3.[28] The households on both sides of the border (marked by red and dark grey squares) are comparable in terms of their cultural traits, geographic features, and pre-reform institutional structures. The Hukou system and land allocation policy in rural China offers a unique advantage in identification by reducing the selection issue related to household locations. In rural China, all land was allocated to Hukou-registered villagers in the late 1940s and remains virtually unchanged, limiting any post-reform relocation within villages.[29] In addition, all social welfare programs are allocated based on the Hukou status of the households, thus eliminating the possibility of post-reform migration across the village borders.[30]. Lastly, the historical nature of the borders further enhances the random distribution of households across arbitrary borders, reinforcing the validity of our identification strategy.



(a) Example of Village Border

(b) Village border type distribution

Figure 3: **Example of Village Borders and Distributions in the Sample County**

We employ a SRDD to form our estimates. The running variable is household distance to the poor–non-poor (P–NP hereafter) village borders. We denote the distance to the border for a household in the ”not-poor” villages in negative values and the distance for a household in the ”poor” villages in positive values, meaning the cutoff point is normalized to 0. Under the assumption of continuity of all other household characteristics—infrastructure, market accessibility, etc.—the SRRD estimator provides the local average treatment effect (LATE) of being treated by the institutional reform for households just around the borders. Following the recommendations of Imbens & Lemieux (2008) and Calonico et al. (2019),

[28]In this figure, the bottom village (marked in yellow) was treated while the top village (marked in gray) was not. For security reasons, we cannot reveal the exact names of these two villages. If further information is needed, please contact the authors.

[29]China launched a nationwide Land Reform in the late 1940s until the early 1950s in an attempt to allocate land to all villagers. Under this reform, villagers were given usage rights but were not allowed to sell or trade these rights, thereby cementing their locations in the long term, except for in a few exceptional cases.

[30]For a detailed introduction to the Hukou system, see, for example, Z. Liu (2005)

our primary specification uses the following local linear regression specifications:

$$Y_i = \beta_0 + \beta_1 \text{institution}_i + \beta_2 \text{distance}_i + \beta_3 \text{distance}_i \times I(\text{distance}_i > 0) + \zeta X_i + \varepsilon_i, \quad (4)$$

where Y_i is the outcome of interest for the household or individual i, distance_i is the direct distance from the i's house to the connected village border, with $\text{distance}_i < 0$ if i belongs to "not-poor" villages. The variable institution_i is an indicator that takes the value 1 if household i is in a "poor" village, and zero otherwise. Therefore, the coefficient β_1 captures the effect of the institutional reform on the outcome variable. The optimal bandwidth is determined by employing the method proposed by Calonico et al. (2014) and Calonico et al. (2019). We use *Epanechnikov* kernel function to construct the local-polynomial estimators. It is important to note that, in general, the results are robust with respect to the exact choice of the kernel function, the bandwidth selection method, and whether we include control variables in the vector X_i that are related to individuals' migration choices and income. We control for household-level characteristics, including indicators for family structure (family size), the number of various kinds of dependents (including 0–5-year-old children, 6–15-year-old students, and over-65-year-old seniors), accessibility to village amenities in the base year (measured by direct distance to the paved road), improvement during the treatment period (change of distance to the road), and household-level direct transfers in the base year. In addition, we control for the individual-level characteristics for the individual-level regressions, including age and education level.[31]

The relatively short and segmented village borders in our study offer another methodological advantage over the long geographic boundaries commonly used in SRRD research, such as the national boundaries employed by Michalopoulos & Papaioannou (2014). Figure 3 (b) provides a map of the county under study, illustrating both the spatial distribution of the village borders we employ and their random dispersion across the county. As Keele & Titiunik (2015) indicates, long boundaries can distort spatial interpretations by not accounting for the actual distance along the border, which often results in comparisons of geographical units that are far away from each other. In our case, each short village border is equivalent to a segment in the segmentation method suggested by Lehner (2019).

For an additional robustness check, we select a more restrictive subset of P-NP borders, namely the borders with crossing roads connecting the two adjacent villages. This connected P-NP border subset (CP–NP hereafter) ensures that households on both sides of a given border are not only exposed to similar cultural norms, genetic traits, and geographical features, but that they have identical access to infrastructure (roads, irrigation, schools, hospitals, electricity, etc.) and job opportunities as a result of the road connection. Panel A of Figure D.1 in Appendix D provides an illustration of the connected vs. disconnected borders for comparison.[32].

[31]Note that identification does not require us to control for household characteristics. Nevertheless, it improves the efficiency of the estimation and can also serve for additional robustness checks, as we show below.

[32]The purple village at the top and the dashed village in the bottom-left can be viewed as *Type A* because of the blue road connecting them. By contrast, the purple village and the white village in the bottom-left

5.1.1 Validity of the Identification Strategy

For the SRRD to be a valid method for identifying the causal effect of the institution reform, two conditions have to be satisfied. Here we provide strong evidence for the *continuity condition*, namely that households around the threshold (connected village borders) are comparable and their observable (and likely unobservable) control variables are continuous at the points of relevant borders.

We verify that individuals and families around the borders are indeed comparable. We first run a sequence of probit regressions on whether a household belongs to the treatment group (i.e., subject to institutional reform) on both household and individual characteristics. The results reported in Appendix D Table D.3 clearly indicate that none of the family characteristics, or individual characteristics, are significant, and, consequently, all the χ^2 tests fail to reject the null hypotheses of equality across the two sides of the relevant borders. Recall that we have already demonstrated that the selection of the treated villages is not affected by any village-level observables.

Furthermore, Appendix D, Figure D.2 shows the distribution of the poor population in the lowest 10% income level within each bin of 50 meters across both sides of the borders, normalized to be zero. As can be clearly seen from the figure, the poor individuals are scattered randomly around the village borders. Thus, one would expect the exact choice of the bandwidth to not affect the results.

5.2 Individual Migration Decisions

The main outcome we focus on in this paper is whether villagers in treated villages are more likely to temporarily migrate to urban areas where they have better employment prospects (e.g., higher income). In this section, we assess the average treatment effect on migration.

To assist us in this task, we introduce the categorical variable Mig to describe one's labor supply and temporary migration choices. We concentrate on the individuals who stayed in their original town in the base year (2015) by constructing the following definition:

$$Mig_{it} = \begin{cases} 1, & \text{if individual } i \text{ migrates to an urban area for a paid job in year t,} \\ 0, & \text{otherwise.} \end{cases} \tag{5}$$

Here, $Mig_{it} = 0$ if the individual did not get a paid job (either because they are unemployed or because they decided to stay in the agricultural sector) or if the individual stays in the town for a paid job in year t. Since we only concentrate on the 2015 stayers, Mig_{it} can be seen as the *change* in their migration decisions in year t, after 2015.

can be viewed as *Type B*, since there is no road directly crossing the border. For our estimation we only use Type A pairs of *connected* villages.

The reduced-form effects in 2018, with and without controls, are shown in Figure 4a–b for males and in Figure 4c–d for females. The graphic evidence reveals that there is a clear and significant jump at the village border between the treated vs control village for both genders, with and without controls.

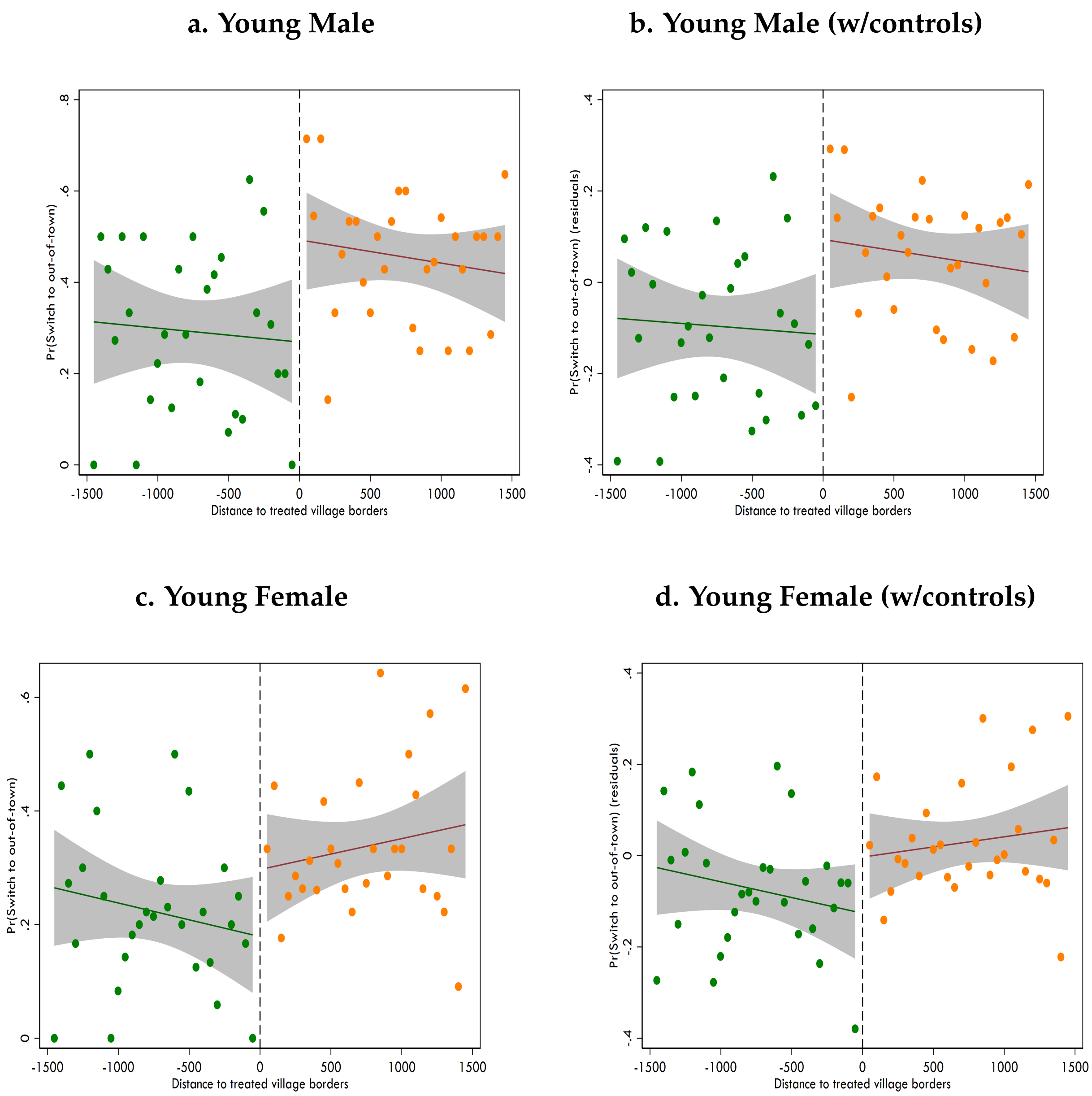


Figure 4: **Reduced-Form Effect of Institutional Reform on Location Switch**

5.2.1 Baseline results

We estimate the average treatment effect among the 2015 stayers for four group: (i) younger females (aged 16–50); (ii) younger males (aged 16–50); (iii) older females (aged 51–75);

and (iv) older males (aged 51–75). Panel A of Table 3 presents the results for the overall P-NP sample, with optimal bandwidths determined using the commonly employed method outlined by Calonico et al. (2019).[33] The findings reveal substantial impacts of the reform, particularly among younger age groups. In 2015, migration rates for young men stood at 0.59 and for young women at 0.216. Young male stayers experienced an effect of 0.262, amounting to an increase of more than 18% in the overall migration rate (or $0.11 = 0.262 * (1 - 0.59)$ in absolute value of the rate). For young women, the effect size was 0.163, equivalent to a 59% increase in the overall migration rate. When we narrow down our analysis to the CP–NP subsample for improved spatial comparison, the robustness of our results for younger individuals is confirmed, as shown in Panel B of Table 3. Additionally, the optimal bandwidth in this CP–NP subsample is nearly double that of the P-NP sample, indicating the robustness with regard to bandwidth selection.

Given the discrete nature of *Mig*, we conduct another robustness check using the Xu (2017) method for selecting bandwidth, which is tailored for discrete outcomes.[34] The results, presented in Panel B(2) of Table 3, demonstrate that our estimates remain robust, irrespective of the method employed for bandwidth selection, relative to the results in Panel B(1) of the table.

We additionally provide detailed estimates for the younger groups by year in Figure 5[35]. In Figure 5a–b, we present the results for those who were not working out of town in 2015, while in Figure 5c–d we provide the results for those who were engaged in the agricultural sector (in town) in 2015. Three important findings emerge. First, the comparability of people in treated and control villages is suggested by the similarities observed for all genders and age groups before treatment, confirming the validity of our identification strategy. Second, the overall effect of individuals leaving the agricultural sector is substantial and occurrs almost instantaneously. Third, the effect for women takes longer to emerge, which is not surprising given that females in the household are typically more engaged in household obligations and so leaving town would take more time for them to arrange. This is also consistent with the significant gender wage gap as well as age-based pay-gap observed in urban areas, as illustrated in Figure F.1 of Appendix F.

5.2.2 Robustness checks

We conduct several analysis for robustness check. In the first set of analyses, we repeated the previous exercise using different samples and migration definitions, the results of

[33]For the method used by Calonico et al. (2019), the results are obtained by *rdrobust* package, which implements local polynomial Regression Discontinuity (RD) point estimators with robust bias-corrected confidence intervals and inference procedures developed in Calonico et al. (2014) and Calonico et al. (2019). We use *epanechnikov kernel* function to construct the local-polynomial estimators, and the MSE-optimal bandwidth selector *msecomb1*.

[34]For the method employed by Xu (2017), we use the R package provided by the author

[35]For 2015, we use 2014 as the base year, i.e., we run the regression with the individuals who stayed in town in 2014

Table 3: **Regression Discontinuity for Location Switch in 2018**

	Male		Female	
	16-50 year-old	51-75 year-old	16-50 year-old	51-75 year-old
Panel A: P-NP Calonico et al. (2019) method:				
Conventional	0.238**	0.095**	0.135	-0.011
Std. Err.	(0.105)	(0.045)	(0.083)	(0.037)
Bias-corrected	0.262**	0.110**	0.163*	-0.003
Robust Std	(0.105)	(0.045)	(0.083)	(0.037)
Sample Size	1,463	2,131	2,036	2,022
$\hat{h}^R$	614.23	511.06	554.60	649.45
$\hat{h}^L$	491.50	501.42	498.13	627.75
$\hat{bw}^R$	1092.73	1079.22	998.13	1120.44
$\hat{bw}^L$	933.07	1066.71	919.74	1124.46
Panel B (1): CP-NP Calonico et al. (2019) method:				
Conventional	0.245**	0.098	0.177*	-0.012
Std. Err.	(0.120)	(0.062)	(0.092)	(0.038)
Bias-corrected	0.256*	0.119	0.198*	-0.012
Robust Std	(0.148)	(0.075)	(0.111)	(0.045)
Sample Size	852	1,265	1,181	1,210
$\hat{h}^R$	1037.50	987.39	886.01	939.01
$\hat{h}^L$	823.02	896.48	789.13	999.76
$\hat{bw}^R$	1761.83	1889.58	1522.91	1666.27
$\hat{bw}^L$	1460.37	1652.94	1538.59	1808.99
Panel B (2): CP-NP Xu (2017) method:				
$ATEs(\hat{\tau})$	0.244	0.031	0.132	-0.008
95% CI	[0.126 ,0.443]	[-0.086,0.097]	[0.068,0.239]	[-0.079,0.046]
t-test	3.516	0.112	3.507	-0.513
$\hat{h}$	904.4	917.3	1865.1	431.1
Wald test	12.364	0.013	12.300	0.264
Wald test p-value	0.000	0.911	0.001	0.608

Note: This table presents the RD regression results for *Mig* among those who stayed in the hometown in the base year, for different genders and age groups separately. In Panel A, we present the results with the optimal bandwidth calculated with Calonico et al. (2019) method and in Panel B, with Xu (2017) method. We control for the household-level and individual-level controls in all regressions.

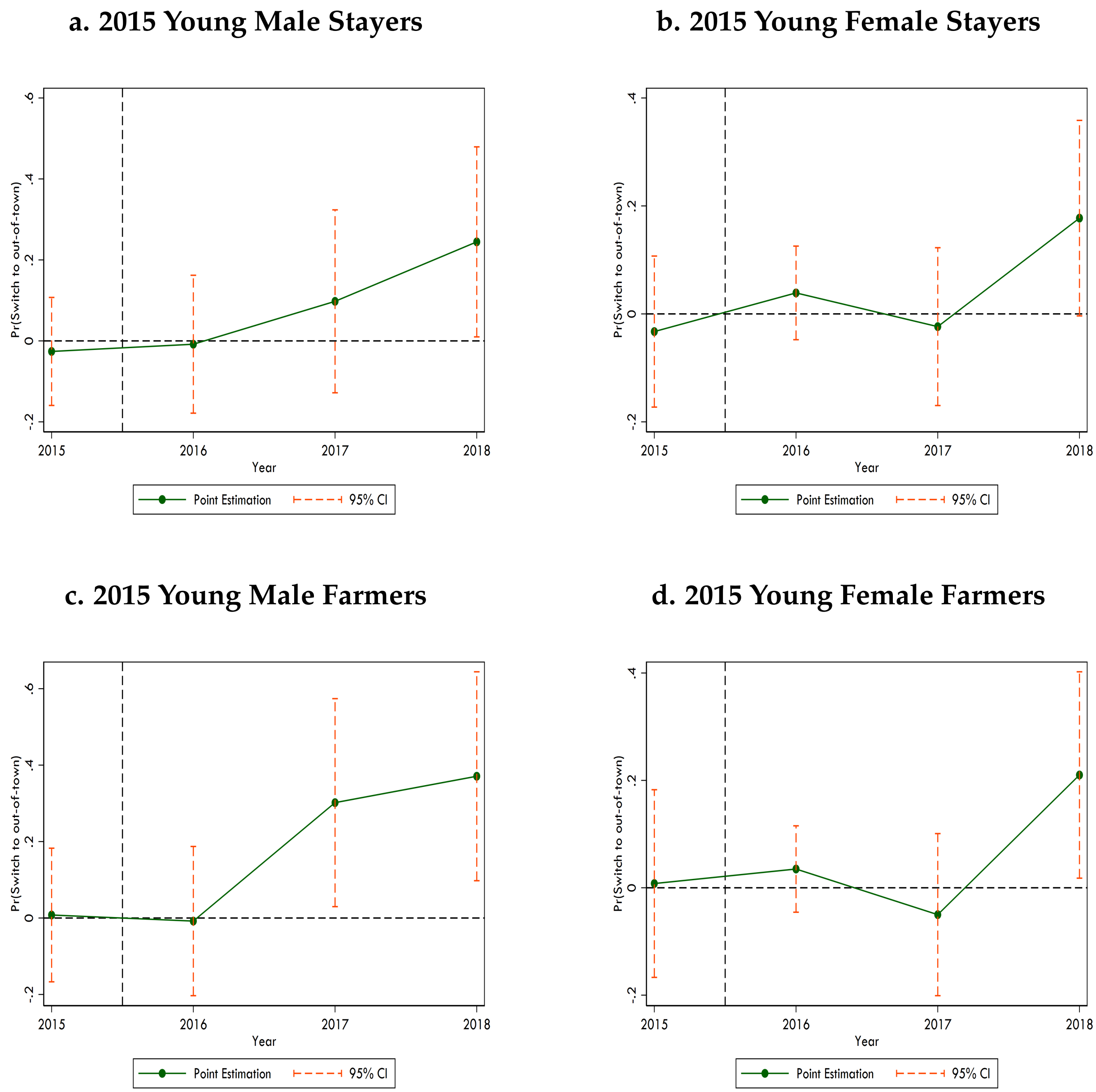


Figure 5: **Treatment Effects on Location-Switching by Year**

which are presented in Appendix F, Table F.1. In Panel A of the table, we concentrate on those who did not have a paid job in the base year, indicating their previous involvement in the agricultural sector. Therefore, the RD coefficient captures the transition from the agricultural sector to non-agricultural sectors out of town. The results are very similar for the young age groups. For the older males, the migration effect also becomes positively significant, at 0.165. In Panel B, we change the "urban area" to "out-of-county" instead of "out-of-town", and still find strong positive and significant effects for males of all age groups as well as for young females. Together, these results suggest that individuals in the treated villages are more likely to migrate and transition out of the agricultural sector. Furthermore, when people do migrate, they tend to move to areas outside of the county.

Second, we further define the temporary migration status to a paid job in an urban area among all individuals in 2018 by using the following equation:

$$LOC_i = \begin{cases} Town & \text{If individual } i \text{ stays in town and gets a paid job,} \\ Migrate & \text{If individual } i \text{ migrates to an urban area and gets a paid job,} \\ U & \text{No paid job for individual } i. \end{cases} \quad (6)$$

Thus, the coefficients estimated in the RD regression can be interpreted as the probability of working in town and out of town relative to being unemployed, which serves as the reference group. The results for the location, provided in Table 4, strengthen the argument made above regarding the destination. Both young males and females tended to migrate more in 2018, as indicated by the higher rates of migration reported in Panel B of Table 4 and the lower rates of working in rural areas (or within town) as reported in Panel A. Consistent with the previous results, the impact on young females is the largest of all.

Table 4: **Regression Discontinuity for Location Choice in 2018**

	Male		Female	
	16-50 year-old	51-75 year-old	16-50 year-old	51-75 year-old
Panel A—work in town (τ_1):				
$ATEs(\hat{\tau}_1)$	-0.0323	0.0500	-0.0857	-0.1096
95% CI	[-0.1258 , 0.0295]	[-0.0275 , 0.2212]	[-0.2100 , -0.0094]	[-0.2258, -0.0364]
t-test	-1.2148	1.5263	-2.1434	-2.7146
t-test p-value	0.2244	0.1269	0.0321	0.0066
Panel B—work out-of-town (τ_2):				
$ATEs(\hat{\tau})$	0.1564	0.0359	0.1903	-0.0375
95% CI	[0.0579 , 0.3139]	[-0.1004, 0.1553]	[0.0172, 0.2897]	[-0.1413 , 0.0233]
t-test	2.8468	0.4206	2.2081	-1.4046
t-test p-value	0.0044	0.6741	0.0272	0.1601
Reference Group: Unemployed				
Sample Size	2,109	1,678	1,507	1,294
$\hat{h}$	4454.878	653.145	637.684	636.483
Wald test	7.6905	0.5012	7.2515	8.3149
Wald test p-value	0.0214	0.7783	0.0266	0.0156

Note: This table presents the RD regression results for *LOC* for the whole labor force in 2018, for different genders and age groups separately. Given that the dependent variable is categorical, we calculate the optimal bandwidth following the method proposed by Xu (2017). The reference group is *Unemployed*, or does not have any paid job. Panel A shows the coefficients for *Town* compared to the *Unemployed* and Panel B for *Migrate* compared to *Unemployed*.

We further conduct a sensitivity analysis to test the robustness of our choice of the income cutoff at 10%. To do this, we repeat the previous exercise for different cutoffs ranging from 2% to 20%. We plot the RD results for all young stayers in 2015 with respect to these different cutoffs in Appendix F, Figure F.2. The sensitivity analysis indicates that our main findings are robust. The coefficients remain positive and significant for all cutoffs, and the size of the effect stabilizes after 5%. Moreover, in Appendix F, Table F.2, we compare the number of months worked by new migrants and all migrants and find no significant differences across village borders. This confirms once again that the main impact is on the binary decision to migrate and does not significantly alter labor supply behavior after migration.

5.3 Other Measurements—Income and Salary of the Household

The results presented above use individual-level data. However, it is typical in developing countries for migration and working decisions to be made at the household level (e.g., Stark & Bloom (1985)). Therefore, we further examine the role of the household and report the results in Table 5, using household income as another measure for households' outcomes that stem directly from the migration behavior of the members of the household. In the first two columns of Table 5, we present the results for the change in per capita self-earned income between 2015 and 2018, both with and without control variables. The control variables include the base year (year 2015) income levels, family characteristics, and household access to infrastructure.[36]

The results indicate that (around the borders) families in the villages that underwent the institutional reform have significantly higher income 3 years after initiation of the reform treatment. In 2018, the lowest 10% of families in the "poor" villages have over 1,700 CNY more self-earned income per year per person (see the robust RD estimator) relative to those in the untreated villages. This is almost 36% of the annual minimum living standard.[37] We decompose the overall effect on self-earned income into its two components, per capita self-earned salary and per capita farming profits. The results for salary are provided in columns 3–4 of Table 5, while the results for farming income are provided in columns 5–6. We find that most of the overall effect comes from significant improvement in self-earned salary income. Furthermore, there is no significant difference in farming income. This suggests that the increase in income stems primarily from the farmers switching from farming activities to salaried jobs.

In Appendix F, Figures F.3, we provide the graphical representations of each of the regression discontinuity estimates according to the type of income. All graphs are depicted as functions of the treatment threshold, i.e., distance from the village borders. Additionally,

[36]Specifically, we control for per capita self-earned income in 2015, direct transfer level in 2015, the total number of family members, and the composition of dependent members (including the number of children under 5 years old, children aged between 5 and 15 (students), and senior people over the age of 65). For household access to infrastructure, we use distance to the paved road in 2018 as well as change in distance to the paved road between 2015 and 2018 (in meters).

[37]In 2018, the minimum living standard for the rural population was 4,833 CNY per year per person. See (in Chinese): https://baijiahao.baidu.com/s?id=1623617583333507721&wfr=spider&for=pc

Table 5: **Sharp RD Results for Self-Earned Income in 2018**

	Overall income		Salary income		Farming profits	
Conventional	1,550.08*	1,557.09**	1,536.03*	1,500.76*	-16.64	1.40
	(828.60)	(789.13)	(838.54)	(788.66)	(125.14)	(132.724)
Bias-corrected	1,760.64**	1,722.66**	1,751.43**	1,663.13**	-37.37	8.74
	(828.60)	(789.13)	(838.54)	(788.66)	(125.14)	(132.72)
Robust	1,760.64*	1,722.66*	1,751.43*	1,663.13*	-37.37	8.74
	(983.51)	(931.26)	(1,003.85)	(936.09)	(147.18)	(156.03)
Controls	No	Yes	No	Yes	No	Yes
Observations	3,070	3,018	3,081	3,029	3,081	3,029

Notes: This table presents the RD results for family income. Columns 1–2 present the results for per-capita self-earned income, Columns 3–4 presents results for per-capita self-earned salary, and Columns 5–6 for per-capita farming profits (farming income - farming cost). Overall income is the sum of salary income and farming profits. The control variables include base year income level (per capita self-earned income in 2015) and direct transfer level (per capita total direct transfer in 2015), family structure measurement (total number of family members, and the composition of dependent members, including the number of children under 5 years old, children between 5-15 (students), senior people over 65) and household access to infrastructure (distance to the paved road in 2018 and change of distance to the paved road from 2015 to 2018 (meters)).

we provide the same type of information for the residuals from the regressions of the growth of the various household incomes after controlling for all the variables listed above.

We conduct additional robustness checks to further assess the pre-treatment similarities between the treated and untreated villages. To do this, we use the income level in each year as the dependent variable instead of the change in income relative to the base year and repeat the previous exercise for each year from 2014 through to 2018.[38] The results of this exercise, reported in Appendix F, Figure F.4, indicate that households around the selected village boundaries were highly comparable in 2014 and 2015, prior to the onset of the treatment. This comparability is essential to our analysis, as it is a necessary assumption for the SRRD. This suggests that the estimates reported above can indeed be interpreted as causal effects, resulting directly from the introduction of the institutional reform. Additionally, we can clearly see that the income gap widens after 2016, ultimately causing a complete divergence between the two groups of households around the border in 2018.

[38]For years 2014 and 2015, we exclude the control variable *base year income level* since this variable is now the dependent variable. The selections of optimal bandwidths for estimation and bias correction are reported in Table F.3 of Appendix F.

6 Mechanism analysis

In the previous two sections, we provided strong evidence for the presence of favoritism in the allocation of welfare transfers in rural China. We also established that favoritism declined significantly after the introduction of the reform. Furthermore, there was a shift in individual migration choices after the reform. These findings suggest a close link between formal insurance and the migration decisions of individuals. It is important to note, though, that our analysis is confined to one county, thus limiting the possible alternative impacts of unobservable institutional and cultural confounding factors. Nevertheless, this does not definitively establish a casual link between formal insurance and rural-urban migration. This section, then, is devoted to establishing that this relationship is indeed causal. We illustrate the potential paths to demonstrate the causal link in Figure 6 and examine each potential path at a time.

We first provide direct evidence of the link between the treatment effect on migration and the extent of reduction in favoritism. We then use an innovative proxy for the reduction in favoritism levels to overcome the issue of the inherent measurement error issue in favoritism. Finally, we conduct comprehensive analyses of other potential links between the policy change and increased migration.

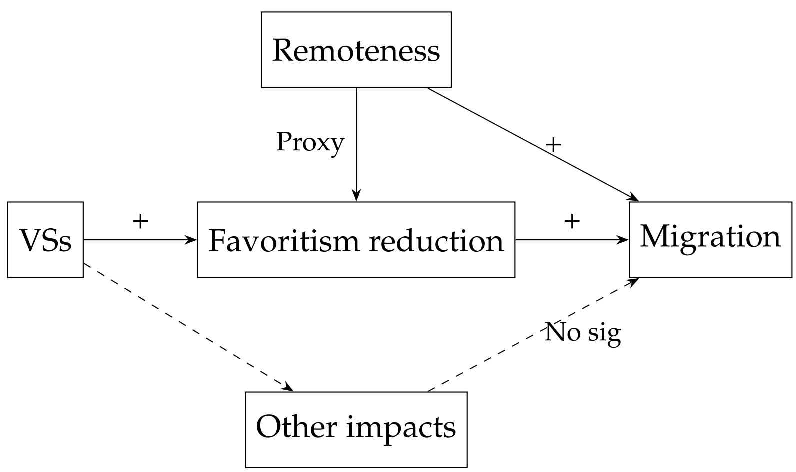


Figure 6: **Logic to Identify the Favoritism Reduction Impact on Migration**

6.1 Relationship between favoritism reduction and migration

To establish the direct link between the favoritism reduction and migration decisions, we employ the unique extensive administrative data sample to first estimate the degree of the reduction in favoritism at the village border level. Specifically, we implement the Basic Living Allowance allocation regression in 2 at the border level of each village. This, in turn, provides us with border-specific coefficients α_{mn}. An estimated marginal effects $\tilde{\alpha}_{mn}$ capture the reduction in favoritism between the connected households in the treated village m relative to those in the untreated village n that shares the same border. A more negative estimated marginal effect indicates a larger reduction in favoritism.

In Figure 7, we present a graph of the ordered values of all estimated border-level coefficients $\tilde{\alpha}_{mn}$, i.e., the estimated favoritism reduction, along with 95% confidence intervals, against their rank order. Figure 7 highlights considerable variation in the magnitude of the favoritism reduction across all borders of P–NP village pairs. Approximately 82% of these pairs estimates are negative, indicating a substantial reduction in favoritism. Moreover, over 62% of these estimates are statistically significant at the 95% confidence level, suggesting that the VS reform had a meaningful impact on reducing favoritism.

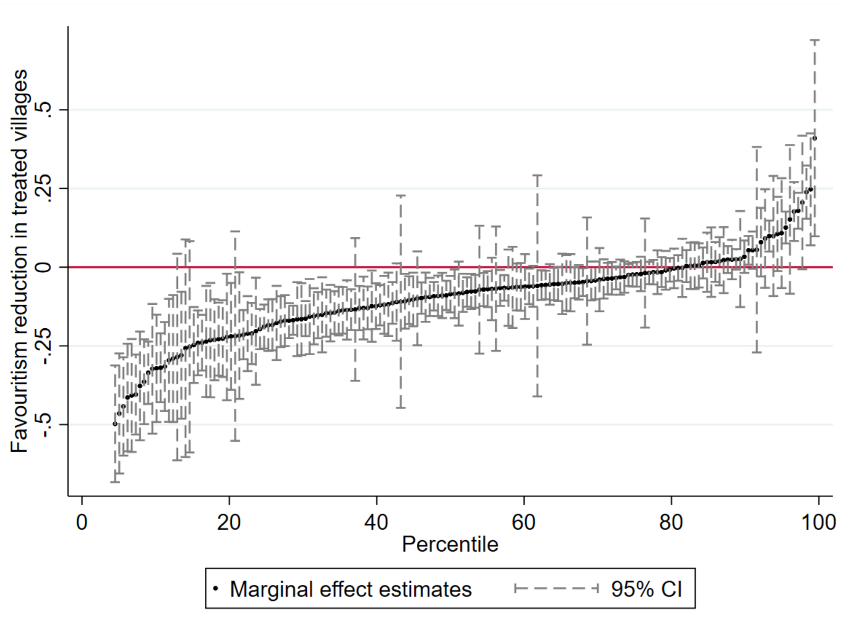


Figure 7: **Distribution of Border-Specific Favoritism Reduction**

Table 6 displays the RD regression results of rural-urban migration choices, namely *Mig*, in the post-treatment period of 2017–2018 for young males. The analysis is based on the estimated coefficients of favoritism reduction[39] In columns 1–2, we present the regression results for all estimated coefficients. These results indicate that the reduction in favoritism has a direct and significant effect on migration. When we restrict our estimation to the lowest coefficient estimates in absolute terms, as shown in columns 3–4 (labelled Top 5% effective), the effect is very large and significant. Meanwhile, we obtain similar results, but not at the same magnitude, when we use all negative estimates for the favoritism reduction (columns 5–6). In contrast, when we narrow our focus to villages where the effect on favoritism was not effective, namely for those with positive estimates for the favoritism effect (columns 7–8), the migration effects are statistically insignificant and are markedly smaller than for the other two groups considered. These findings clearly imply that the effect of migration stems directly from the change in favoritism and not from

[39] We pool the post-treatment periods together to attain a larger sample size. This data pooling is further justified by our previous year-by-year RD results that young males experience positive migration effects immediately after treatment initiation in 2017.

the mere presence of the VSs in the villages. Thus, the observed average migration effect presented previously can be attributed to the substantial impact witnessed in villages with effective favoritism reduction. We also conducted a similar exercise for the young females in our sample. The results, reported in Appendix Table F.4, lead to the same conclusion for young males reported here.

Table 6: **Heterogeneous Migration Effect by Degrees of Favoritism Reduction**

VARIABLES	(1) Overall	(2)	(3) Group by effectiveness on favouritism reduction: Top 50% effective	(4)	(5) All effective	(6)	(7) Not effective	(8)
Conventional	0.196***	0.217***	0.594***	0.562***	0.258**	0.249**	0.138	0.168
	(0.064)	(0.068)	(0.162)	(0.168)	(0.104)	(0.100)	(0.102)	(0.104)
Bias-corrected	0.222***	0.245***	0.670***	0.626***	0.298***	0.290***	0.163	0.196*
	(0.064)	(0.068)	(0.162)	(0.168)	(0.104)	(0.100)	(0.102)	(0.104)
Robust	0.222***	0.245***	0.670***	0.626***	0.298**	0.290**	0.163	0.196
	(0.076)	(0.080)	(0.194)	(0.198)	(0.122)	(0.119)	(0.126)	(0.128)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2,868	2,859	777	777	1,705	1,702	1,163	1,157

Notes: This table presents the RD regression results for Mig among young males for the post-treatment period of 2017-2018, stratified by different levels of favoritism reduction. Columns 1–2 display results for the overall sample. Columns 3–4 focus on the sub-sample for which VS treatments are among the upper 50th percentile in effectiveness, as measured by a border-specific marginal effect $\alpha_{\tilde{mn}}$ that exceeds the median across all poor-nonpoor village borders. Columns 3–4 present results for all young males around borders where $\alpha_{\tilde{mn}} < 0$ significantly, indicating the VS treatment is effective. Lastly, Columns 5–6 present results for the sub-sample with $\alpha_{\tilde{mn}}$ not significantly different from 0, i.e. the VS treatment not effective at all. For each subgroup, we provide results both with and without family-level and individual-level controls.

It is important to note that the SRRD regressions reported in Table 6 use coarse categorizations of groups, which means they do not use the exact value of the continuous variable for favoritism estimates. To address this issue, we adopt the method suggested in (Hsu & Shen, 2019). In short, this method defines the Conditional Local Average Treatment Effect $CLATE(w)$, based on a given continuous covariate w. Their proposed testing procedure makes it possible to then more efficiently test the effect of favoritism reduction (namely, $-\tilde{\alpha}_{mn}$) on migration effects. Following their recommendation, we test the null hypotheses $CLATE(\tilde{\alpha}) \geq 0$ and $CLATE(\tilde{\alpha}) = LATE$. Rejection of these two hypotheses provides additional, stronger statistical evidence that favoritism reduction does indeed affect migration directly, and that this relationship is positive. That is, a greater reduction in favoritism leads to greater migration.

Table 7 reports the p-values for the uniform non-negativity $H_0 : CLATE(\tilde{\alpha}) \geq 0$, and the monotonicity test, $H_0 : CLATE(\tilde{\alpha}) = LATE$. In Panel A, all the border-level marginal effect estimates $\alpha_{\tilde{mn}}$ are used directly to represent the degree of favoritism reduction. For the robustness check in Panel B, we report the results when we use the statistically significant $\alpha_{mn}^{\tilde{}}$ along with values of 0 assigned to all the insignificant estimates. Moreover, we adopt the bootstrap method proposed by Hsu & Shen (2019) to simulate critical values for the test. Also, for the uniform tests, we follow Hsu & Shen (2019) by reporting two alternative choices for the test's critical value, namely the Least Favorable Configuration (LFC) and the moment selection method (GMS). We use 1,000 bootstrap repetitions for the computation of the bootstrap critical values.[40] The results are very clear: both hypotheses are rejected as indicated by the very low p-values. Furthermore, this finding is not sensitive to either the bandwidth used or to the method used in the construction of the test's critical value. This further strengthens our finding that larger migration flows are directly linked to more effective favoritism reduction.

Table 7: **Heterogeneity tests with respect to favouritism reduction**

	Null: $CLATE(\tilde{\alpha}) \geq 0$			Null: $CLATE(\tilde{\alpha}) = LATE$		
	$c < bw^*$	$c = bw^*$	$c > bw^*$	$c < bw^*$	$c = bw^*$	$c > bw^*$
Panel A: Marginal effect $\tilde{\alpha}$						
(1) Least Favorable Condition (LFC)						
p-value	0.018	0.006	0.008	0.006	0.000	0.000
(2) Generalized Moment Selection (GMS)						
p-value	0.016	0.006	0.008			
Panel B: Significant marginal effect $\tilde{\alpha}$						
(1) Least Favorable Condition (LFC)						
p-value	0.038	0.042	0.022	0.022	0.012	0.014
(2) Generalized Moment Selection (GMS)						
p-value	0.028	0.038	0.022			

Notes: This table presents nonparametric heterogeneity tests with respect to favoritism reduction, using the method proposed by Hsu & Shen (2019). Columns 1–3 display results for uniform non-negative tests, while Columns 4–6 for monotonicity tests. The optimal bandwidth, referred to as bw^*, is derived from Calonico et al. (2014). The columns to the left and right of this optimal bandwidth use c values of 4.5 and 5, respectively, resulting in bandwidths lower or higher than bw^* as calculated by $bw^* \times n^{1/5-1/c}$. Panel A directly uses the border-level marginal effect $\tilde{\alpha}$ to quantify the degree of favoritism reduction, while Panel B additionally resets non-significant $\tilde{\alpha}$ estimates to zero. Critical values for the uniform non-negative test are computed using both Least Favorable Configuration (LFC) and Generalized Moment Selection (GMS) methods. All bootstrap critical values are derived from 1,000 bootstrap simulations.

[40]To ensure robustness, we also consider three bandwidths according to the formula $bw * \times n^{1/5-1/c}$, where $bw*$ represents the optimal robust bandwidth proposed by Calonico et al. (2014). Here, $c < 5$ and $c > 5$ give bandwidths that are smaller or larger than $bw*$.

6.2 Other measurements of favoritism reduction

6.2.1 Village remoteness as a proxy for favoritism reduction

The previous measurement of favoritism reduction is limited in its level of precision due to the small sample sizes in the granular border-level regressions.[41] To address this issue, we use an alternative proxy variable for favoritism: village remoteness. First, it's reasonable to believe that village remoteness is positively correlated with favoritism reduction through the VS reform. In developing regions, such as China (Martinez-Bravo et al., 2022) and African hinterlands (Mamdani, 1996), there is limited government reach in remote areas, due to administrative constraints, inadequate infrastructure, and challenging geography. Our first-stage results, showing a significant reduction in favoritism due to VS reform, also support this narrative. This leads to an important hypothesis: institutional reforms designed to bring governance to villages are likely to have a more pronounced impact in remote areas that are situated farther from county or town centers. Figure 8 provides graphs in which we plot the estimated marginal effects of the VSs, i.e., $\tilde{\alpha}_{mn}$, against the distance to town and county centers. Indeed, we see that the more remote villages experienced larger reductions in favoritism (i.e., $\tilde{\alpha}_{mn}$ is more negative the longer the distance to town and/or county centers).

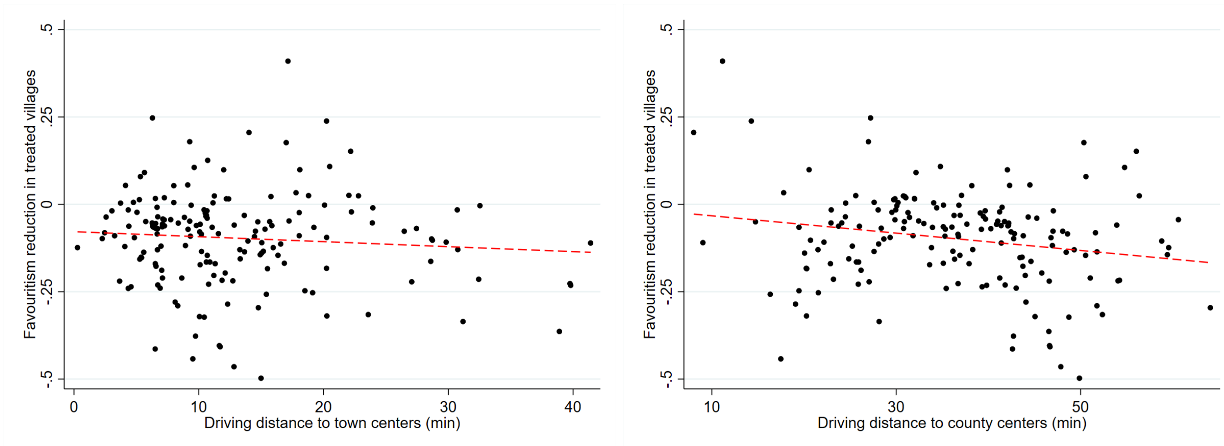
Another advantage of this proxy is that it is negatively correlated with most other factors that may induce migration, such as peer effects, role model effects, or relaxed budget constraints due to larger transfer levels during the treatment period. Given that migration costs tend to increase with remoteness, one would expect a diminishing reform effect as remoteness rises if factors other than favoritism reduction are the driving forces behind the rising migration. In other words, rejecting the null hypothesis that the migration effect is non-positive with respect to remoteness provides further support for the fact that migration is a result of favoritism reduction. We formally explore this hypothesis next.

We start with sub-sample regression method and partition the villages into two groups: those that are in close proximity to the town center and those farther away, based on driving time, measured in minutes, to the town centers.[42] For a robustness check, we also use the distance to the county center as an alternative partitioning criterion. Table 8 displays the results of the RD regression for these sub-samples, which indicate that young males residing in more distant villages experience a notably stronger impact on migration. This pattern is very persistent for the two stratification criteria and model specifications. In contrast, the effect for those in villages that are closer to town (or county) centers is much smaller in magnitude and the significance of the results are not robust across the various model configurations.

Next, we conduct the heterogeneity test described above. We report the results of these

[41] Specifically, in the border-level regressions, the unique cross-sectional units vary from 140 to 500, and each regression includes between 420 and 1,500 observations.

[42] The mean distance is 8.06 km, equivalent to 12.72 minutes of driving on a typical day, with a median distance of 7.14 km.



(a) Distance to town centers

(b) Distance to county center

Figure 8: **Remoteness as a Proxy for Favoritism Reduction**

Table 8: **Heterogeneous Migration Effect by Degrees of Favoritism Reduction**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Distance to Town Centers				Distance to County Center			
	Close		Farther away		Close		Farther away	
Conventional	0.165*	0.146*	0.540***	0.408***	0.146*	0.132	0.245***	0.290***
	(0.091)	(0.086)	(0.188)	(0.134)	(0.084)	(0.096)	(0.093)	(0.098)
Bias-corrected	0.183**	0.161*	0.608***	0.451***	0.145*	0.127	0.283***	0.333***
	(0.091)	(0.086)	(0.188)	(0.134)	(0.084)	(0.096)	(0.093)	(0.098)
Robust	0.183*	0.161	0.608***	0.451***	0.145	0.127	0.283**	0.333***
	(0.105)	(0.102)	(0.210)	(0.157)	(0.101)	(0.115)	(0.114)	(0.117)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1,627	1,621	1,393	1,390	1,394	1,391	1,600	1,594

Notes: This table presents the RD regression results for *Mig* among young males for the post-treatment period of 2017-2018, stratified by different village remoteness. Columns 1–4 display results for the overall sample. Columns 3–4 focus on the sub-sample for which VS treatments are among the upper 50th percentile in effectiveness, as measured by a border-specific marginal effect $\alpha_{\tilde{mn}}$ that exceeds the median across all poor-nonpoor village borders. Columns 3–4 present results for all young males around borders where $\alpha_{\tilde{mn}} < 0$ significantly, indicating the VS treatment is effective. Lastly, Columns 5–6 present results for the sub-sample with $\alpha_{\tilde{mn}}$ not significantly different from 0, i.e. the VS treatment not effective at all. For each subgroup, we provide results both with and without family-level and individual-level controls.

tests in Table 9 for the uniform non-positive tests in columns 1–3 and the monotonicity tests in columns 4–6. As in the analysis reported in Table 7, we consider three alternative bandwidths and two alternative construction methods for the test's critical value. The results robustly reject the null hypothesis that the migration effect is negatively correlated with remoteness or that there is no correlation between the migration effect and remoteness across all setups at a very low significance level.

Table 9: **Heterogeneity Tests with Respect to Village Remoteness**

	Null: $CLATE(\tilde{\alpha}) \leq 0$			Null: $CLATE(\tilde{\alpha}) = LATE$		
	$bw < bw^*$	$bw = bw^*$	$bw > bw^*$	$bw < bw^*$	$bw = bw^*$	$bw > bw^*$
Panel A: Distance to town centers						
(1) Least Favorable Condition (LFC)						
p-value	0.000	0.000	0.000	0.002	0.002	0.000
(2) Generalized Moment Selection (GMS)						
p-value	0.000	0.000	0.000			
Panel B: Distance to county center						
(1) Least Favorable Condition (LFC)						
p-value	0.016	0.008	0.004	0.002	0.000	0.002
(2) Generalized Moment Selection (GMS)						
p-value	0.014	0.006	0.004			

Notes: This table presents nonparametric heterogeneity tests with respect to favoritism reduction, using the method proposed by Hsu & Shen (2019). Columns 1–3 display results for uniform nonnegative tests, while Columns 4–6 for monotonicity tests. Each test is conducted with varying smoothing parameters c and their corresponding bandwidths bw. The optimal bandwidth, referred to as bw^*, is derived from Calonico et al. (2014). The columns to the left and right of this optimal bandwidth use c values of 4.5 and 5, respectively, resulting in bandwidths lower or higher than bw^* as calculated by $bw^* \times n^{1/5-1/c}$. Panel A directly uses the border-level marginal effect $\tilde{\alpha}$ to quantify the degree of favoritism reduction, while Panel B additionally resets non-significant $\tilde{\alpha}$ estimates to zero. Critical values for the uniform non-negative test are computed using both Least Favorable Configuration (LFC) and Generalized Moment Selection (GMS) methods. All bootstrap critical values are derived from 1,000 bootstrap simulations.

6.2.2 Favored vs. non-favored family clans

Another variation in the efficacy of formal insurance that can be explored is one's affiliation with a village cadre's family clan. As favoritism decreases, we would expect the social safety net to improve for the clans who were previously discriminated against (the non-favored). In contrast, we would expect the insurance coverage for the favored clans to remain unchanged at best, or even to worsen.[43] Table 10 provides the results from this exercise. Panel A, columns 1–2 present separate RD regressions for individuals sharing the same surnames with village cadres and those who do not. The treatment effect of the reform is notable only for those with different surnames—a group that was initially not favored—and the magnitude is considerably higher compared with their favored counterparts. For robustness, Panel B presents the results when the family clans are identified according to whether any member of the household has the same surname as the cadre. The results remain largely unchanged.

To determine whether this inter-clan disparity is significant, we utilize the *diff-in-disc* method proposed by Grembi et al. (2016). This combines the traditional DID and RD approaches, assuming that only the individuals with a surname different from the cadre's surname are affected, while those with the same surname serve as a control group. The results of this examination, as shown in columns 3–4 of Table 10, confirm two important facts. First, the reform effect is indeed significantly stronger for the non-favored group. The coefficient on the interaction term for the non-favored (i.e., the "DID" coefficient) is highly positively significant. Second, there are substantial differences in the migration rates between different family clans in untreated villages. This provides further support for the link between migration choices and the pre-existing favoritism and thus the village-level formal insurance efficacy.

In addition, this result reinforces the causal relationship between the formal insurance efficacy and migration, even when taking into account any possible shift in corruption during the reform. The logic is that, even if VSs introduced corruption in other forms, the level of favoritism concerning the village cadres has been reduced, resulting in varying treatment intensities across various family clans in terms of the formal insurance efficacy. Hence, the heterogeneous effects on migration exhibited by these family clans provide direct evidence for the causal link we seek to establish.

6.3 Excluding Other Possible Mechanism

If one VS is effective at reducing favoritism within the assigned village, he/she might also be effective in other aspects. Consequently, the correlation we previously established between favoritism reduction and migration effects potentially encompasses the underlying working mechanism.

One key advantage of utilizing village-level data as opposed to nationwide data is that the administrative authority of village leaders is confined primarily to the allocation of welfare resources. This is due to the fact that all funds related to the various policy supports and welfare programs originate solely from county-level, or higher, government authorities. Hence, by exploring the potential influence of VSs on various other aspects of villages, we

[43] Even though favored family clans might see a decline in government transfers *level* due to the redistribution of welfare, the reform's goal of equitable distribution based on the same criteria suggests that these clans are still adequately insured in the post-reform period.

Table 10: **Heterogeneous Migration Effect by Family Clans**

VARIABLES	(1)	(2)	(3)	(4)
	Sub-sample regression		RD-DID	
	non-favored = 0	non-favored = 1	baseline	with controls
Panel A: Individual's surname				
Treated =1	0.045	0.233***	-0.144	-0.184
	(0.104)	(0.087)	(0.120)	(0.113)
non-favored = 1			-0.245**	-0.220**
			(0.112)	(0.107)
non-favored*Treated			0.385***	0.374***
			(0.143)	(0.138)
Observations	1,027	1,993	1,327	1,325
R-squared			0.021	0.065
Panel B: Family members' surnames				
Treated =1	0.071	0.284***	-0.048	-0.068
	(0.110)	(0.093)	(0.088)	(0.091)
non-favored = 1			-0.342***	-0.281***
			(0.107)	(0.105)
non-favored*Treated			0.356**	0.298**
			(0.108)	(0.102)
Observations	1,382	1,638	1,211	1,209
R-squared			0.028	0.074

Notes: This table presents the RD regression results for *Mig*, dividing individuals based on whether they share surnames (Same = 1) or not (Same = 0) with village cadres; these results are displayed in Columns 1–2. Subsequently, Columns 3–4 offer results utilizing the *diff-in-disc* methodology as suggested by Grembi et al. (2016), both with and without control variables. In alignment with the approach of Grembi et al. (2016), the bandwidths used in the *diff-in-disc* analyses are calculated as the average of the optimal bandwidths from the separate RD regressions for the two distinct groups. Panel A classifies these groups based on the individual's own surname, whereas Panel B makes the classification based on the surnames of household members.

can rule out alternative mechanisms.

6.3.1 Other Village Treatment—Local Economic Activities

One potential channel to consider is that the VSs are also effective in boosting the local economies of villages. While all public infrastructure is theoretically available to all residents on both sides of any given border in the county, firms operating in a particular village may exhibit favoritism toward residents of the villages where they are located. This, in turn, could induce, *de facto*, discontinuity in job opportunities at the village borders[44].

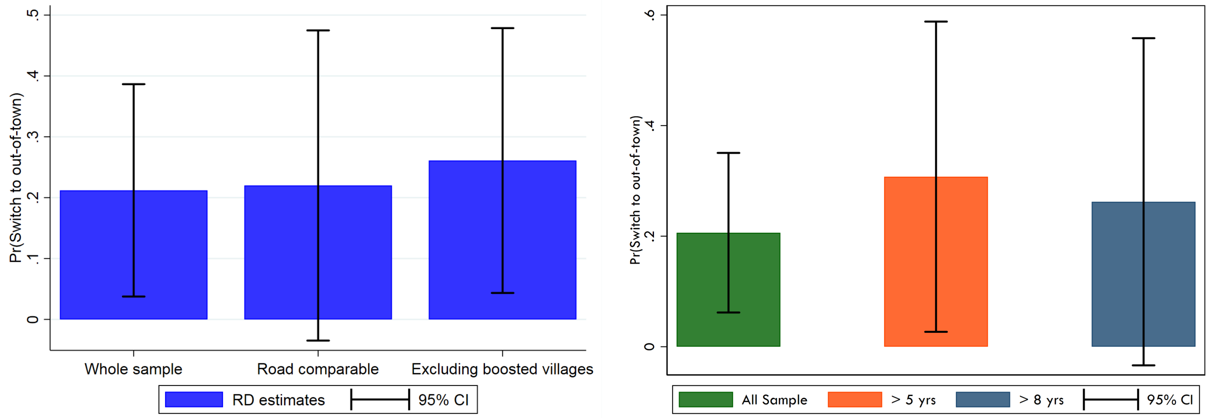
However, it is unlikely that the boost in local economies is the driving force behind the results we obtained. This is for two reasons. First, in the main results provided above, we observe a larger migration flow from the treated villages. There is little theoretical or empirical evidence to suggest that increased local labor demand resulting from more firms in a village would have a significant impact on the migration of villagers to urban areas. If anything, the impact should have the opposite effect. Secondly, we demonstrate in a companion study (F. Li et al. (2023)) with the village non-residential electricity usage data and firm registration data that the level of corruption in rural China does not have an impact on the overall economic activities but rather on the distribution of firms to different family clans. Stated differently, any increase in local labor demand in the short run can be seen as random with the VS treatment, conditional on village characteristics. Consequently, it cannot have any meaningful effect on any of the results obtained.

To further test the robustness of our results, we exclude villages that experienced an economic boost, i.e., villages that experienced an increase in total non-residential electricity usage between 2015 and 2018 that is above the median[45].

Figure 9 (a) compares the RD results for all young adults between the whole sample and a sub-sample of villages that excludes those that underwent an economic boost. The results for the two samples are very similar, which confirms the validity of our previously reported estimate and supports the conclusion that the estimated effect can be attributed to the reduction in corruption level and not to any additional economic improvements. Appendix F, Table F.6 provides detailed results of the SRRD regression for the sub-sample of villages that excludes those that experienced significant economic boosts.

[44] We collected the rosters of the employees in most of the firms operating in our sample county and found that 91.5% of them came from the villages in which they operate. That is, it appears that there is strong favoritism toward individuals from the same village in which the firm operates.

[45] According to the statistics of non-residential electricity consumption, the median was 33% during this three-year period. In Appendix D, Figure D.3, we depict the distribution of all types of villages across the county. Specifically, we provide a classification of all villages, by their treatment status, as well as whether they were exposed to a local economic boost. The geographical distribution of different types of villages appears to be completely random across the sample county.



(a) Other village treatments

(b) Role model effect

Figure 9: **Excluding other Possible Explanations**

6.3.2 Other Village Treatment—Road Connection

Another concern that could potentially harm the validity of our identification strategy is the possibility that treated villages may receive better improvements in infrastructure, like roads, due to personal connections between VSs and higher-level government officials, which in turn could lead to increased resource allocation from the upper government. Despite the similar access to main roads and other amenities near the borders in our CP–NP sub-sample, we cannot rule out the role of smaller intra-village roads. These could ease daily tasks or agricultural labor and henceforth free up younger individuals for migration. With exact house location and road GIS information in 2015 and 2018, we are able to calculate the distance of each household to the nearest paved roads for these two years. Based on the distances in 2015 and 2018, we can categorize the CP–NP borders based on whether there are significant differences in paved road accessibility improvement on both sides of the border. We can then use a sub-sample of borders where improvements in road accessibility on both sides are comparable, thereby isolating the impact of VSs from that of road improvement[46]

Figure 9 (a) compares the RD results for all young adults with the results from the sub-sample of borders where the road accessibility improvements on both sides of the border are comparable. This comparison reveals that there is no visible difference between these two models, which further suggests that the effect comes from the institutional reform and not from other aspects that may be related to changes in the treated villages.

Appendix F, Table F.5 presents the detailed results for the sub-sample where the differences in road accessibility improvement on both sides of the border are comparable. To

[46]In order to do this, we conduct a pairwise RD regression for each border on ”road access” in 2018 (RAC_i^{2018}). We use all households i within a 1,000-meter range on each side of the border and control for the distance to the road in 2015 and family characteristics.

ensure robustness, two criteria for comparability are used: one is that the pairwise RD regression on road accessibility is insignificant at the 5% or 10% significance level, and the other is for villages where the absolute differences in road accessibility dummy are smaller than 0.2. We also consider the sub-sample where both conditions are met, i.e., the differences in road accessibility are lower than 0.2 and insignificant at the 5% significance level.

6.3.3 A Role Model Effect

The reform discussed above brought younger and more educated officials into village governance, as is evident in Appendix A, Figure A.1a and Figure A.1b, respectively. Thus, another potential channel could be that it is not the reform that led to the observed changes but rather the fact that the new officials in the treated villages may be serving as role models for their respective village's younger population, encouraging them to migrate. This is possible but highly unlikely given that in 2013 migration rates were already relatively high for both females and males (0.31 and 0.41 for the two groups, respectively), as is indicated in the descriptive statistics in Appendix D, Table D.1. Among the poor population, the migration rate for young adults was even higher, at approximately 0.59 in the base year. Rural-urban migration among the rural population has a history of over 30 years in China, so it is hardly the case that village supervisors served as role models to those who stayed. Furthermore, as highlighted in the institutional background, every poor household, regardless of whether they're in treated villages, is paired with a social worker who visits and gathers information. As a result, all these households have a connection to an educated individual who could potentially serve as a role model.

To further evaluate the robustness of our results, we explore whether the effect varies across individuals with traits that are closer to the village supervisors. We hypothesize that if the results were driven by having a role model, then the effect should be larger for individuals who are more similar to the designated supervisor. Specifically, we classify individuals by their age difference from their appointed village supervisor. The RD estimates on migration for different groups, which are provided in Figure 9, reveal no discernible patterns with respect to age difference. Indeed, the results remain virtually unchanged when we change the cutoff in age difference to 8 years.[47]

6.3.4 VSs' personal networks

Another potential mechanism could be the personal network effects of each VS. That is, in principle, each VS can leverage their personal networks to assist residents of their assigned villages to help them migrate. However, this seems unlikely given that the majority of VSs are from county-level governments. In the P–NP sample we use here, all the VSs come from within the county. Our results indicate that the most pronounced effect is on out-of-county migration. Thus, given the nature of their position in the county government,

[47] While one would also like to check whether the gender of supervisors has an asymmetric impact on individuals of different genders, we cannot credibly conduct this analysis. This is because there are only 7 female supervisors in the 73 treated villages.

it seems implausible that they would systematically have influential networks that extend well outside of the county. Indeed, this is illustrated in Figure ??, from which it can be seen that there are three potential migration destinies: ”out-of-town”, ”out-of-county”, and ”out-of-province”. Clearly, the migration effect is more pronounced for broader geographic scopes. This suggests that when formal insurance incentivizes rural residents to leave their home villages, they look for the best possible economic opportunities. These cause them them to migrate to areas that are quite distant from their home county.

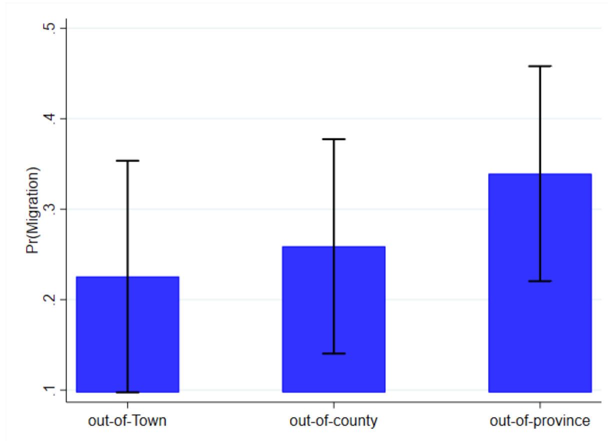


Figure 10: **Different Definition of Migration**

7 Conclusions

In this paper, we address a crucial question in economics, particularly in developing countries: *To what extent can formal insurance affect individuals' migration choices*? While there have been many attempts to improve welfare by enhancing the social safety net, their impact on the spatial allocation of labor remains unclear. This issue is difficult to address, largely because of the endogenous nature of institutional formation and the limited variation in exogenous variable that are available for analysis.

In this study, we use unique data from a quasi-experiment setting in rural China to explore the village-level variation in the efficacy of social safety nets that result from random variation in the implementation of a policy reform. This setting makes it possible for us to establish a causal relationship between social safety nets and individuals' choices. We find that the improvements in the efficacy of formal insurance dramatically decreased the spatial labor misallocation by substantially increasing the migration of individuals to urban areas. This effect is especially pronounced for individuals who were not likely to benefit from any favoritism. It is also significant for those living farther from town and county centers, who were more exposed to inadequate bureaucratic capacity before the reform. The substantial rise in migration flows has contributed to increased family income and played a key role in driving poverty alleviation.

Our study also contributes to the quantification of alternative factors affecting migration decisions. While recent research has emphasized the monetary costs of migration, such as transportation and visa expenses, and liquidity constraints, our findings highlight the impact of enhanced formal insurance and the significance of non-monetary costs, such as risks, in decision-making. This result also aligns with the relative large structural estimation of catch-all cost term to justify a reluctance of more migration. We suggest potential policy measures to increase rural-urban migration likelihood, including strengthening social safety nets through government or industry efforts and implementing social welfare programs to support transitioning populations.

It is worth emphasizing, too, that even though our quasi-experiment focuses on one representative county in China, the treatment under study—the mitigation of local corruption and the implementation of a social safety nets—is a common issue in developing countries or regions receiving foreign aid. Therefore, our results offer insights applicable to a broader range of contexts than just China.

In addition, we acknowledge that we have not examined the costs of implementing the reform. The reform under study comes at substantial personnel costs for county officials serving as VSs and their team members and relocating the VSs from their original appointments to remote villages cannot be sustained indefinitely.[48] Yet, such manpower involvement in poverty reduction has been noted to be vital, such as by A. Banerjee et al. (2015) in the "Graduation Program" and Roelen et al. (2019) in the Chemen Lavi Miyo (CLM) program in Haiti. Most developed nations have a well-functioning social service infrastructure with social workers, claim examiners, and audit specialists. Their roles are akin to the role that the VSs and their teams play in our study. For instance, U.S. federal funding for social services amounted to $17.6 billion in 2023. When it comes to shaping policies to bolster the efficacy of formal insurance, nations should consider their unique contexts and conduct thorough cost-benefit assessments. While further research is still in order, our paper provides a clear direction for ongoing investigations.

[48] In the villages from our sample, the average distance from village centers to the county center is roughly 26km, translating to a 38-minute drive. Thus, the transportation cost itself is enormous, let alone other costs of being away from their main position etc.

References

Acemoglu, D., Johnson, S., & Robinson, J. A. (2005). Institutions as the fundamental cause of long-run growth. *Handbook of economic growth*, *1*, 385–472.

Ahmad, E., Drèze, J., Hills, J., & Sen, A. (1991). *Social security in developing countries*. Oxford University Press.

Alatas, V., Banerjee, A., Hanna, R., Olken, B. A., & Tobias, J. (2012). Targeting the poor: evidence from a field experiment in indonesia. *American Economic Review*, *102*(4), 1206–40.

Alatas, V., Purnamasari, R., Wai-Poi, M., Banerjee, A., Olken, B. A., & Hanna, R. (2016). Self-targeting: Evidence from a field experiment in indonesia. *Journal of Political Economy*, *124*(2), 371–427.

Alpermann, B. (2003). An assessment of research on village governance in china and suggestions for future applied research. *EU-China Training Programme on Village Governance*.

Ashraf, N., & Bandiera, O. (2017). Altruistic capital. *American Economic Review*, *107*(5), 70–75.

Banerjee, A., Duflo, E., Goldberg, N., Karlan, D., Osei, R., Parienté, W., ... Udry, C. (2015). A multifaceted program causes lasting progress for the very poor: Evidence from six countries. *Science*, *348*(6236), 1260799.

Banerjee, A. V., & Newman, A. F. (1998). Information, the dual economy, and development. *The Review of Economic Studies*, *65*(4), 631–653.

Beegle, K., De Weerdt, J., & Dercon, S. (2011). Migration and economic mobility in tanzania: Evidence from a tracking survey. *Review of Economics and Statistics*, *93*(3), 1010–1033.

Bryan, G., Chowdhury, S., & Mobarak, A. M. (2014). Underinvestment in a profitable technology: The case of seasonal migration in bangladesh. *Econometrica*, *82*(5), 1671–1748.

Bryan, G., & Morten, M. (2019). The aggregate productivity effects of internal migration: Evidence from indonesia. *Journal of Political Economy*, *127*(5), 2229–2268.

Calonico, S., Cattaneo, M., Farrell, M. H., & Titiunik, R. (2019). Regression discontinuity designs using covariates. *Review of Economics and Statistics*, *101*(3), 442–451.

Calonico, S., Cattaneo, M., & Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, *82*, 2295–2326.

Card, D., Johnston, A., Leung, P., Mas, A., & Pei, Z. (2015). The effect of unemployment benefits on the duration of unemployment insurance receipt: New evidence from a regression kink design in missouri, 2003-2013. *American Economic Review*, *105*(5), 126–30.

Chong, A., & Calder'on, C. (2000). Institutional quality and poverty measures in a cross-section of countries. *Economics of Governance*, *1*(2), 123–135.

Chuhan-Pole, P. (2016). *Africa's pulse spring 2016*. World Bank Publications.

Clemens, M. A. (2010). The roots of global wage gaps: evidence from randomized processing of us visas. *Available at SSRN 1629318*.

Datta, P. K. (2019). Exploring the dynamics of deliberative democracy in rural india: Lessons from the working of gram sabhas in india and gram sansads in west bengal. *Indian Journal of Public Administration*, *65*(1), 117–135.

Do, Q.-A., Nguyen, K.-T., & Tran, A. N. (2017). One mandarin benefits the whole clan: hometown favoritism in an authoritarian regime. *American Economic Journal: Applied Economics*, *9*(4), 1–29.

Fafchamps, M., & Lund, S. (2003). Risk-sharing networks in rural philippines. *Journal of Development Economics*, *71*(2), 261–287.

Fisman, R. (2001). Estimating the value of political connections. *American economic review*, *91*(4), 1095–1102.

Gelber, A., Moore, T. J., & Strand, A. (2017). The effect of disability insurance payments on beneficiaries' earnings. *American Economic Journal: Economic Policy*, *9*(3), 229–61.

Glaeser, E. L., La Porta, R., Lopez-de Silanes, F., & Shleifer, A. (2004). Do institutions cause growth? *Journal of economic Growth*, *9*(3), 271–303.

Golan, J., Sicular, T., & Umapathi, N. (2017). Unconditional cash transfers in china: Who benefits from the rural minimum living standard guarantee (dibao) program? *World Development*, *93*, 316–336.

Gollin, D., Lagakos, D., & Waugh, M. (2014). The agricultural productivity gap. *The Quarterly Journal of Economics*, *129*(2), 939–993.

Gollin, D., Parente, S., & Rogerson, R. (2002). The role of agriculture in development. *American economic review*, *92*(2), 160–164.

Grembi, V., Nannicini, T., & Troiano, U. (2016). Do fiscal rules matter? *American Economic Journal: Applied Economics*, 1–30.

Guo, Y., Zhou, Y., & Liu, Y. (2022). Targeted poverty alleviation and its practices in rural china: A case study of fuping county, hebei province. *Journal of Rural Studies*, *93*, 430–440.

Honorati, M., Gentilini, U., & Yemtsov, R. G. (2015). *The state of social safety nets 2015* (Tech. Rep.). The World Bank.

Hsu, Y.-C., & Shen, S. (2019). Testing treatment effect heterogeneity in regression discontinuity designs. *Journal of Econometrics*, *208*(2), 468–486.

Hunt, J., & Laszlo, S. (2006). Who loses from bribery and who are its beneficiaries? *Working Paper: McGill University*.

Imbens, G. W., & Lemieux, T. (2008). The regression discontinuity design: Theory and applications. *Journal of Econometrics*, *142*(2), 611–614.

Imbert, C., & Papp, J. (2020). Costs and benefits of rural-urban migration: Evidence from india. *Journal of Development Economics*, *146*, 102473.

Jia, R., & Nie, H. (2017). Decentralization, collusion, and coal mine deaths. *Review of Economics and Statistics*, *99*(1), 105–118.

Kaufmann, D., Kraay, A., & Mastruzzi, M. (2009). Governance matters viii: aggregate and individual governance indicators, 1996-2008. *World bank policy research working paper*(4978).

Keele, L. J., & Titiunik, R. (2015). Geographic boundaries as regression discontinuities. *Political Analysis*, *23*(1), 127–155.

Khalil, U., Oak, M., & Ponnusamy, S. (2021). Political favoritism by powerful politicians: Evidence from chief ministers in india. *European Journal of Political Economy*, *66*, 101949.

Kung, J. K.-s., & Zhou, T. (2021). Political elites and hometown favoritism in famine-stricken China. *Journal of Comparative Economics*, *49*(1), 22–37. Retrieved from https://doi.org/10.1016/j.jce.2020.06.001 doi: 10.1016/j.jce.2020.06.001

Lehner, A. (2019). Essays in long-run development and spatial economics.

Li, F., Ji, C., & Buchinsky, M. (2023). Corruption and firms' location choice: Evidence from a quasi-experiment of institutional reform in rural china. *unpublished manuscript*.

Li, Z., & Lawson, E. D. (2002). Generation names in china: past, present, and future. *Names*, *50*(3), 163–172.

Liu, Y., Guo, Y., & Zhou, Y. (2018). Poverty alleviation in rural china: Policy changes, future challenges and policy implications. *China Agricultural Economic Review*, *10*(2), 241–259.

Liu, Z. (2005). Institution and inequality: the hukou system in china. *Journal of comparative economics*, *33*(1), 133–157.

Lopez, H. (2004). Pro-poor growth: A review of what we know and of what we don't. *Mimeo, The World Bank, Washington*.

Mamdani, M. (1996). *Citizen and subject: contemporary africa and the legacy of late colonialism*. Princeton, N.J: Princeton University Press.

Martinez-Bravo, M., Padró i Miquel, G., Qian, N., & Yao, Y. (2022). The rise and fall of local elections in china. *American Economic Review*, *112*(9), 2921–58.

McKenzie, D., Stillman, S., & Gibson, J. (2010). How important is selection? experimental vs. non-experimental measures of the income gains from migration. *Journal of the European Economic Association*, *8*(4), 913–945.

Michalopoulos, S., & Papaioannou, E. (2014). National institutions and subnational development in africa. *The Quarterly journal of economics*, *129*(1), 151–213.

Miguel, E. (2004). Tribe or nation? nation building and public goods in kenya versus tanzania. *World politics*, *56*(3), 327–362.

Munshi, K., & Rosenzweig, M. (2016). Networks and misallocation: Insurance, migration, and the rural-urban wage gap. *American Economic Review*, *106*(1), 46–98.

Myerson, R. B. (2017). *Village communities and global development*. African Development Bank.

Nunn, N., & Puga, D. (2012). Ruggedness: The blessing of bad geography in africa. *Review of Economics and Statistics*, *94*(1), 20–36.

Olken, B. A. (2006). Corruption and the costs of redistribution: Micro evidence from indonesia. *Journal of public economics*, *90*(4-5), 853–870.

Olken, B. A. (2007). Monitoring corruption: evidence from a field experiment in indonesia. *Journal of political Economy*, *115*(2), 200–249.

Olken, B. A. (2010). Direct democracy and local public goods: Evidence from a field experiment in Indonesia. *American Political Science Review*, *104*(2), 243–267. doi: 10.1017/S0003055410000079

Olken, B. A., & Pande, R. (2012). Corruption in developing countries. *Annu. Rev. Econ.*, *4*(1), 479–509.

Qian, N. (2015). Making Progress on Foreign Aid. *Annual Review of Economics*, *7*(1), 277–308. doi: 10.1146/annurev-economics-080614-115553

Reinikka, R., & Svensson, J. (2004). Local capture: Evidence from a central government transfer program in Uganda. *The Quarterly Journal of Economics*, *119*(2), 679–705. doi: 10.1162/0033553041382120

Restuccia, D., Yang, D. T., & Zhu, X. (2008). Agriculture and aggregate productivity: A quantitative cross-country analysis. *Journal of Monetary Economics*, *55*(2), 234–250.

Rodrik, D. (2000). Institutions for high-quality growth: What they are and how to acquire them. *Studies in Comparative International Development*, *35*(3), 3–31.

Roelen, K., Kim, S. K., Barnett, I., & Chanchani, D. (2019). Pathways to stronger futures in haiti: the role of graduation programming in promoting early childhood development.

Samphantharak, K., & Malesky, E. J. (2008). Predictable corruption and firm investment: evidence from a natural experiment and survey of cambodian entrepreneurs. *Quarterly Journal of Political Science*, *3*, 227–267.

Schubert, G. (2002). Village elections in the prc: a trojan horse of democracy? *Institute for East Asian Studies/East Asian Politics, Project Discussion Paper No. 19*.

Sequeira, S., & Djankov, S. (2010). An empirical study of corruption in ports. *MPRA Paper*, *21791*.

Spolaore, E., & Wacziarg, R. (2013). Long-term barriers to economic development. In P. Aghion & S. N. Durlauf (Eds.), *Handbook of economic growth* (Vol. 2A, pp. 121–176). Elsevier, North Holland.

Stark, O., & Bloom, D. E. (1985). The new economics of labor migration. *The American Economic Review*, *75*(2), 173–178.

Svensson, J. (2003). Who must pay bribes and how much? evidence from a cross section of firms. *The Quarterly Journal of Economics*, *118*(1), 207–230.

Tebaldi, E., & Elmslie, B. (2008). Institutions, innovation and economic growth. *Journal of Economic Development*, *33*, 27–53.

Vollrath, D. (2009). How important are dual economy effects for aggregate productivity? *Journal of Development Economics*, *88*(2), 325–334.

Xu, K.-L. (2017). Regression discontinuity with categorical outcomes. *Journal of Econometrics*, *201*(1), 1–18.

Yang, D., & Martinez, C. (2006). Remittances and poverty in migrants' home areas: Evidence from the philippines. *International migration, remittances and the brain drain*(3).

Young, A. (2013). Inequality, the urban-rural gap, and migration. *The Quarterly Journal of Economics*, *128*(4), 1727–1785.

Yuan, H. (2019). Identification of poor villages and root governance- evidence from m county (*in Chinese*). *Nanjing Agriculture University Journal*, *19*(03), 18-28.

Zhang, Y., Zheng, X., & Lunyu, X. (2021). How do poverty alleviation coordinators help the impoverished in rural china?–evidence from the chinese poor population tracking dataset. *China Economic Review*, *69*, 101686.

Appendix A—The Targeted Poverty Alleviation Program

The TPA was launched during the period 2013-2015 with the goal of ending poverty by 2020.[49] The period of 2014-2015 could be seen as the preparatory stage for the TPA, in which a massive effort was launched to identify "poor households" and build a national data management system for these households. There were all together 29 million households (90 million poor individuals) identified and recorded into the system. The procedures used to identify the poor households included home visits, asset verification via administrative data, and democratic votes by peers. Local governments first designated some villages as "poor villages" (*pinkuncun*). A total of 128,000 villages were designated as "*poor villages*" in the entire country. In addition to the direct household-level funding and services that were made available for "poor households",the villages designated "poor" villages received institutional reform that is key to our study.

The exact date of the start of institutional reform was slightly different across counties in China. Nevertheless, the reform itself had the same structure as in the sample county. We use the sample county as an example in illustrating the exact timeline. Starting from the end of 2014, the sample county paired the "poor" villages with one or more high tier official (from county, prefecture, or province) as the village supervisor (VS). By the end of 2015, all "poor" villages were paired with at least one village supervisor with the publication of the assessment standards.[50] Figure A.1 illustrates a comparison between the age and education levels of village cadres and village supervisors, evidently showcasing that the latter group is significantly younger and more educated compared to the former. In addition to the VSs working full-time in the villages, the reform also paired the VSs with a support team that conducts regular visits. The full structure of the support team is depicted for clarity in Figure A.2. As we can see, the support team and village supervisors come from various county departments, and this policy created a direct link between the village and the department.

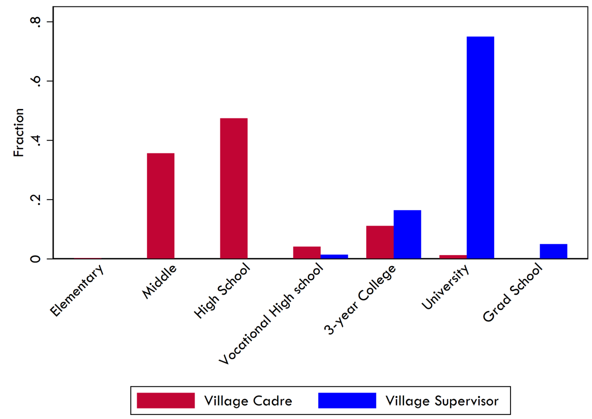
The institutional reform was further expanded in the summer of 2017 (May 6, 2017) by pairing each village with an additional town official, typically of a high rank. The town officials are more familiar with the villagers and village cadres; therefore they can better facilitate the supervisors' job[51]. This policy created a direct link, and therefore accountability, between village performance and specific town officials.

By mid-2018, the scope of the VS reform expanded to encompass the "not-poor" villages as

[49] At the end of 2013, President Xi introduced the nation to the "Targeted Poverty Alleviation" program during his visit to Henan. In November of 2015, the State Council published a document entitled: "Decision on Winning the Fight against Poverty," officially announcing with detailed policy guidance and plans the goal of ending poverty by 2020.

[50] The sample county Village Supervisor Performance Assessment Standards were published on October 13, 2015.

[51] This new working structure is called "a village responsibility team" (*zerenzu*)



(a) Education comparison

Figure A.1: **Age and Education for Village Supervisors and Cadres**

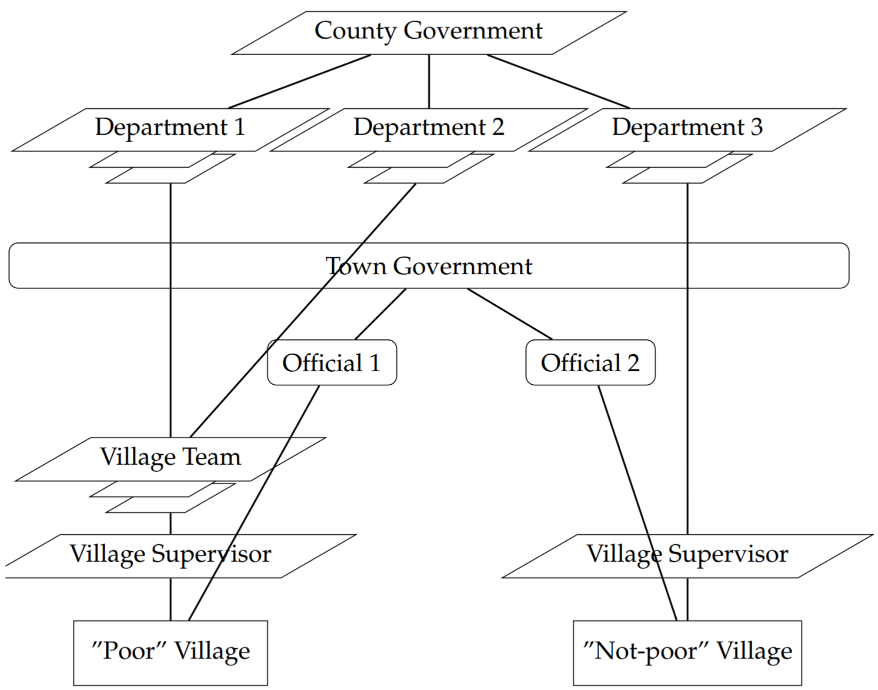
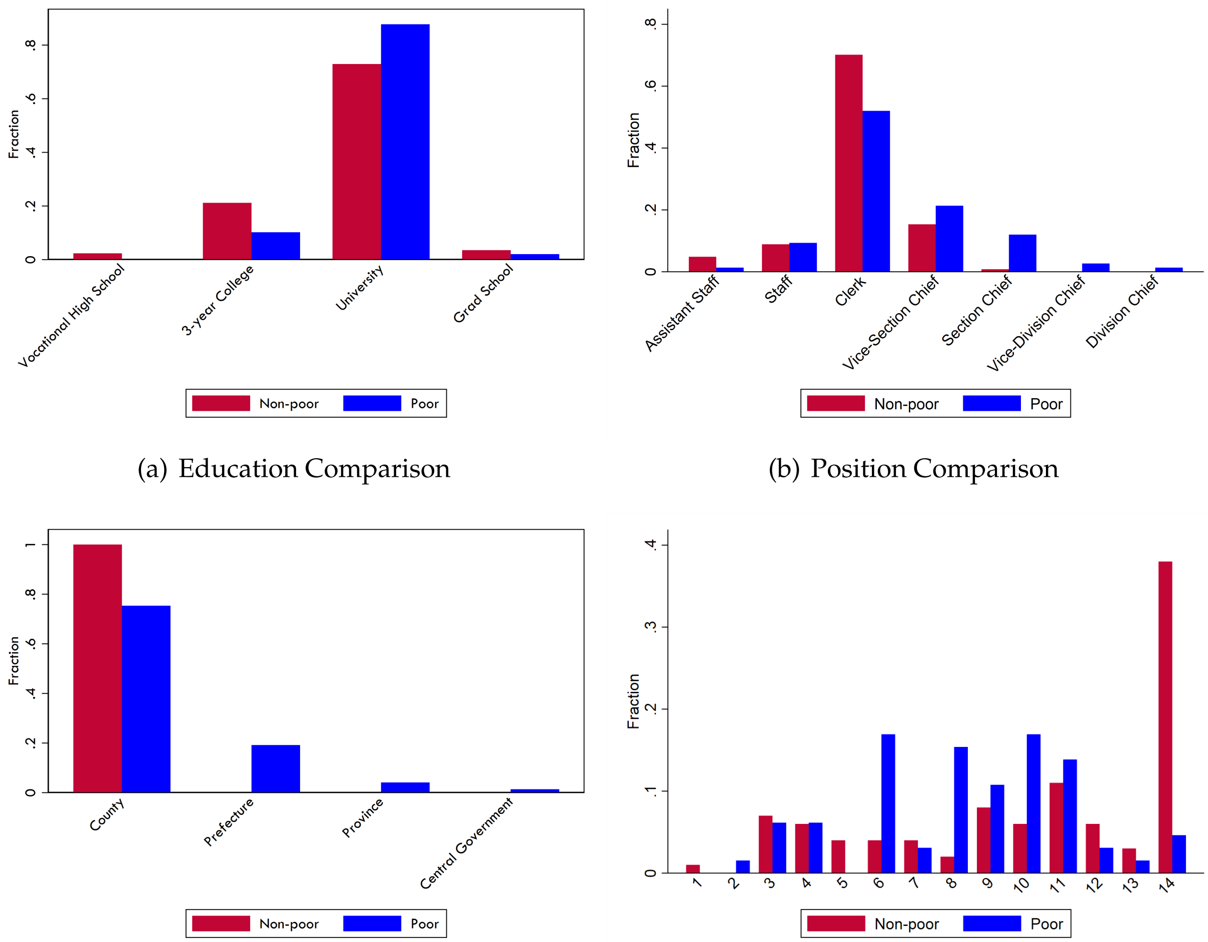


Figure A.2: **Institutional Reforms—Support Team Structure**

well, but in a more simplified way, as shown in Figure A.2.[52] In Figure A.3 we compare the education level of the VSs, their government positions, the quality of their departments, and the rankings of the paired town officials across both the "poor" and "not-poor" villages. Clearly the VSs in the "not-poor" villages are less capable compared to those in poor villages, as evidenced by their lower educational attainment, lower position rankings,

[52]On April 23, 2017, an announcement was made regarding the extension of the village supervisor policy to "not-poor" villages. By mid-2018, all VSs assigned to "not-poor" villages commenced their duties.

and, also, by the lower rankings of the assigned town officials.[53]



(a) Education Comparison

(b) Position Comparison

(c) Department Comparison

(d) Town Official Ranking Comparison

Figure A.3: **Village Supervisors for Poor and Not-Poor Villages**

The TPA is not a standalone new welfare scheme, as it is sometimes perceived to be. Rather, it was established in order to revamp the existing system. Its goals are to accurately identify, continuously track, and gather data on the poorest rural households. This is in order to be able to offer these households suitable programs tailored to their needs from the available pool.[54] In Table A.2, we compile a summary of all the welfare programs that were available for rural residents *prior to the start of the TPA*, alongside the programs introduced during the TPA exclusively for those identified as poor.

[53]For reference, Table A.1 displays the hierarchy of the government positions, ranked from 1 (the most important) to 14.

[54]This is similar to the *Ultra-Poor Graduation Program* proposed by A. Banerjee et al. (2015), which was also designed to identify the most needed first and then provide a ”big push”, by offering a basket of welfare programs tailored to their needs.

Table A.1: **Town Official Ranking**

Official name (in English)	**Official name (in Chinese)**	Ranking
Secretary of township Committee of the CPC	dangwei shuji	1
Chief Executive	xiangzhang (zhenzhang)	2
Deputy Secretary	fu shuji	3
Chairman of township People's Congress	renda zhuxi	4
Third Chief Clark	sanji zhuren keyuan	5
Secretary of Commission for Discipline Inspection	jiwei shuji	6
First Vice Chief Executive	changwu fu xiangzhang	7
Secretary of Commission for organization	zuzhi weiyuan	8
Secretary of Commission for publicity	xuanchuan weiyuan	9
Secretary of Commission for armed forces	wuzhuang buzhang	10
Vice Chief Executive	fu xiangzhang	11
Chief Manager of public studio	qunzhong gongzuozhanzhang	12
Fourth-level Chief Clark	siji zhuren keyuan	13
others	qita	14

Appendix B—Representativeness of the Sample County

The total population in the sample county was 350 thousand in 2010. According to the 2010 Chinese Population Census, the median county population size in all of China was about 380 thousand. The county's per capita GDP in 2015 was 28.8 thousand Chinese Yuan (CNY), similar to the median of all Chinese counties at 27.1 thousand CNY (as reported by the National Bureau of Statistics of China, 2016). Overall, the sample we have for the sample county is comparable to a median county in China in terms of population demographics and other social and economic characteristics. This section provides comparisons of distributions of all types of social and economic variables between the sample county and all Chinese counties with macro statistics and survey data (CHFS) for 2015. The degree of urbanization (Figure B.1a) in the sample county (.36) was very similar to that of the median county in China (.39), as were the average education levels (Figure B.1c) and the population density measure by the number of individual per m^2 (Figure B.1d).

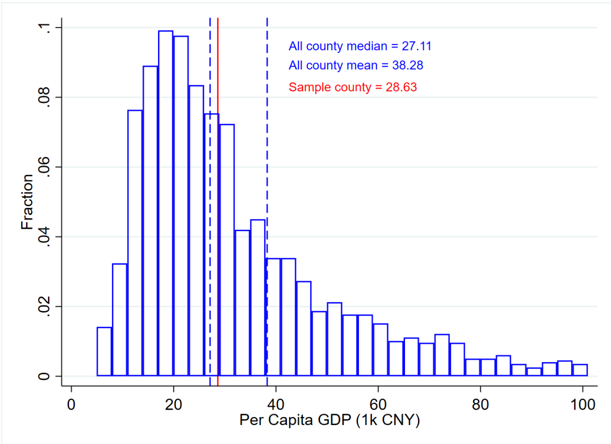
The socioeconomic variables that are related to out-migration, computed from the 2010 census, were also very similar, as is demonstrated in Figure B.2. Nevertheless, because the sample county is a poor county the out-migration is larger (22%) relative to that of a median county in China (3%). The dependency ratio (i.e., the ratio of the population in the 0–14 and 65+ age ranges relative to the whole population) is larger in the sample county than in the median county, largely because there is a large population in the 0–14 age group. Consequently, there is a smaller fraction in the productive age group of 15–64 years old (Figure B.2c) relative to the median county in China; 66% and 74%, respectively.

Table B.1 provides detailed statistics of the key household-level (Panel A) and individual-level (Panel B) variables in our data extract and the CHFS. We have significantly more

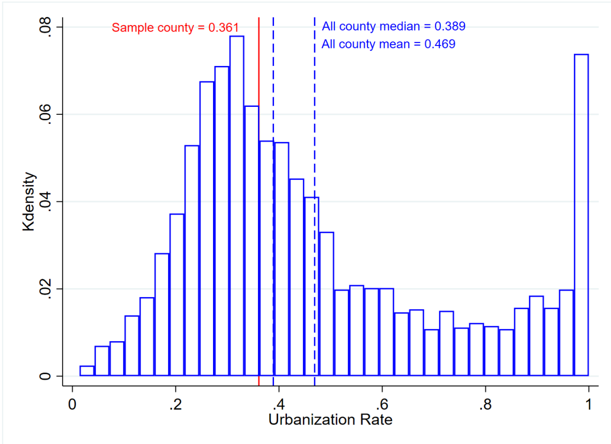
Table A.2: **Welfare Programs Available in Rural China 2017-2019**

Type	Program	Description	Beneficiary		
			obs	mean	std
Pre-TPA	Rural minimum living standard guarantee (dibao)	Starts in early 2000 and provides cash supplements to households with income below specified income thresholds	5,984	3189.63	1976.37
	Relief for the Extremely Poor (wubao)	This is a CCT for "Elders, the disabled, and orphans who are incapable of work and have no persons to provide maintenance", started in the 1950s.	1,153	5161.45	1549.30
	New Rural Social Pension	This is a nationwide rural social pension plan that started in 2009	5,732	1675.42	657.35
	Ecological compensation	This program started in 2002 and provides annual compensation for certain lands	9,547	201.15	224.15
	Family planning subsidy	This provides annual compensation for families who meet the family planning requirement	596	1681.88	2338.78
Exclusive	Asset transfer plan	In 2015, local governments provided one-shot cash transfers to poor households (4000 CNY) to invest in local businesses and collect annual interest	8,815	474.72	137.76
	Government positions	Starting in 2017, local governments created local jobs (like cleaners) only for poor families, with little skill requirement or work requirements.	2,217	5880.25	3779.28
	Solar poverty alleviation	Starting in 2014, local governments built up PV stations and used the generated revenue to provide cash transfers to selected poor families	365	464.04	241.43
	Subsidized loan plan	In 2016, local governments provided one-shot cash transfers to poor households (50,000 CNY) to invest in local businesses and collect annual interest	267	2283.21	694.94
	Transportation subsidy	In 2016, local governments allocated 1,000 CNY per trip to any impoverished individual migrating out-of-county for employment opportunities.			
Overall transfer			11,962	6576.09	5402.79

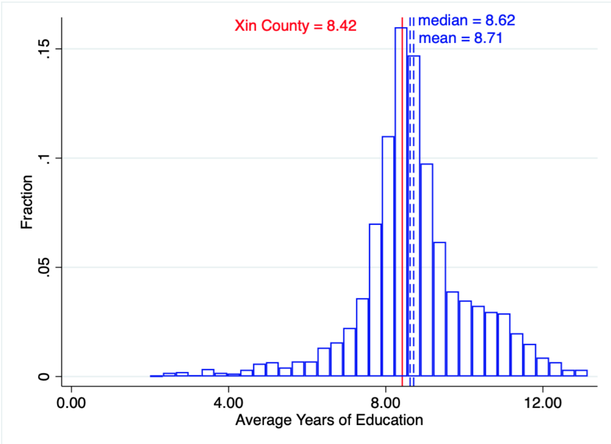
observations for the sample county than for any other county in China. The statistics for the key financial variables we have in the sample county are very similar for those in the rest of China. This is true for family income, wage, capital income, and RBLA. There are some minor differences, though, for the individual-level data. In general, the population in the sample county is younger and less educated than in the rest of China, but the differences are not very large. As for the overall health condition, the sample county is doing better than the rest of the country, but, again, the differences are not very large.



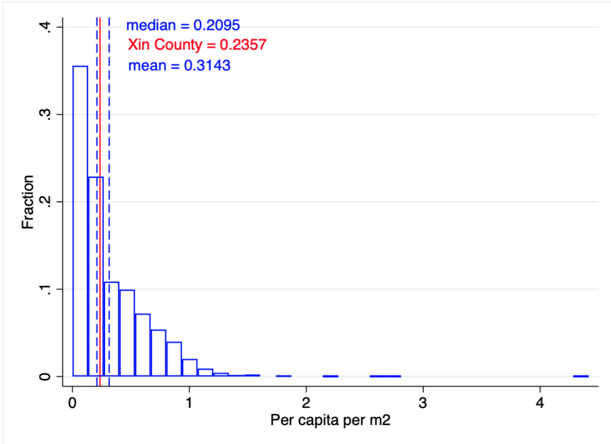
(a) Per Capita GDP



(b) Urbanization

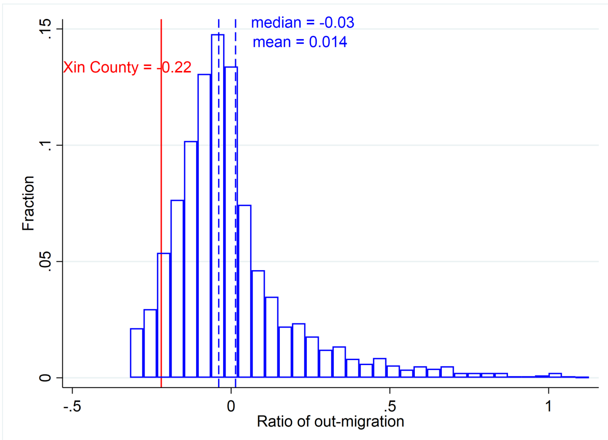


(c) Education Level

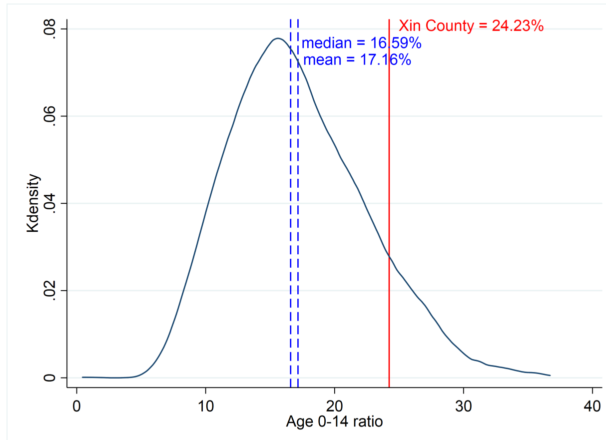


(d) Per Capita per m^2

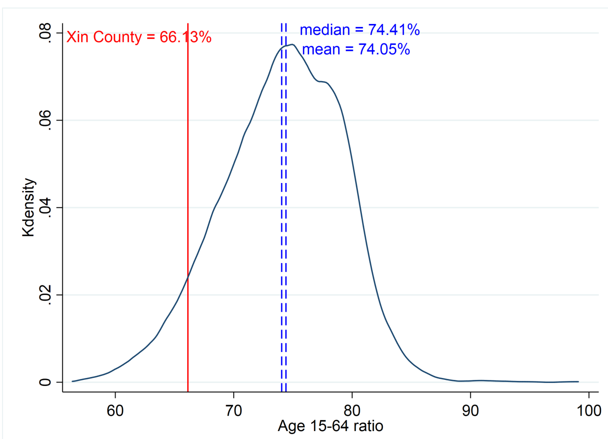
Figure B.1: **Distributions of Socioeconomic Variables**



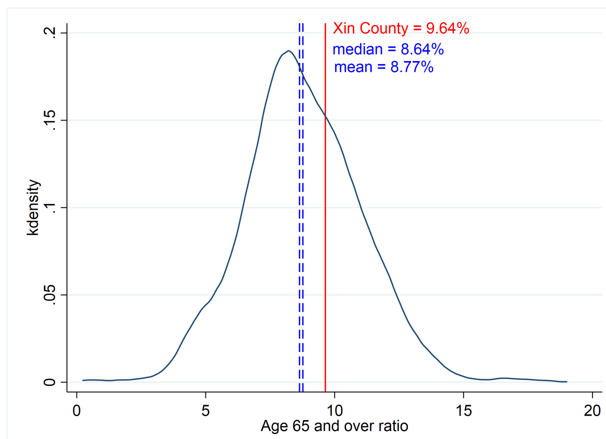
(a) Out-Migration Rate



(b) Ratio of 0–14 Years Old to Population



(c) Ratio of 15–64 Years Old to Population



(d) Ratio of 65+ Years Old to Population

Figure B.2: **Distributions of Socioeconomic Variables Related to Out-Migration**

Table B.1: **Data Comparison for Our Sample and the CHFS (2015) Survey Data**

Variable	Statistics	Sample county	CHFS (2015)			
			Overall	East	Middle	West
Panel A. Household-Level						
Family income (CNY)	mean	7,650	7,950	7,722	8,563	7,585
	median	7,740	7,114	6,775	8,400	6,365
Wage (CNY)	mean	6,429	7,820	7,730	8,073	7,685
	median	6,000	7,500	7,200	8,000	7,500
Capital income (CNY)	mean	52	78	85	67	84
RBLA (CNY)	mean	1,373	1,620	1,650	1,625	1,602
	median	1,188	1,300	1,420	1,200	1,300
Farming land (mu)	mean	2	2	2	2	3
	median	2	2	2	2	3
Family size	mean	3	4	4	4	4
	median	3	4	3	4	4
Dependence ratio	mean	0.2	0.3	0.3	0.3	0.2
	median	0	0.2	0.2	0.2	0.1
obs		11,350	1,333	332	451	550
Panel B. Individual-Level						
Age	mean	37	45	46	46	42
	median	40	45	47	48	42
Education	$\leq$ elementary	51.97	55.53	49.12	51.48	63.87
	middle school	36.58	32.21	32.22	31.47	22.61
	high school	7.55	11.07	12.82	11.95	8.99
	college +	3.90	5.09	5.85	5.11	4.53
Health condition	healthy	76.34	69.60	74.05	66.82	68.98
	unhealthy	23.06	30.40	25.95	33.18	31.02
obs		39,405	9,788	2,608	3,376	3,804

Appendix C—Identification Process of the Poor in TPA

Poor households were identified through a four-step process detailed below:

Step 1: In 2014, each village submitted a finalized roster of poor households to the respective town and county governments. Initially, households individually applied for recognition as poor households. Subsequently, the village community endorsed or rejected each application through a democratic voting process. The voting outcome was then ratified by the village cadres.

Step 2: Beginning in November 2015, following the submission of village lists, the county government assembled a collaborative force of county and town officials, village supervisors, and village working teams to conduct individualized assessments and on-site interviews of each household identified as poor. These assessment teams delved into a comprehensive evaluation covering infrastructure accessibility, the surrounding environment, living conditions, annual income, and the potential for income growth (referred to as the five-aspect or *wukan* assessment). The evaluative process entailed a comparative analysis of resources available to the assessed household against those available to adjacent households. Throughout this process, any household meeting the poverty criteria, yet not initially listed by its village, was incorporated into the roster of identified poor households. Subsequent to this primary assessment, a secondary round of village voting was organized by officials, resulting in the finalization of the second-round roster of poor households.[55]

Step 3: In December 2015, the roster from the second round was forwarded to multiple national bureaus for a detailed cross-reference analysis. The intent behind this exercise was to meticulously assess household assets, such as vehicles, real estate, and businesses, in order to precisely distinguish households that did not qualify as "poor".[56]

Step 4: Starting in 2016, the PHRMS organized a further round of random inspections targeted at the households designated as poor. In instances of misidentification of a poor household, stringent penalties were to be imposed on the corresponding county, prefecture, and provincial governments.

Upon the completion of Rounds 2 and 3, out of the initial 29 million households nominated by village cadres, 9.29 million were discerned as not poor and subsequently removed from the system. Concurrently, 8.07 million households were newly recognized as poor and

[55] The steps are outlined as: Entry, observation, computation, comparison, voting, and finalization, or (in Chinese): *yijin, erkan, ansuan, sibi, wuyi, liuding*.

[56] The primary disqualifications include: ownership of real estate outside the village valued at over 100,000 CNY; possession of private cars or trucks valued above 30,000 CNY; having family members serving as officials or village cadres; having family members who operate a business; or having a family member earning a substantial regular income, specifically a pension exceeding 1,000 CNY monthly.

incorporated into the system.[57] In the sample county, 11,810 households were initially, selected in 2014. In the subsequent review conducted in November of 2015, it was determined that 18.2% of these selections were erroneous. A further 3.7% were identified as incorrectly selected in December of 2015.

There was a tendency among the village cadres to designate households from their family clans as poor during Stage 1, thereby granting them access to additional resources. Therefore, it is important to examine the effectiveness of both Stage 2 and Stage 3 in identifying the inaccuracy of the initial endorsements, and thus help understand the level of selection bias present in the final roster. With the availability of the family roster from each round, we can examine whether more connected families were included in Stage 1, and thus have a higher likelihood of being detected in the later stages. To do that we utilize the following Probit model:

$$Pr(\text{detected as not poor}_{ij}) = \alpha_0 + \alpha_1 Connection_i + X_i'\beta + \varepsilon_i, \tag{7}$$

for $j = 0, 2, 3$, with j= 0 denoting that a household was not identified as "non-poor" across the three rounds, implying they are indeed genuinely poor.

Here, the key explanatory variable in this regression is *Connection*, defined in the main paper. To ascertain the *cadre_surname*, we utilize the cadre in position at the end of 2014, as this marks the time when the cadres assembled the initial round roster. In our model, we account for the income-based eligibility for identification as poor within the variable X. This includes factors such as household composition (the number of children under 5, number of children between 6 to 15, number of elderly individuals over 65, number of healthy adult males and females, and number of sick family members), as well as household living conditions, which are determined by a within-village income ranking based on reported income.

Table C.1 presents the regression results. Indeed, we see that families belonging to the cadres' clan are about 10% more likely to be identified as not-poor in Stage 2. Interestingly, this impact diminishes and becomes insignificant in Stage 3, indicating a comparability of income status between the connected and non-connected families post Stage 2. These findings suggest that there was a substantial degree of favoritism within the village during Stage 1. More importantly, the indicate that the the external evaluators in Stage 2 were effective, and very necessary, in discerning and rectifying such favoritism. This, in turn, suggest that sample selection is not an issue in our sample.

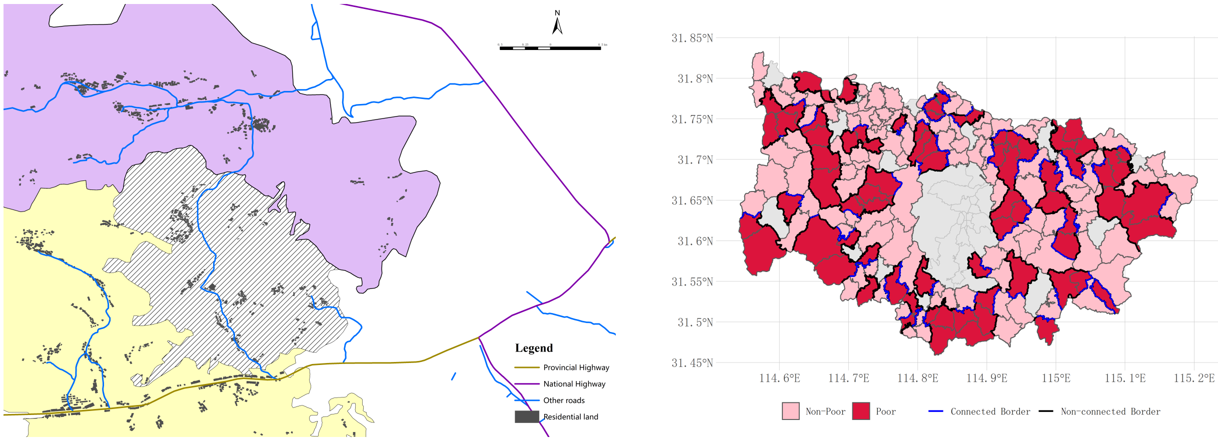
[57] Source: https://baijiahao.baidu.com/s?id=1691278301138105841&wfr=spider&for=pc.

Table C.1: **Probit Results for Identification of Poor Households**

Variable	*Round* = 2			*Round* = 3		
Connection	0.108***	0.110***	0.0950*	0.0764	0.0747	0.0642
	(0.0391)	(0.0392)	(0.0505)	(0.0597)	(0.0602)	(0.0729)
Demographics	No	Yes	Yes	No	Yes	Yes
Family income	No	No	Yes	No	No	Yes
Constant	-1.265***	-1.182***	-0.858***	-2.249***	-2.561***	-2.337***
	(0.0236)	(0.0756)	(0.103)	(0.0361)	(0.122)	(0.158)
Observations	11,820	11,820	8,098	11,820	11,820	8,098

Notes: This table presents the impact of family elite connections on the probability of detection during Stages 2 and 3, or equivalently, the chance of being erroneously included by cadres during Stage 1. For each Stage, we report the results without any controls first and then with controls, including per capita self-earned income and family decomposition.

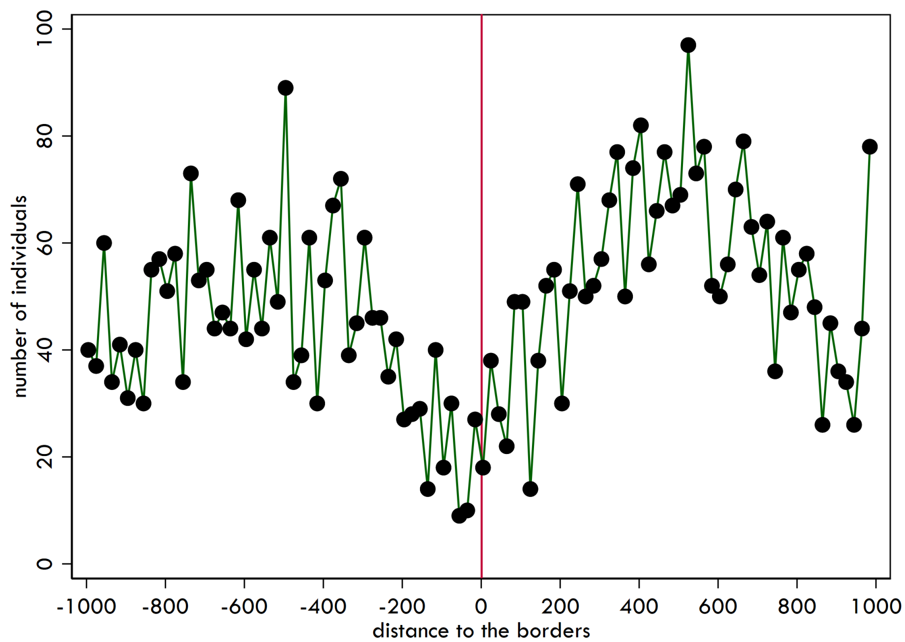
Appendix D—Village Characteristics



(a) Village border example (b) Village border type distribution

Notes: The purple village and the dashed village are connected villages, while the purple village and the white village are disconnected villages.

Figure D.1: **Types of Village Borders and Distributions in the Sample County**



Notes: Negative numbers on the left-hand side indicate distance from the border on the side of the not-poor villages, while positive numbers on the right-hand side indicate distance from the border on the side of the poor villages.

Figure D.2: **Distribution of the Number of Individuals Around the Borders**

Table D.1: **Descriptive Statistics of Village-Level Data**

Variable	Sources	Observations		Mean	Std. Dev.
		Total	Period		
Electricity usage					
Electricity (kwh)	State Grid	1,190	2013-2019	95,750	288,216
Number of accounts	State Grid	1,190	2013-2019	5.67	5.53
Demographics in basic year (2013)					
Population	PHRMS	173	2010	1,419	552
Migration % (male)	PHRMS	173	2010	0.41	0.11
Migration % (female)	PHRMS	173	2010	0.31	0.11
Road density	Road GIS	173	2013	0.43	0.40
Safe water HH %	PHRMS	172	2013	0.39	0.29
Location and endowments					
Distance to town (km)	GIS	173	2018	13.77	9.34
Distance to county (km)	GIS	173	2018	38.20	16.15
Per capita forest (km^2)	PHRMS	173	2013	787	10,273
Per capita land (mu)	PHRMS	173	2013	0.71	0.44
Per capita grass (mu)	PHRMS	173	2013	0.01	0.03
Per capita water (mu)	PHRMS	173	2013	0.09	0.17
I(deep mountain)	PHRMS	173	2013	0.38	0.49
Cadre age	Personnel	165	2019	49.50	8.49
Cadre education	Personnel	165	2019	3.40	1.04
Personal characteristics of village supervisors					
Rank	PHRMS	165	2020	9.52	3.67
Education	PHRMS	134	2019	5.83	0.48
Department level	PHRMS	195	2019	1.12	0.41

Notes: (1) Mu is a Chinese unit, with one mu equal to 666.67 km^2. (2) The town position name and the corresponding ranking table is provided in Appendix A Table A.1. (3) The education here is a categorical variable with 1 = elementary and 7 = postgrad and so on

Table D.2: **Descriptive Statistics of Villages and Balance Tests**

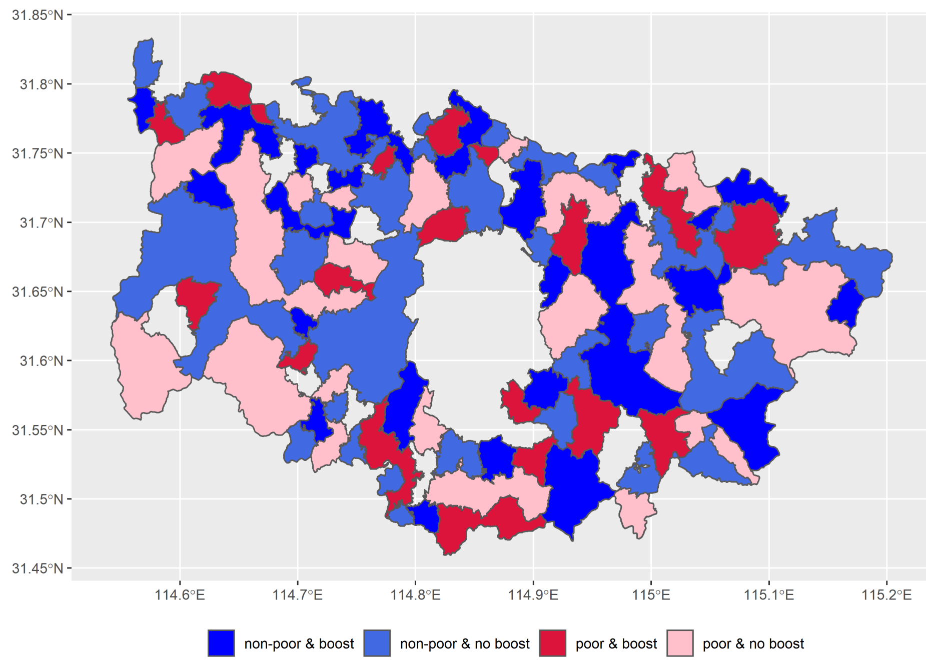
	Statistics			F-Tests	
	Obs.	Mean	Std.	Poor vs. not-poor	Four categories
Natural Resources					
Per capita cultivated land	172	0.460	0.339	2.28 (0.1327)	1.74 (0.1598)
Per capita paddy fields	149	0.108	0.181	2.74 (0.1002)	1.09 (0.3536)
Per capita forests	173	786.526	10273.8	0.73 (0.3943)	0.67 (0.5694)
Location					
Time to town center	176	11.355	6.295	0.01 (0.9120)	1.89 (0.1328)
Time to county center	176	38.357	16.721	0.03 (0.8572)	0.51 (0.6739)
Infrastructure Level					
Lengths of roads (in km)	176	3.464	2.950	0.38 (0.5384)	0.03 (0.9942)
Density of roads	176	0.565	0.487	0.57 (0.4502)	2.17* (0.0930)
Characteristics of village cadres					
Age	314	52.758	7.962	0.34 (0.5612)	0.44 (0.7265)
Education Level	314	2.939	0.985	4.98** (0.0263)	1.72 (0.1631)
Economic Activities					
# of SLCs	170	1.424	1.564	0.60 (0.4405)	0.14 (0.9338)
# of SLCs & private farms	170	1.776	1.790	0.35 (0.5546)	0.24 (0.8714)
Population Structure					
$Pop_{2013} - Pop_{2010}$	173	118.612	128.82	3.34* (0.0695)	1.13 (0.3391)
Migration rate in 2010	173	0.363	.109	0.28 (0.5993)	0.14 (0.9385)

Notes: In this table, we display statistics of various village-level characteristics from an array of aspects. In Column 5, we provide the F-test statistics to evaluate the equality between poor and non-poor villages concerning each village-level characteristic. Further, we segregate the villages into four distinct categories and examine the presence of significant disparities among them. The four categories comprise:(a) villages never labeled as ”poor”; (b) villages labeled as ”poor” pre-2014 but designated as ”not-poor” in 2014; (c) villages labeled as ”poor” in 2014, making them eligible for the institutional reform before they switched to ”not-poor” prior to 2015; and (d) villages labeled as ”poor” in 2014 but relabeled as ”not-poor” between 2016 and 2018, also making them eligible for the reform.

Table D.3: **Comparison of Family Characteristics around Village Borders**

Variable	Estimate	St. Err.	Wald test
Panel A— Family Level Characteristics			
Self-earned income (CNY)	-0.00002	0.00004	0.16
Distance to road	0.00005	0 .0002	0.07
Social transfer (CNY)	0.00003	0.00004	0.47
Family size	0.010	0.034	0.09
Num. of adults	0.007	0.049	0.02
Num. of children < 5	0.005	0.0406	0.01
Num. of students	-0.0118	0.0469	0.06
Num. of sick individuals	-0.037	0.0597	0.39
Panel B— Individual Level Characteristics			
Age	0.0008	0 .0008	1.00
Whether college degree	-0.0029	0.055	0.00
Month	0.006	0.006	0.97
Month in town	-0.013	0.015	0.73
Month out-of-town	0.009	0.006	1.97
Month out-of-county	0.002	0.007	0.06

Notes: This table presents the continuity test results. We conduct a series of probit regressions to assess whether a household is belonging to poor villages (i.e., subjected to institutional reform), using various household and individual characteristics as covariant. We report the estimation coefficients, robust standard errors clustered at the town level and the Wald test result for each variable.



Notes: In this figure, villages are further categorized into "boosted" and "not boosted" villages based on the increase in electricity usage. Specifically, villages with non-residential electricity usage below the median are classified as "not boosted," while those with usage above the median are classified as "boosted."

Figure D.3: **Treatment Type and Local Economy Condition**

Appendix E—Additional Results for Section 4

In this section, we provide further evidence to support the effectiveness of the VS reform on welfare allocation. Table E.1 displays the Probit and Tobit regression results for Equation 1 by year. The results indicate that the reform treatment effects were significant in 2017, a year after the initiation of the treatment. The treatment persisted over the subsequent two years, being highly significant in 2018 and 2019. The partial treatments that was administered for the not-poor villages from 2018 onward did not manifest itself into any noticeable effects, at least in the first two years after the implementation.

Table E.1: **Probit and Tobit Regression Results for Welfare Program Allocation by Year**

	Probit for welfare possibility			Tobit for welfare level		
	2017	2018	2019	2017	2018	2019
Panel A — Basic Living Allowance:						
connection*T	-0.325***	-0.246***	-0.344***	-846.0***	-594.7***	-949.6***
	(0.0592)	(0.0565)	(0.0525)	(165.6)	(184.0)	(163.1)
connection	0.186***	0.135***	0.211***	540.6***	317.9**	553.5***
	(0.0496)	(0.0480)	(0.0447)	(137.9)	(153.5)	(137.1)
Obs.	10,096	10,102	10,113	10,096	10,102	10,113
Panel B —Government Position:						
connection*T	-0.446***	-0.446***	-0.482***	-309.5***	-345.8***	-463.7***
	(0.0485)	(0.0485)	(0.0481)	(37.20)	(40.86)	(45.95)
connection	0.251***	0.251***	0.267***	167.5***	187.6***	246.4***
	(0.0429)	(0.0429)	(0.0426)	(31.90)	(34.55)	(39.38)
Obs.	10,096	10,102	10,113	10,096	10,102	10,113

Notes: This table presents the impact of family elite connectedness on welfare access and allocation by year. In Panel A, we report the results for Basic Living Allowance (BLA) program and in Panel B, for Government Job program. For each program, we report the probit results of program access in the first three columns and program amount allocated in the last three columns. For each year, we report the results with all controls controls, including per capita self-earned income, family size and number of dependent members, distance to the nearest paved road and to the village center.

Table E.2 additionally check the robustness of our results by utilizing different definition for connectedness. In Panel A, we use the number of family members who share the same surname as village cadres as an indicator of connectedness, and in Panel B, the dummy variable of whether any family member shares not only the same surname but also the same generational name with the cadres.

Table E.2: **Favoritism and Reform Effect on Favoritism Reduction – Different Definitions of Connectedness**

	$Pr(\text{program}_{it}^k = 1)$			Program^k		
Panel A — # of family members sharing surnames:						
Connection×T	-0.202***	-0.183***	-0.192***	-214.648***	-147.302***	-144.309***
	(0.009)	(0.009)	(0.010)	(7.709)	(6.952)	(6.973)
Connection	0.152***	0.148***	0.157***	90.295***	110.718***	110.576***
	(0.008)	(0.009)	(0.009)	(6.715)	(6.082)	(6.098)
Income	No	Yes	Yes	No	Yes	Yes
Demographics	No	Yes	Yes	No	Yes	Yes
Distance	No	No	Yes	No	No	Yes
Obs	35,712	35,647	30,311	35,712	35,647	30,311
Panel B — Surname and generational name:						
Connection×T	-0.515***	-0.467***	-0.446***	-533.786***	-359.907***	-340.874***
	(0.046)	(0.047)	(0.050)	(44.441)	(40.535)	(40.843)
Connection	0.389***	0.365***	0.332***	331.096***	273.659***	254.240***
	(0.039)	(0.040)	(0.042)	(36.104)	(33.431)	(33.891)
Income	No	Yes	Yes	No	Yes	Yes
Demographics	No	Yes	Yes	No	Yes	Yes
Distance	No	No	Yes	No	No	Yes
Obs	35,712	35,647	30,311	35,712	35,647	30,311

Notes: This table presents the impact of family elite connectedness on welfare access and allocation. In Panel A, we use the number of family members who share the same surname as village cadres as an indicator of connectedness, and in Panel B, the dummy variable of whether any family member shares not only the same surname but also the same generational name with the cadres. For each regression and *Connection* measurement, we report the results without any controls first and then with controls, including per capita self-earned income, family size and number of dependent members, distance to the nearest paved road and to the village center.

Table E.3: **Corruption Shifting**

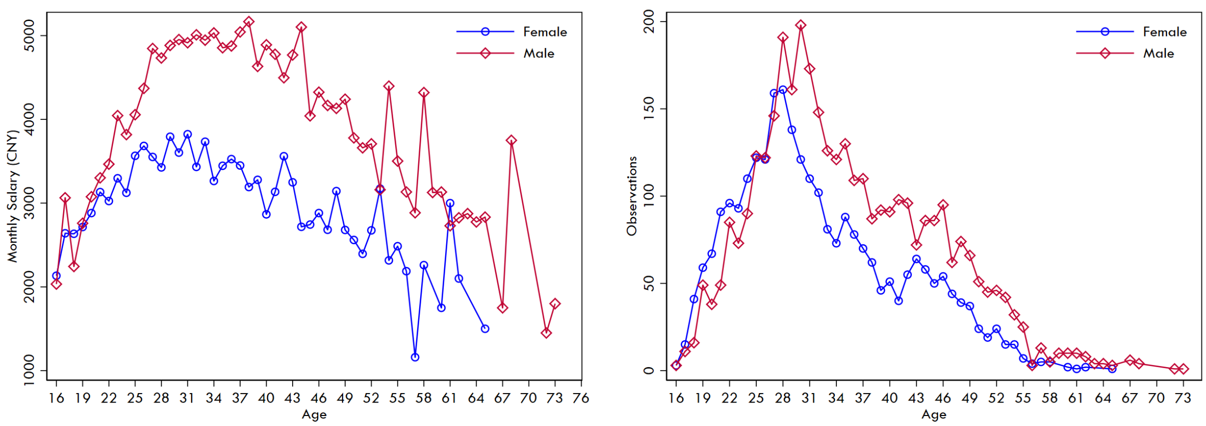
	Basic Living Allowance		Government Jobs	
	$Pr(\text{program}_{it}^{k}=1)$	Program^{k}	$Pr(\text{program}_{it}^{k}=1)$	Program^{k}
$Connection^{VS}$	0.0492	-9.687	-0.109	-373.1**
	(0.0368)	(32.45)	(0.0925)	(164.8)
Constant	0.282***	1,368***	-0.756***	-1,078***
	(0.0306)	(37.68)	(0.0465)	(100.0)
Income	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes
Distance	Yes	Yes	Yes	Yes
Observations	40,722	40,722	10,181	30,547

Notes: The primary explanatory variable in this table is a dummy variable indicating whether any family member shares the same surname with the assigned VS. In Columns 1–2, we examine the allocation of RBLA, while in Columns 3–4, we explore government job allocations. For each program, we present results both for the likelihood of accessing the program $Pr(program^{k}=1)$ and the annual amount received from the program $Program^{k}$. We control for various factors including per capita self-earned income, family size and number of dependent members, as well as the distance to the nearest paved road and to the village center.

Appendix F—Additional Results for Sections 5-6

Gender and Age Wage Gap

To illustrate the existence of gender and age wage gaps, we use data from the annual nationally representative survey of migrants conducted by the PRC's National Health Commission, namely the China Migrant Dynamics Survey (CMDS). We calculate the average monthly wages of migrants by age and gender. The average monthly wages by age and gender are shown in Figure F.1a. According to the 2017 round survey, among the 7,181 observations of migrants with rural Hukou status and migrating for work, the salary of young and middle-aged males is significantly higher than that of their female counterparts (4,547 CNY for males vs. 3,308 CNY for females). The salaries of old males and females are even lower (3,402 CNY for older males and 2,409 CNY for older females). Nevertheless, as Figure F.1b shows, because the number of observations for the older males and females is relatively small, the estimate is less accurate.



(a) Salaries of Rural Migrants, by Age and Gender (b) Number of Observations, by Age and Gender

Figure F.1: **Salaries of Rural Migrants, by Age and Gender (CMDS (2017))**

Robustness Check with 2015 Stayers

We conduct several analysis for robustness check. We first repeat the previous exercise for different samples and different migration definitions and report the results in Table F.1. In Panel A, we concentrate on those who did not have a paid job in the base year (i.e. people used to stay in the agricultural sector). Therefore the RD coefficient captures the transition from the agriculture sector to non-agriculture sectors out of town. The results are very similar for the young age groups. For the older males, the migration effect also becomes positively significant, at 0.165. In Panel B, we change the "urban area" to "out-of-county" instead of "out-of-town", and still find strong positive and significant effects for males of all age groups and young females. Together, these results suggest that people in the treated villages indeed migrate more and leave the agriculture sector more. In addition, when individuals migrate, they almost exclusively migrate to out-of-county.

In Panel B, we change the "urban area" to "out-of-county" instead of "out-of-town," and still find strong positive and significant effects for males of all age groups and young females. Together, these results suggest that when individuals migrate, they almost exclusively migrate to out-of-county.

Table F.1: **Regression Discontinuity for Location Switch in 2018 – Using Those Unemployed in 2015**

	Male		Female	
	16-50 year-old	51-75 year-old	16-50 year-old	51-75 year-old
Panel A —switch to work out of town:				
Conventional	0.275**	0.135**	0.178*	-0.004
	(0.131)	(0.057)	(0.095)	(0.040)
Bias-corrected	0.299*	0.165**	0.198*	-0.007
Robust Std	(0.161)	(0.069)	(0.114)	(0.047)
$\hat{h}^R$	1141.85	868.27	870.98	847.73
$\hat{h}^L$	899.76	796.77	758.34	888.47
$\hat{bw}^R$	1981.70	1612.32	1522.46	1467.04
$\hat{bw}^L$	1759.63	1547.95	1455.70	1521.71
Panel B —switch to work out of county:				
Conventional	0.270**	0.109**	0.168**	0.035
	(0.130)	(0.052)	(0.082)	(0.032)
Bias-corrected	0.287**	0.126**	0.182**	0.040
Robust Std	(0.160)	(0.063)	(0.095)	(0.038)
$\hat{h}^R$	1014.02	939.61	870.41	713.71
$\hat{h}^L$	988.08	744.98	701.58	713.71
$\hat{bw}^R$	1738.02	1647.70	1647.00	1362.36
$\hat{bw}^L$	1900.20	1488.46	1469.49	1372.81
Sample Size	647	1,021	1,125	1,172

Robustness check for different income cutoffs

We conduct a sensitivity analysis to test the robustness of our choice of income cutoff at 10%. We repeat the baseline RD regression for young males for different cutoffs ranging from 2% to 20%, and then plot the RD results for all young stayers in 2015 with respect to these different cutoffs in Figure F.2. The coefficients remain positive and significant for all cutoffs, and the size of the effect stabilizes after 5%. Therefore the sensitivity analysis indicates that our main findings are robust for the cutoff.

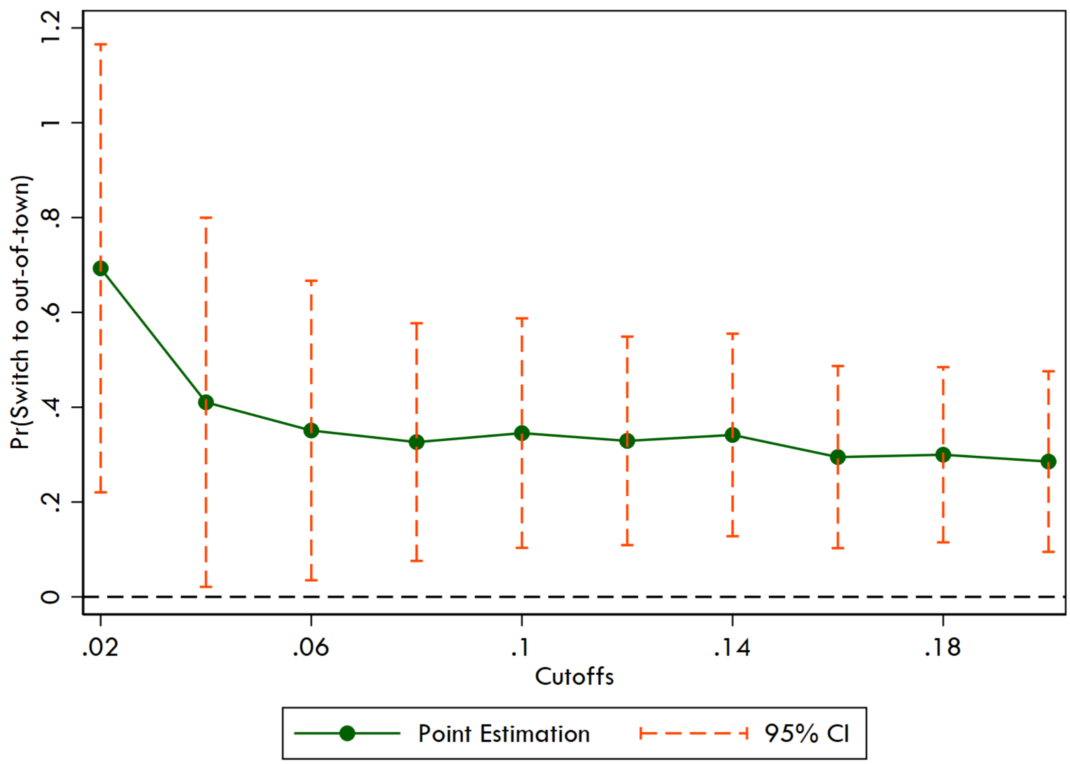


Figure F.2: **Treatment Effects for Different Cutoffs**

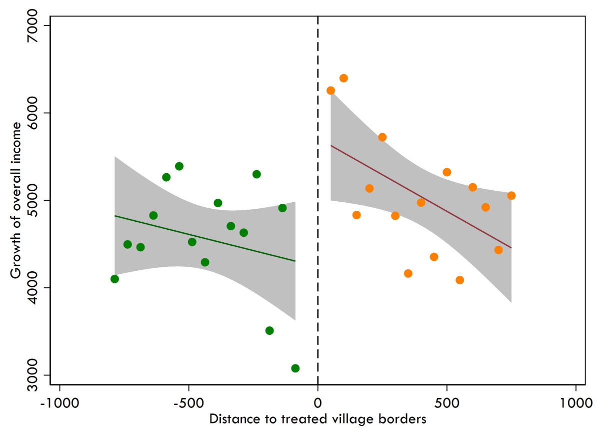
Impact on the Number of Months Working

In this section, we compare the number of months working among the new migrants and all migrants, and find no significant differences across the village borders. This suggests that the effect is only on the dichotomy migration choice and not change labor supply behavior after migration.

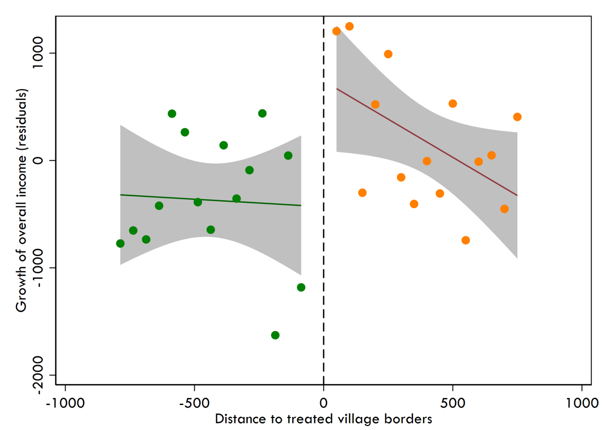
Table F.2: **Number of Months Worked for Migrants**

	Young Male		Young Female	
	New Migrants	All Migrants	New Migrants	All Migrants
Conventional	-0.209	-0.110	-0.035	0.492
	(0.893)	(0.400)	(1.102)	(0.895)
Bias-corrected	-0.122	-0.163	-0.057	0.668
Robust Std	(1.120)	(0.484)	(1.332)	(1.099)
$\hat{h}^R$	812.96	934.58	950.87	948.27
$\hat{h}^L$	784.38	844.32	1013.51	909.52
$\hat{bw}^R$	1386.73	1764.94	1632.76	1633.24
$\hat{bw}^L$	1364.96	1543.97	1810.24	1765.59
Sample Size	338	1,461	357	636

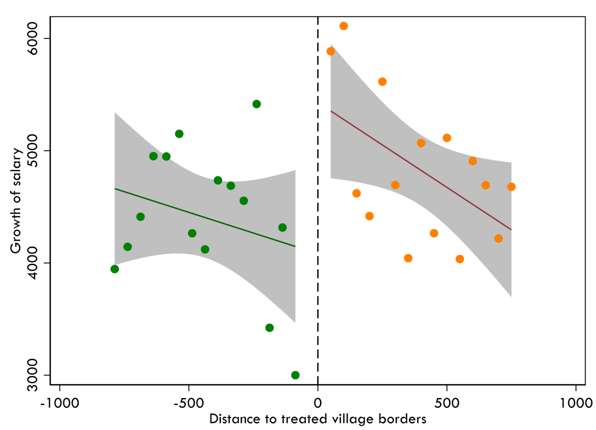
Reduced form for Income



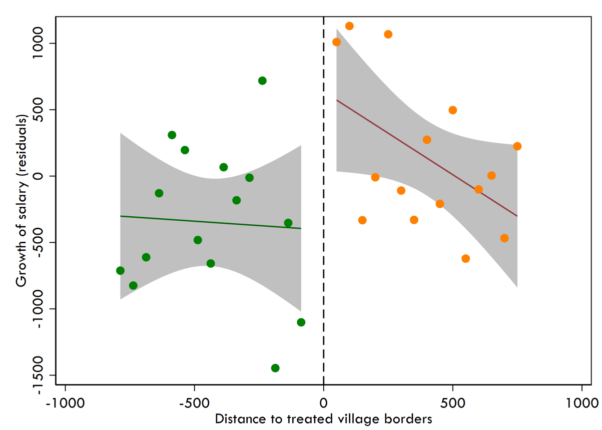
(a) Growth of Income



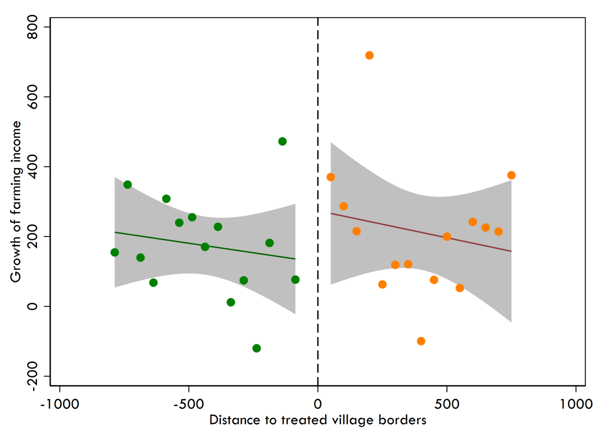
(b) Growth of Income (residualized)



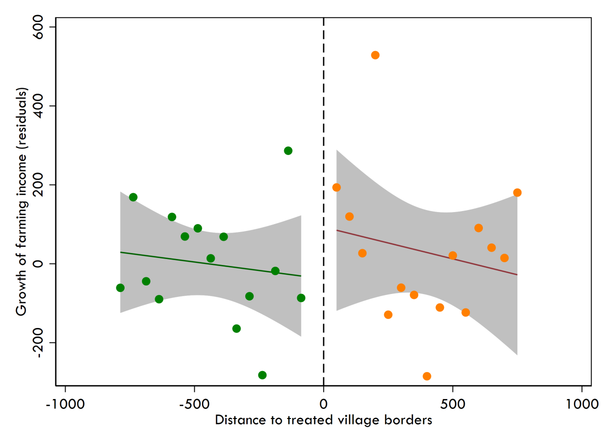
(c) Growth of Salaries



(d) Growth of Salaries (residualized)



(e) Growth of Farming Income



(f) Growth of Farming Income (residualized)

Figure F.3: **Reduced-Form Effect of Institutional Reform on Sources of Income**

.1 Robustness Check for the Time Trend Result of the Lowest 10% Households

In Figure F.4 we present the results from the spatial RD regression for each year separately. Since each regression is run independently, the chosen optimal bandwidths are, in general, different across the various RD regressions. The optimal bandwidths are shown in Table F.3. We can see that there are significant differences in the chosen bandwidths across the regression. There might be a concern about the impact of the bandwidth used on the results. To check this we repeat the estimations that are shown in Figure F.4 using the same bandwidth instead, namely the optimal bandwidth that was estimated for 2014, and the results stay the same.

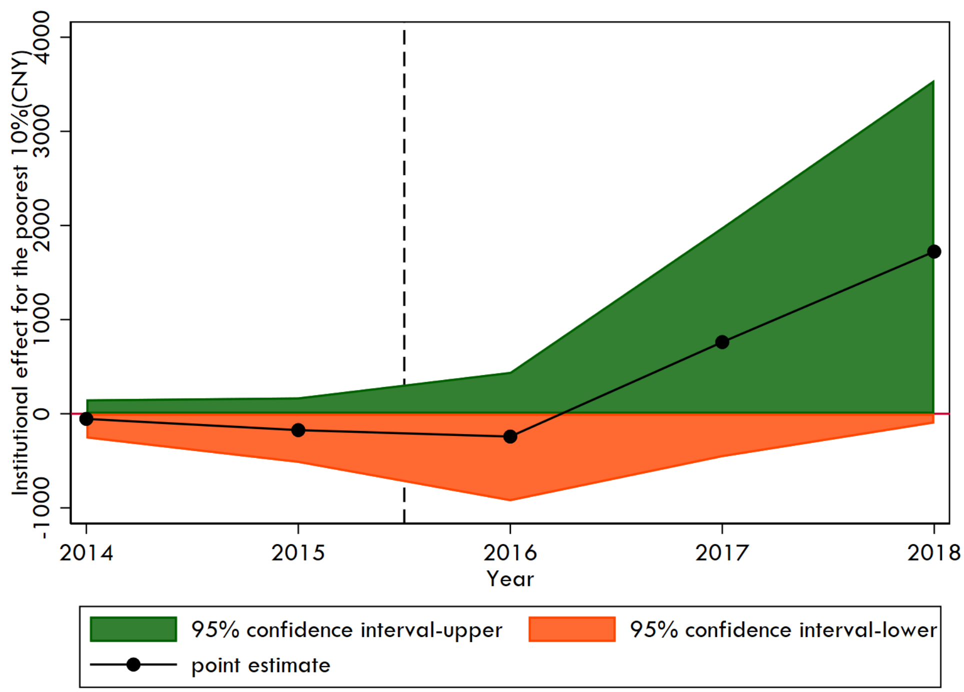


Figure F.4: **Trends in the Treatment Effects**

Table F.3: **Optimal Bandwidth Selected in Figure**

Year	2014	2015	2016	2017	2018
Panel A: Optimal bandwidths for the lowest 10% group					
h_{opt}	828.659	738.118	867.806	856.249	752.053
b_{opt}	1,477.308	1,520.610	1,660.647	1,544.151	1,437.154
Nobs	3,033	3,029	3,028	3,015	3,018
Panel B: Optimal bandwidths for the $\geq \frac{2}{3}$ dependence ratio group					
h_{opt}(m)	760.401	787.675	751.942	766.672	660.302
b_{opt}	1,360.877	1,389.260	1,463.210	1,298.105	1,321.802
Nobs	1,754	1,684	1,683	1,675	1,678

[1] This table reports the optimal bandwidths of estimation and bias correction for Tables 5 and Figure F.4.
[2] For the base year 2014 and 2015, the control variables include direct transfer level (per capita total direct transfer in 2015), families structure measurement (total number of family members and composition of the dependent members, including the number of children under 5 years old, children between 5-15 (students), senior people over 65) and household access to infrastructure (distance to the paved road in 2015). For year 2016 onward, the control variables include the above ones and the base-year income level (per capita self-earned income in 2015) and road access in 2018.

Mechanism analysis

Table F.4: **Heterogeneous Migration Effect by Degrees of Favoritism Reduction - Young females**

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Group by effectiveness on favouritism reduction					
	Top 50% effective		All effective		Not effective	
Conventional	0.199***	0.154**	0.085**	0.056	0.059	0.076**
	(0.072)	(0.066)	(0.042)	(0.040)	(0.038)	(0.038)
Bias-corrected	0.214***	0.167**	0.090**	0.058	0.071*	0.088**
	(0.072)	(0.066)	(0.042)	(0.040)	(0.038)	(0.038)
Robust	0.214**	0.167**	0.090*	0.058	0.071	0.088**
	(0.084)	(0.078)	(0.048)	(0.046)	(0.044)	(0.044)
Controls	No	Yes	No	Yes	No	Yes
Observations	1,047	1,047	2,461	2,461	1,565	1,565

Notes: This table presents the RD regression results for *Mig* among young females for the post-treatment period of 2017-2018, stratified by different levels of favoritism reduction. Columns 1–2 focus on the subsample for which VS treatments are among the upper 50th percentile in effectiveness, as measured by a border-specific marginal effect $\alpha_{\tilde{mn}}$ that exceeds the median across all poor-nonpoor village borders. Columns 3–4 present results for all young males around borders where $\alpha_{\tilde{mn}} < 0$ significantly, indicating the VS treatment is effective. Lastly, Columns 5–6 present results for the subsample with $\alpha_{\tilde{mn}}$ not significantly different from 0, i.e. the VS treatment not effective at all. For each subgroup, we provide results both with and without family-level and individual-level controls.

Table F.5: **Regression Discontinuity for Location Switch in 2018**

	2015 Stayers				2015 Farmers	
				$abs < 0.2$	$abs < 0.2$ & 5% insig	
	5% insig	10% insig	$abs < 0.2$	& 5% insig	Out of Town	Out of County
Conventional	0.195*	0.210*	0.185	0.216**	0.194*	0.204**
	(0.109)	(0.113)	(0.113)	(0.109)	(0.111)	(0.102)
Bias-corrected	0.220*	0.237*	0.226*	0.241*	0.226*	0.229*
Robust Std	(0.130)	(0.134)	(0.135)	(0.129)	(0.134)	(0.119)
Sample Size	759	712	986	726	661	661
$\hat{h}^R$	645.421	613.912	510.129	655.121	699.768	583.592
$\hat{h}^L$	607.861	554.235	505.147	619.688	637.788	542.577
$\hat{bw}^R$	1158.408	1035.148	976.162	1181.85	1181.348	1088.181
$\hat{bw}^L$	1179.204	1059.981	870.067	1202.159	1229.51	1126.016

Notes: This table provides SRRD results for the sub-sample of villages that have comparable changes in intra-village road density. In order to do this, we conduct a pairwise RD regression for each border on "road access" in 2018 (RAC_i^{2018}). We use all households i within a 1,000-meter range on each side of the border and control for the distance to the road in 2015 and family characteristics. Here RAC_i^{2018} is a dummy variable equals 1 if the distance to the road is less than 200 meters. In Column 1, the analysis is confined to village pairs where the RD results are statistically insignificant at the 5% level. This threshold is extended to 10% insignificance in Column 2. Column 3 alters to an alternative criterion, including only pairs with an absolute RD coefficient magnitude less than 0.2. Lastly, Column 4 integrate the criteria from Columns 1–3, rendering a more stringent inclusion framework for the analysis. In Column 5–6, we focus on the subgroup who did not have a paid job in 2015. The control variables are the same with Table 5.

Table F.6: **Regression Discontinuity for Location Switch in 2018 – Economy boosting**

	Location Choices of Young			Family Total Salary
	2015 Stayers		2015 farmers	
	To out-of-town	To out-of-county	To out-of-town	
Conventional	0.241***	0.209**	0.200**	2,329.326**
	(0.092)	(0.104)	(0.090)	(1,133.314)
Bias-corrected	0.261**	0.222*	0.208**	2,553.594*
Robust Std	(0.111)	(0.123)	(0.105)	(1,339.536)
Sample Size	1,403	1,252	1,252	2,123
$\hat{h}^R$	999.767	1001.298	889.707	855.868
$\hat{h}^L$	804.044	717.705	812.964	883.57
$\hat{bw}^R$	1833.522	1857.059	1808.995	1681.605
$\hat{bw}^L$	1586.416	1489.832	1718.281	1616.016

Notes: This table provides SRRD results for the sub-sample of villages that excludes those that experienced significant economic boosts, i.e. those villages with the non-residential electricity increase below the median. We include all young people in this regression, and Column 1 we use ”out-of-town” as a definition for migration, while in Column 2, we use ”out-of-county” instead. In Column 3, we focus on the subgroup who did not have a paid job in 2015. In Column 4, the dependent variable is family total salary income. The control variables are the same with Table 5.