Social security net, rural-urban migration and poverty alleviation

Evidence from a quasi-experiment of institutional reform in rural China

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Abstract

In this paper, we explore a quasi-experiment in rural China that sends county officials (outsiders) to villages as village supervisors to estimate the impact of the improved formal insurance and local governance quality on the households' migration choices and therefore income. By combining the variation around the arbitrary village borders with administrative household-level data geo-referenced to the exact location around the borders, we manage to identify this institutional effect through spatial regression discontinuity design given the fact that all other factors are continuous around the village borders. We find that: (1) the introduction of village supervisors can improve the efficiency and fairness of the formal insurance, by diminishing the favoritism by the original village cadres. (2) the improvements in formal insurance decrease the spatial misallocation of labor by substantially increasing the ultra-poor's migration to urban areas by 18.96%, especially young males and females. (3) the migration helps increase the family income by 1722.6 CNY (35.65% of the annual minimum living standard)in two years and serves as the main drive of the poverty alleviation in rural China from 2016 to 2020.

1 Introduction

There are several competing theories to explain the large wage gaps between rural and urban areas in almost every developing country (e.g.Young (2013) for 65 couturiers and Munshi & Rosenzweig (2016) for India). One theory led by Young (2013) suggests that this gap can be solely explained by the sorting of heterogenous workers across space, hence this gap reflects efficient outcomes. Another line of research believes that these gaps represent a misallocation of resources and the low mobility is driven by the well-functioning informal insurance and the absence of formal social security net for rural households (Lund (2003), Munshi & Rosenzweig (2016)), or uninsurable migration risk (Bryan et al. (2014)), whereby the movement of workers out of unproductive rural agricultural activities could yield substantial welfare gains via income and consumption improvement. Not only do these studies suggest different models to understand the behavior of the poor, but these different explanations also lead to completely different policy implications. Hence identifying and quantifying these different channels are important for theory development and for the improvement of practice.

This article exploits a quasi-experiment setting in China's Targeted Poverty Alleviation Program ("Jing Zhun Fu Pin" in Chinese, TPA afterwards) which introduced an institutional reform only for selected villages. This institutional reform brings educated and capable county officials (outsiders) into the daily village governance, which we prove in the paper will substantially improve the delivery efficiency and fairness of the social security net, especially for the unprivileged group de facto (defined as the families from different family clans with the village cadres). With administrative household-level data geo-referenced to the exact location, we manage to exploit the variation around the arbitrary village borders which are road-connected, to isolate this institutional effect through spatial regression discontinuity design, given the fact that all other factors (including culture traits and geographic factors, accessibility to infrastructure etc.) are continuous around the village borders. Our findings provide support for the second perspective, and we show that spatial labor misallocation can be mitigated through the improvement of the formal social security net and the increased temporary migration flow gives a significant rise in people's income. This effect is heterogeneously stronger for the previously unprivileged group. Last but not least, the interplay of the improved institutional system and infrastructure is strong. With better infrastructure which facilitates the daily life of the dependence that left behind, the migration trend is stronger for the healthy young and middle-aged adults.

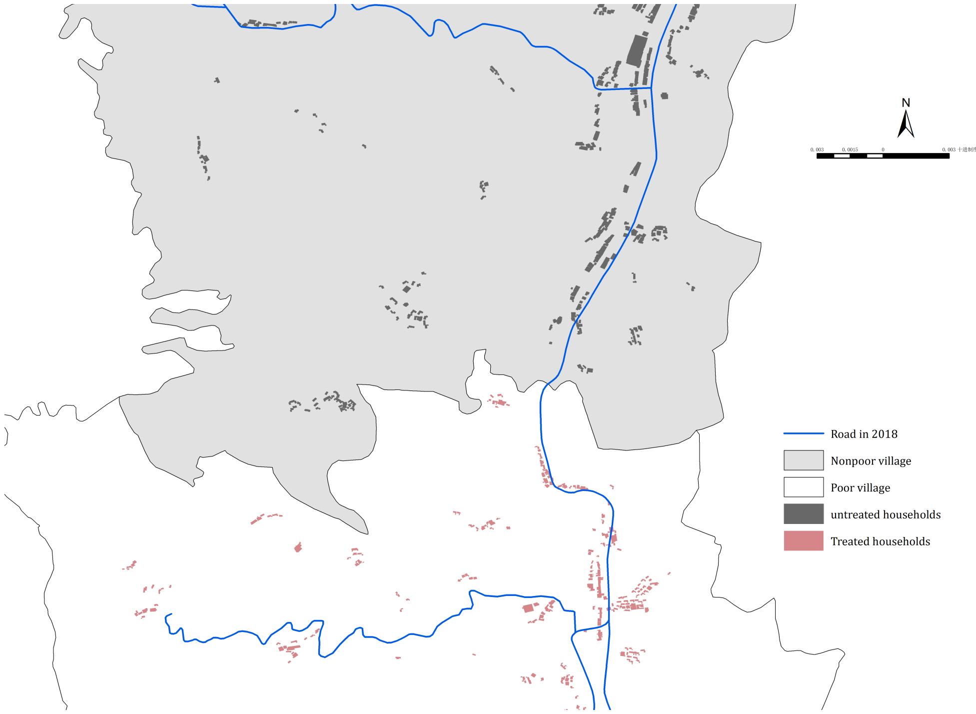
Our study is also in line with the increasing literature about institutions and their impact on economic development. Several studies have shown that the quality of institutions is a necessary condition for poverty reduction (see Acemoglu et al. (2004) and Tebaldi & Elmslie (2008) for a review). Institutions also affect the distribution of economic growth benefits across various social and political groups in a society, such that, despite similar economic performance, poverty reduction differs substantially among nations (Lopez (2004)). Yet other works downplay the role of formal institutions, emphasizing instead the importance of geographical features, informal cultural norms, genetic, and epidemiological traits (see Spolaore & Wacziarg (2013) for a review). The study of institution systems is hard to reach consensus for the following reasons. First, a major difficulty in conducting empirical work with institutions is the inherent imprecision and limitations in the definition and measurement of institutions (Tebaldi & Elmslie (2008)). From a theoretical standpoint, there is even no consensus on the definition of "institution" (see Rodrik (2000),Chong & Calderón (2000)). Second, the endogeneity of the institutions makes it hard to isolate from the unobservables like cultural, geographic features etc.. Despite the implementation of ingenious instrumental variable approaches employed in the cross-country literature, omitted variables and estimate stability remain major concerns (see Glaeser et al. (2004) and Nunn & Puga (2012) among others). Besides, since the social security net is usually uniform within one country, the common cross-country study is hard to isolate the institutional effect. One exception is Michalopoulos & Papaioannou (2014) who exploits the political boundaries in Africa to isolate the national institutional effect from the ethnicity factors. However, they cannot identify the specific institutional aspect that drives this result. Here in this paper, we go beyond the spatial regression discontinuity and study specifically the fairness of social security net and the effect of that on individuals' choices and outcomes.

The plausible random allocation of people around the village borders also allows us to generate a quasi-experiment estimates of the causal effects of internal temporary migration. Estimating the returns to migration has been hampered by difficult selection issues. Prior attempts to estimate the causal impact of migration includes various econometrics techniques (e.g. instrumental variables(Yang & Martinez (2006) for instance), panel data techniques (Beegle et al. (2011) for instance). Several policy experiments like Clemens (2010), Gibson et al. (2013) explores the exogenous changes in destination conditions to study the effects of permanent international migration. Bryan et al. (2014) is the closest in the vein to ours, which provides the first experimental evidence for internal migration impact, through the migration chances induced from a random small cash payment to rural Bangladesh villagers conditional on migrating. Our study uses data from rural China, and explores the impact through the changes in migrating resulted from the exogenous changing in the social security net efficiency within their home villages.

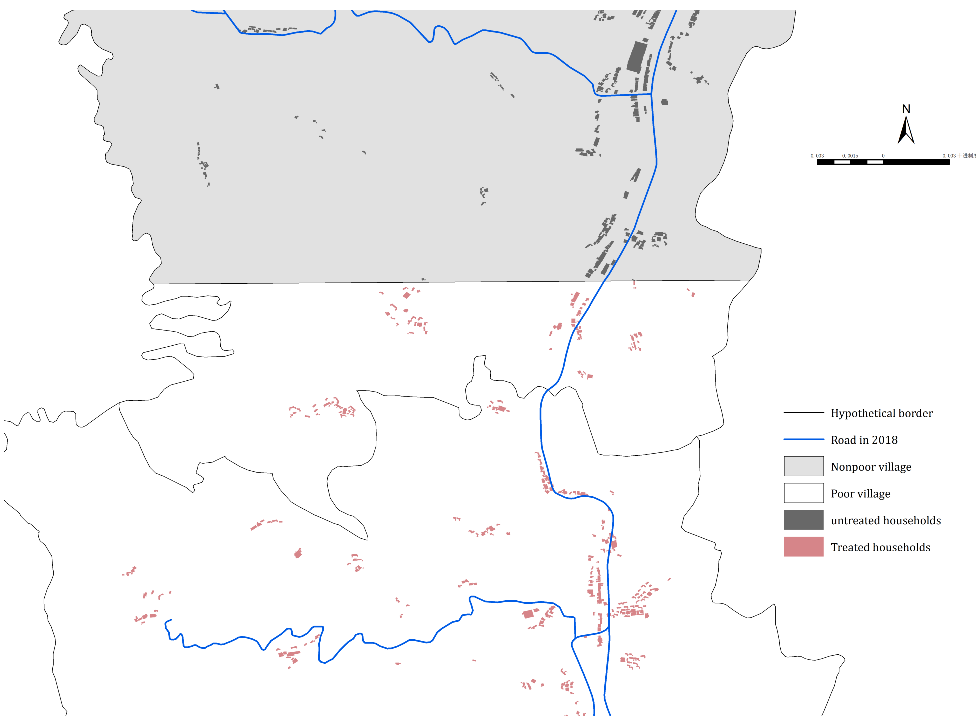
We take Figure 1 to illustrate our identification strategy. These are two adjacent villages[1], with the true border depicted in Figure 1 (a). As we can see in this typical village, the households are organized in clusters around the village, and are not distributed uniformly across the available area of the village. For these two specific villages, which are connected through a county road, the households around the borders are comparable in terms of infrastructure (schools, hospitals etc.) and market accessibility. However, the upper village is not selected for the institutional reform treatment, hence the households around the borders in these two villages are exposed to different institutional systems. Moreover, if the village border was determined hundreds of years ago as in Figure 1 (b), then the households just around the border in the upper village would've be included in the treatment. To sum up, by comparing the households around the true border, we compare the households with similar culture, infrastructure and market accessibility, geographical and climate features etc, but different institution because of the pre-determined and arbitrary village border.

Our identification strategy entails two major challenges. First, we need to prove the treatment is not selective conditional on households' characteristics. The second challenge derives from the multifaceted nature of the TPA, which not only includes the institutional reform but also other village-level and household-level treatment. On the first issue, we exploit the pre-TPA data, to show that the selection of the treatment villages is random on the observables of the villages, which is also suggested in current literature about the selection of the treatment villages (e.g.Yuan (2019)). Besides, we also compare the people

[1] According to the security reason, we cannot give the exact names of those two villages. Please contact the author for details if anyone wants to replicate this result.



(a) True border



(b) Hypothetical border

Figure 1: Illustration of the identification strategy

on the two sides of the village borders, and prove they are also comparable, which can be stated in another way, that the people around the border cannot determine the treatment decision. Theoretically speaking, even the selection depends on the village characteristics, it's implausible that it will depend on the households who live just around the borders. On the second issue, we use the road GIS data and choose only the village borders with roads crossed and linking the two sides. In this case, the households live near the borders on the two sides share the same infrastructure and market accessibility, the only difference is the institution system exposed to. Besides, we argue that most household-level treatments during TPA is not selective, which means all household in our data set are entitled to those policies. For robustness, we also directly control for the household-level policies received by the households in the regression and study the subsample of households exposed to different treatments in other aspects.

Our research also nests several other strands of the literature. First, our identification scheme that exploits border discontinuities in institutional arrangements relates to works that study the role of national policies across a particular border. The early work in this field is Miguel (2004) who compares public policies in health and education across the Kenya–Tanzania border to examine the effect of Tanzanian nation-building efforts. Michalopoulos & Papaioannou (2014) is the most relevant paper, rather than focusing on the role of national features across a single border or within a single group, their study just like ours, examines the role of overall institutions on development across all partitioned ethnicities in Africa. Hence their definition of institution is a broad definition including everything except for ethnicities. The difference of our paper is that we concentrate on the household level and specifically study the effect of institutions on the household choices and mechanism.

Second, the paper also fits into a body of literature that examines the cost of favoritism in the delivery of public services and the implementation of policies. The study of the organization of the state is rapidly expanding (see Finan et al. (2017) for a review), and our paper contributes to this growing literature by studying one representative developing region (rural China) and how the village cadres and introduction of outsiders into the village governance can affect village-level efficiency of public services. In contrast to the larger body of literature on the selection of public servants (e.g. Brollo et al. (2017), Deserranno (2019)), our empirical strategy holds selection constant and generates a quasi-experiment evidence of the abolition of within-village favoritism. Xu (2018) studies patronage in British Empire and concludes that patronage distorts the allocation of public sector positions and reduces the incentives of favored bureaucrats to perform. Our paper complement the literature by focusing on the efficiency improvement effect of social security net and the impact of that on individuals' behavior, instead of the growth-related policies and macroeconomics performance.

Last but not the least, our results relate to the large literature in China's poverty alleviation. China was once the developing country with the largest rural poor population in the world. China's poverty problem has attracted broad attention from academics at home and abroad because of the various types and complex causes of poverty and the arduous task of antipoverty (see Liu et al. (2017) for a review). Most published studies focused on the poverty line the causes of poverty (Jalan and Ravallion, 2000, Glauben et al., 2012, Wu et al., 2015), anti-poverty targets (Zhou et al. (2018)), identification problem(Shi et al. (2020)), and the poverty alleviation effect of socioeconomic development (Barrett et al. (2019)) etc.. This study serves as a policy evaluation of the institutional reform during the Targeted Policy evaluation and also meaningful for the future antipoverty policy adjustments worldwide.

The rest of the paper is organized in the following order. In the next section we introduce the quasi-experiment and TPA in general to give a context of this study. In section 3 we discuss the estimating framework and detail the identification design. In Section 4 we introduce the data we use from various sources, including administrative data on household level and village level. In Section 5, we report the estimates of the effect of institutional reform on the degree of favoritism. In Section 6, we present spatial regression discontinuity (RD) estimates that quantify the effect of the improved formal insurance delivery at the border. In section 7 we summarize discussing avenues for future research.

2 Context and Background

Villages are the smallest governance units in China. According to the Organic Law of Village Committees (1987, amended 1998), the villages are "self-governance" units (zizhi) including self-management, self-education, and self-service. The village heads who are elected by villagers[2], are most likely local elites. Those cadres elected in this way usually stay in position for as long as possible. On the other hand, since they are not the official civil servants, hence have no possibility of promotion, and only receive little monetary compensation from the government. With little supervision from the upper governments and few incentives or punishments to correct those cadres' behaviors, the village cadres redistributive and administrative powers are believed to be significant.[3]

The Targeted Poverty Alleviation Program (TPA), was launched in 2013-2015 with the goal of ending e poverty by 2020[4]. 2014-2015 can be seen as the preparation stage of TPA, including identifying the "poor households" and building up a national data management system for these people. There are altogether 29 million households (90 million poor individuals) are identified and recorded in the system In Appendix I.1, we introduce in details the identification process, which includes home visits, assets verification via administrative data and democratic votes etc., to make sure to the accuracy to the best. Since evaluat-

[2]There are usually two village cadres in one village, one is called Party Representative (zhishu), the other is called village leader (zhuren)

[3]For review essays about village election, see Gunter Schubert, "Village Elections in the PRC: A Trojan Horse of Democracy?". For review essays about village governance, see Björn Alpermann, "An Assessment of Research on Village Governance in China and Suggestions for Future Applied Research"

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

ing each household is impossible in practice, in order to make the identification process more efficient, the local government first designate some villages as the ”poor villages” (pinkuncun), and assign a quota of more than 20% to those villages, meanwhile, the rest of villages only got a quota of strictly less than 20%. In China, all together 128,000 villages are designated as ”Poor villages”. Those selected villages received extra treatment during the TPA on top of the household-level measures for the ”poor households”, including extra infrastructure investment (mainly roads and irrigation system) and institutional reform.

2.1 Institutional reform for the selected villages during TPA

The core of the institutional reform is to send county government (or even higher tier) officials to work full time in villages as the *”village supervisor”*. Those village supervisors are required to stay five work days in the villages and four nights living in the villages and in charge of all village administrative tasks, together with the original self-elected village cadres. At the same time, the department to which the village supervisor belongs (sometimes several departments together if one department doesn't have enough officials) send out a village work team (around 5 people), composed by the department head (or deputy head) and some members, to visit the village regularly. Their jobs including: (1) holding regular night talks with the villagers to get first-hand feedback from them; (2) visiting the villages spontaneously to check the village supervisor's job and provide necessary helps. In other words, instead of just one supervisor, the village team system brings in the whole department as outside helpers and supervisors. In 2017 summer, the institutional reform got deepen, by pairing each village with one town high-tier official, and forming a village responsibility team (zerenzu) together with the village supervisor, village team members and the village cadres. In other words, this policy directly links the village performance with individual town officials. Till then, the new village governance framework emerged, that for the poor villages, there are full-time staying-in village supervisor, led by paired town official and the village work team leader. Theoretically speaking, this new system changes the original ”self-governance” in the following three ways:

a. This reform brings in younger and more educated officials into the village governance. We show the comparison of age and education level in Figure 3

b. Those ”outsiders” make it possible to break up the potential favouritism within the village clans

c. This reform increases the centralization degree in the village decision making. The town government are more informed and involved in the village governance.

We depict the new system in Figure 2.

Meanwhile, for the nonpoor villages, they get village supervisors only till the mid of 2017, but not the village work team. Besides, they are paired also with town officials in 2017. We can take the 2015-2017 as the treatment period. Starting from 2018, the nonpoor villages

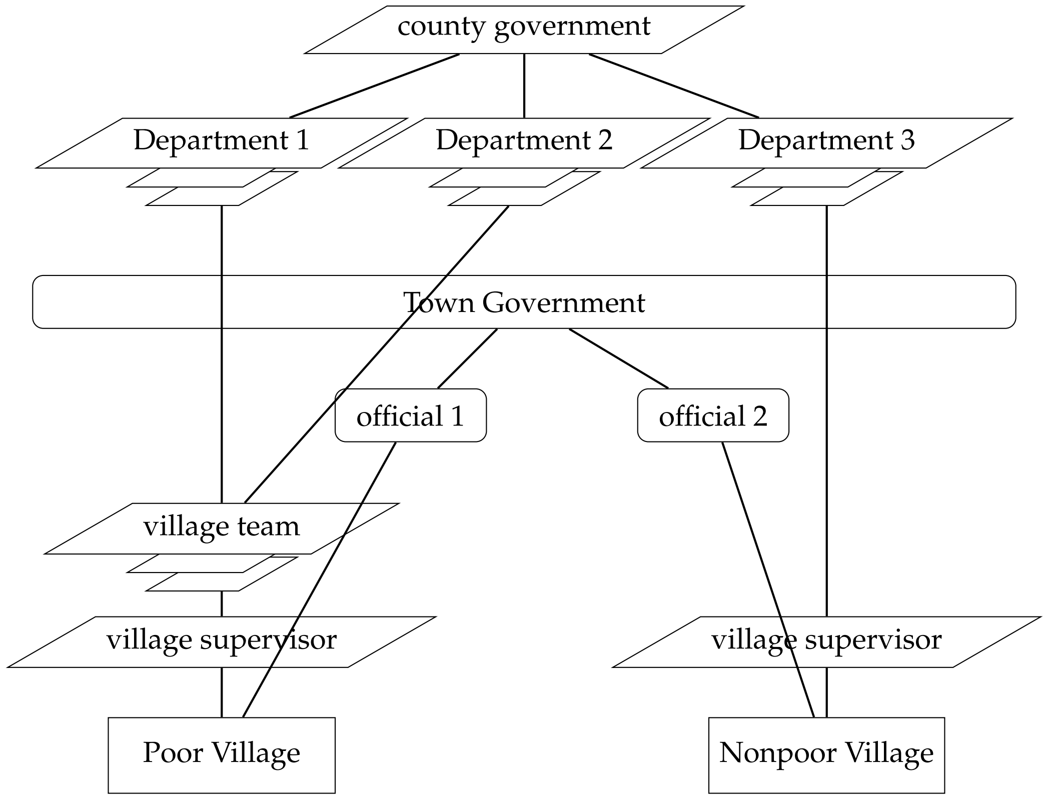


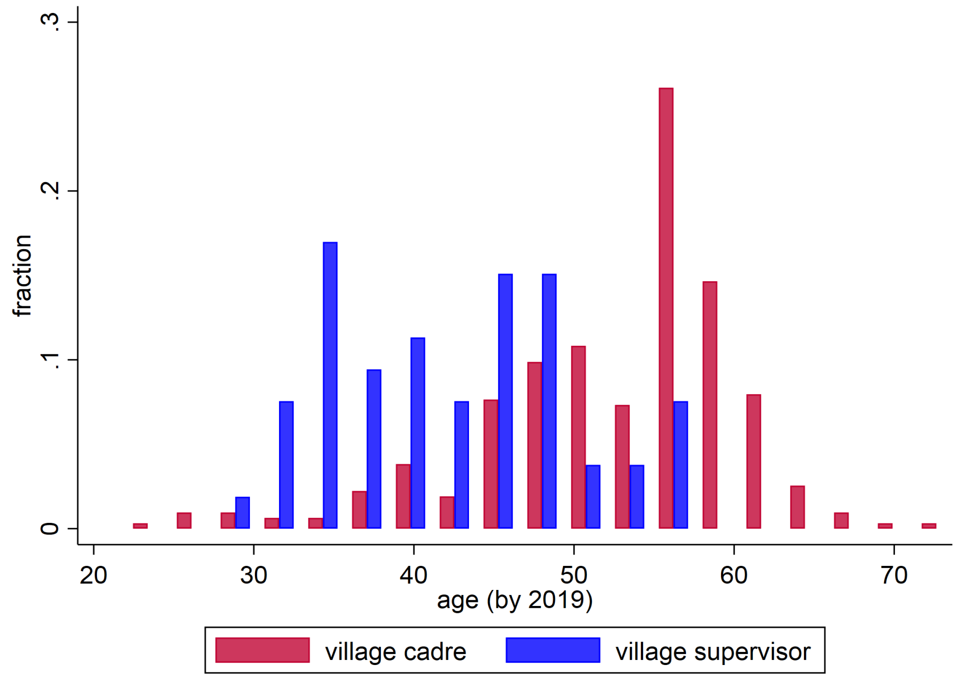
Figure 2: Institutional Reforms

also get the institutional reform but in different intensity (village team vs. one supervisor) and different supervisor quality. We compare the supervisors for the poor villages and nonpoor villages in terms of education level, current position level and department level, as shown in Figure 4[5]. As we can see, the village supervisors chosen for poor villages are better educated and have higher position and department levels.

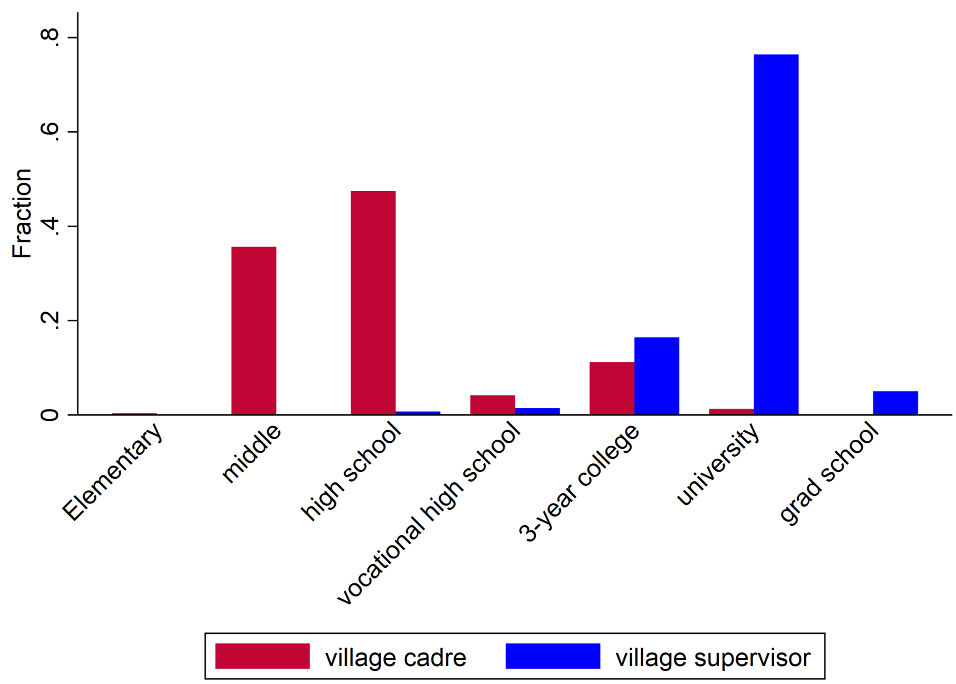
Since in different county, the exact date of the institutional reform is slightly different, we take Xin county as an example to show the timeline in more details. Starting from the end of 2014, Xin County started to pair the poor villages with one or more high tier officials (from county, prefecture or province) as the village supervisor. Till the end of 2015[6], all poor villages were paired with at least one village supervisor with the assessment standards announced. On April 23, 2017, the village supervisor policy spread to non-poor villages.On May 6, 2017, the second stage of reform started by paring each village with one town official.

[5] town official ranking to be ADDED

[6] On October 13, 2015, the Xin County Village Supervisor Performance Assessment Standards were published.

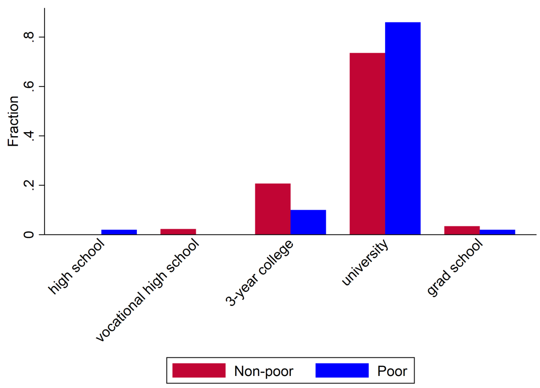


(a) Age comparison

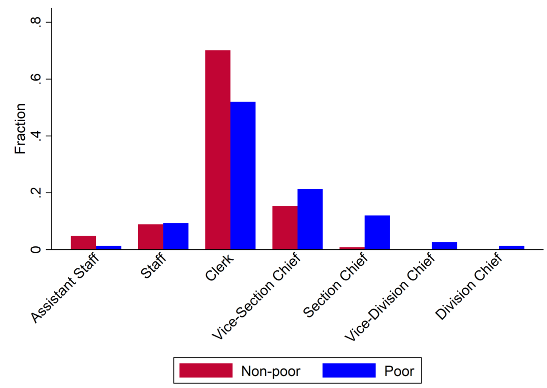


(b) Education comparison

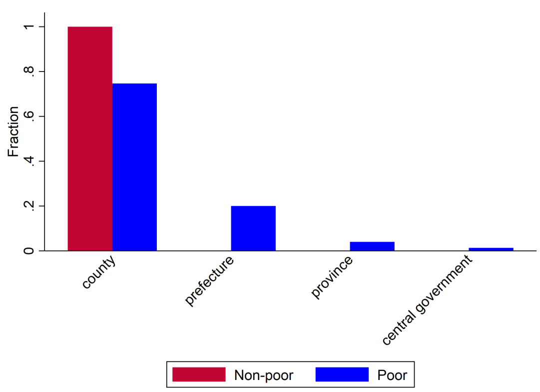
Figure 3: Comparison between village supervisors and village cadres



(a) Education comparison



(b) Position comparison



(c) Department comparison

Figure 4: Village supervisors for poor and nonpoor villages

2.2 Randomness of the village selection

One of the main threat to the identification is that the selection of the treated villages maybe not random. We will show that the selected "poor villages" and the "non-poor villages" are actually comparable in this section.

First of all, the title of "poor village" was introduced in 2001, and has been rotated among villages to balance the resources. As discussed in Yuan (2019), the selection of poor villages depend merely on the village characteristics. Given the central government has no direct information about the villages, let alone the individuals, it relies entirely on the local governments' assessment and designation. Besides, the counties cannot fully support all villages at the same time because of limited available resources. Thus, the county's practical strategy is to rotate the "poor village" titles among all the villages. 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". Consequently, a village that is designated as "*no longer poor*" or "*not poor*", may still be be comparable to the selected "poor villages", but it is no longer eligible for the funding allocated for poor villages.

To better understand the resource allocation problem and the designation of villages as poor villages (for a given and predetermined length of time), in Table 1, we compare the villages belonged to different categories in the following aspects in 2013: poverty rates, natural resources, locations, infrastructure level, characteristics of village cadres and economic activities. We divide all villages into whether poor or not from 2014 onward, the scheduling timing of ending poverty for the 2014 poor villages, and the overall poverty status from 2010 onwards including four type of villages: (a) villages that were never designated as poor; (b) villages that were designated as poor before 2014; (c) villages that were designated as poor for the 2014-2015 period (villages that were designated as poor in 2014 and scheduled to end poverty for the 2014-2015 period); and (d) villages that were designated as poor from 2016 onward (villages that were designated as poor in 2014 and scheduled to end poverty for the 2014-2015 period). As we can see, not only the selected "Poor" and "Non-poor" villages are comparable in all these village-level characteristics, but the timing of ending poverty is not correlated with either of these variables either.

3 Data

We undertook a large-scale data combination exercise to construct a household-level and an individual-level dataset of villagers. My analysis combines data from six sources: the poor data management system 2014-2020, the land registration GIS data 2018, road GIS data in 2013, 2015 and 2018, electricity usage data from 2013 to 2020, official personnel data 2015-2020, village boundary information in 2015. In this section, we provide basic data construction information and summary statistics. The Appendix I.2 provides a detailed documentation of each of the data source and the merging process.

Table 1: Descriptive statistics of villages and balance tests

	Statistics			Tests		
	Obs	mean	std. dev.	poor vs. non-poor	scheduled ending year	four categories
Poverty Rate						
Poor hh rate	73	26.738	7.664		F = 0.52 (0.6727)	
Poor population rate	73	24.793	8.498		F = 0.76 (0.5222)	
hh rate with safety net	73	7.369	7.520		F = 1.44 (0.2377)	
pop rate with safety net	73	5.433	4.565		F = 1.04 (0.3807)	
Natural Resources						
per capita cultivated land	172	0.460	0.339	F = 2.28 (0.1327)	F = 0.32 (0.8086)	F = 1.74 (0.1598)
per capita paddy fields	149	0.108	0.181	F = 2.74 (0.1002)	F = 0.59 (0.6247)	F = 1.09 (0.3536)
per capita forests	173	786.526	10273.8	F = 0.73 (0.3943)	F = 0.30 (0.8284)	F = 0.67 (0.5694)
Location						
Time to town center	176	11.355	6.295	F = 0.01 (0.9120)	F = 3.39** (0.0229)	F = 1.89 (0.1328)
Time to county center	176	38.357	16.721	F = 0.03 (0.8572)	F = 0.89 (0.4505)	F = 0.51 (0.6739)
Infrastructure Level						
Lengths of roads(km)	176	3.464	2.950	F = 0.38 (0.5384)	F = 0.19 (0.9059)	F = 0.03 (0.9942)
density of roads	176	0.565	0.487	F = 0.57 (0.4502)	F = 0.51 (0.6756)	F = 2.17* (0.0930)
Characteristics of village cadres						
Age	314	52.758	7.962	F = 0.34 (0.5612)	F = 0.25 (0.8583)	F = 0.44 (0.7265)
Education Level	314	2.939	0.985	F = 4.98** (0.0263)	F = 0.09 (0.9629)	F = 1.72 (0.1631)
Economic Activities						
# of SLCs	170	1.424	1.564	F = 0.60 (0.4405)	F = 0.66 (0.5817)	F = 0.14 (0.9338)
# of SLCs & private farms	170	1.776	1.790	F = 0.35 (0.5546)	F = 0.84 (0.4742)	F = 0.24 (0.8714)
Population Structure						
$Pop_{2013} - Pop_{2010}$	173	118.612	128.82	F = 3.34* (0.0695)	F = 2.46* (0.0699)	F = 1.13 (0.3391)
Migration rate in 2010	173	0.363	.109	F = 0.28 (0.5993)	F = 0.61 (0.6102)	F = 0.14 (0.9385)

Notes: Prob¿F in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

3.1 Data sources

Poor Registration and Management System (PRMS)

The Leading Group Office of Poverty Alleviation and Development (of the State Council) (LGOPAD) built up a nationwide registration system collecting household and individual level data (containing 29 million households and approximately 90 million individuals) and updating annually starting from 2014 till now. Authorized by the government of a typical poor county, we get those longitudinal micro data for the whole county, containing information like household assets, annual income (with decomposition) and family members' demographic information and labor supply information. In this county, there are around 12,000 poor households with 45,000 poor members, located in 173 villages.

The identification process of the poor households contains the following steps:

- Step 1: In 2014, each village submitted a finalized roster of poor households to the town and county government, after self-application, within village votes and final decision made by the village cadres.
- Step 2: After all villages finished submission, starting in 2015 November, the county government organized county and town officials, village supervisors and village working teams, to visit the nominated households one by one, focusing on the infrastructure accessibility, surrounding environment, living conditions, annual income and income-increase potential (five-aspect or "wukan"), comparing with the neighbors' to get the relative measurement. In this process, any households who satisfy the criteria would also be included. After that, those officials re-organize the village voting and finalize the second-round roster (First enter, then look, calculate, compare, vote and finalize, or "yijin,erkan,sansuan,sibi,wuyi,liuding" in Chinese)
- Step 3: In December 2015, the second-round roster send to several national bureaus for cross-comparison, with the intention to calculate the family assets (including cars, real estate, business etc.) identify the "rich" as much as possible.[7]
- Step 4: Starting in 2016, LGOPAD organized causal inspection and the county, prefecture and province government will receive punishment if any identification problem is spotted.

After the last two rounds, out of the 29 million households originally nominated by village cadres, 9.29 million were identified as "not-poor" and eliminated from the system, in the meanwhile, another 8.07 million newly identified were added in.[8] In Xin County as an example, out of the original 11,810 households nominated in 2014, the second round in November 2015 identified 18.20% falsely selected and the third round in December 2015

[7]There are six major criteria, no real estate out of the village worth more than 100,000 CNY, no private cars, or trucks worth more than 30,000 CNY, no family members work as an official or village cadre, no family member own any business, no family member has "high" stable salary (defined as the pension base large than 1,000 CNY per month).

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

identified 3.66%. These steps guarantee that the poorest households are included in this system.

Land Registration

The county government keeps a record of land GIS information for each piece of land, with the owner identity, land GPS, land property etc. In Xin County, there are all together 95,000 pieces of rural residential land. By merging the poor household information and the land registration information via national ID, we can locate 84.75% poor families.

village boundary and Road GIS information

We get village boundary information from the county government and the paved road information from the Bigemap, a professional GIS map provider in China. These two information are crucial for our regression discontinuity design (RDD) design. The village boundary GIS information makes it possible to keeping track of all existing village borders in Xin County. There are total of 173 rural villages in Xin county (with additional 23 urban districts). Of these villages 73 are poor villages, while 100 are non-poor villages. Combined with household location data, we can calculate the direct distance from each household to the village boundary. Besides, by combining the road GIS information and the household location information, we can measure the road accessibility for each household. In Figure 5, we depict the household location and the road location for one typical village in Xin County.

Village cadre personnel information

The village cadres election hold every three years, in Xin County, the exact time is 2014 December, 2018 April and 2020 December. We obtain the village cadre rosters from this three elections from the county organization department, together with each cadre's education level, age and gender. For the 2014 election, we also obtain information of the starting year of each village cadre. Hence we can construct a panel data for part of villages in 2014[9] and all villages from 2015 onward.

4 Empirical Strategy - Identification and Estimation

4.1 Identification Strategy

The impacts of better institutional system on households' income and the mechanism are challenging for economists to measure for several reasons. First is the inherent imprecision and limitations in the definition and measurement of institutions. From a theoretical standpoint, there is even no consensus on the definition of "institution". Second, the institutional system is endogenously affected by geographical features, informal cultural

[9] We can only know the village cadres for those villages that all cadres won the re-election in 2014 December

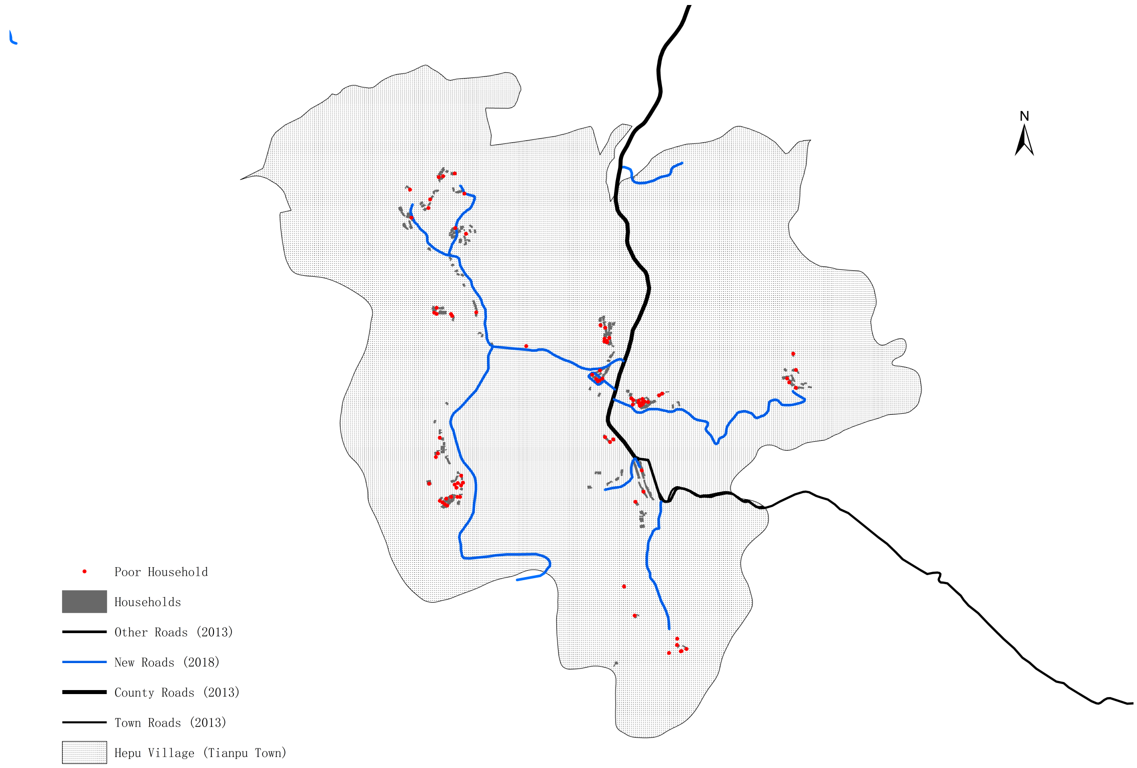


Figure 5: One typical village in Xin County

norms, genetic, and epidemiological traits etc.

We address these challenges by combining random variation around the village borders with administrative household data geo-referenced to the exact location around the borders. To be precise, we select only the village borders that have poor village on one side and non-poor village on the other side, with roads crossed and connected both sides. In this way, the households on both sides are exposed to similar geographical features, informal cultural norms, genetic traits, infrastructure (roads, irrigation, schools, hospitals, electricity etc.) and job opportunities, but different village institutions. We obtain causal identification from the discontinuity of only the institution across this specific type of village borders.

The nature of the road types end up with a spoke-wise road map in Xin County, hence we can classify the village borders into two types, connected villages and segregated villages, as depicted in Figure 6. For the connected villages, the households around the village borders are exposed to similar level of infrastructure, job opportunities, and others but different village institutions if one side is the selected poor villages and the other side is the non-poor village.

A map of Xin County with the all the villages in Xin County is provided in Figure **??**. The non-poor villages are colored in light pink, while the poor villages are colored with red and the urban areas are light grey. Note that generally the village are spread randomly across

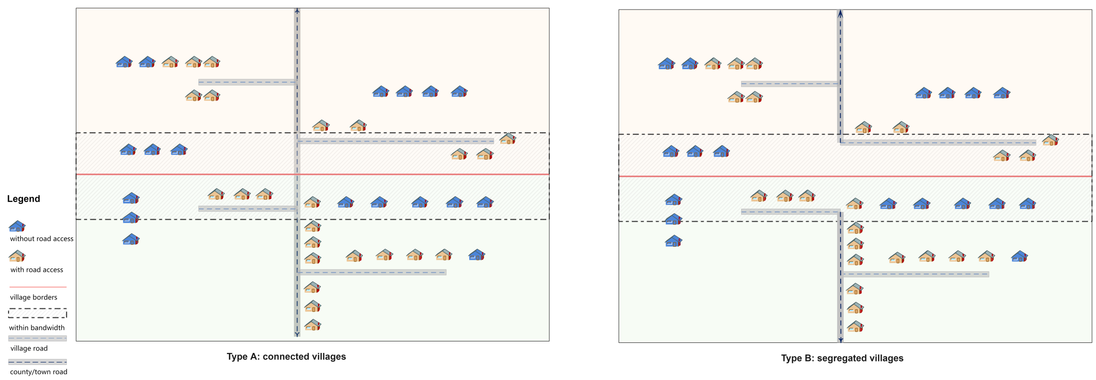


Figure 6: Two types of village borders

the county. Thus, there are many villages that border both poor and non-poor villages. The exact locations of all the borders of the villages were determined hundreds of years ago and are therefor assume to be exogenous with respect to the analysis we conduct in this study. Specifically, we assume that the exact location of any household is exogenous with respect to the location of the borders. The borders of all poor villages are colored in black and the connected villages borders with poor village on one side and the nonpoor village on the other side is colored in blue. As we can see, around half of the borders can be seen as connected borders.

One potential threat to our identification is that people may "vote by foot" and migrate after the reform. However in rural China this is not possible since the land in rural China is allocated only among the Hukou registered villagers, and land owners only have the usage right, hence cannot sell or exchange with people from outside of the village. The land allocation happens[10] every 15 years, which is long before the institutional reform happened. On the other hand, even if people can rent the land, but most redistributive public services like minimum living subsidies are arranged according to the Hukou status, hence without the village Hukou status, they still cannot entitle to the same institutional system.

We pool households according to the distance from the house location to these connected borders, with the households from the non-poor villages approaching from the negative side and poor villages from the positive side, so the running variable is household distance from the connected borders normalized to 0. Under the assumption of continuity of all other household characteristics and infrastructure/market accessibility other than village institution at the connected poor village borders, the spatial RD estimator calculates the local average treatment effect (LATE) of receiving the institutional reform for households just around the borders. Following the recommendations of Lemieux & Imbens (2008) and Gelman & Imbens (2019), our primary specification uses local linear regression within a given bandwidth of the treatment threshold, and controls for the running variable (distance to the connected village borders) on either side of the threshold. We use the following

[10] https://baijiahao.baidu.com/s?id=1668562732883109382&wfr=spider&for=pc in Chinese

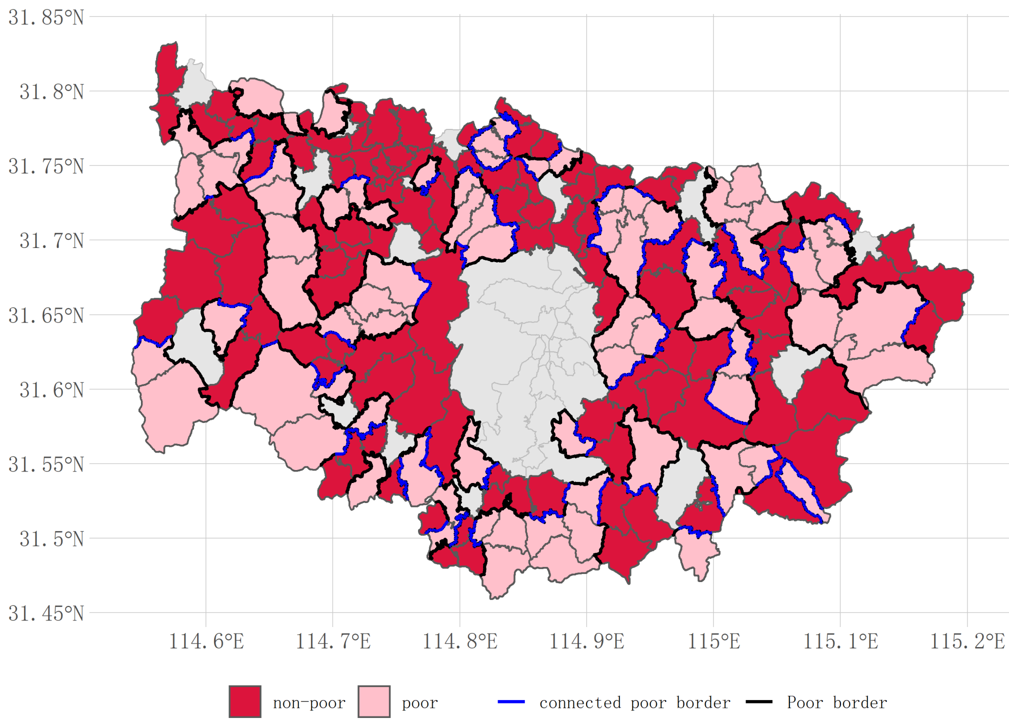


Figure 7: Map of all villages and the type of poor borders

specification:

$$Y_i = \beta_0 + \beta_1 \text{institution}_i + \beta_2 \text{distance}_i + \beta_3 \text{distance}_i \times I(\text{distance}_i > 0) + \zeta X_i + \varepsilon_i \quad (1)$$

Here, Y_i is the outcome of interest in household or individual i. distance$_i$ is the direct distance from i's house to the connected village border, with distance$_i < 0$ if i belongs to non-poor villages. X_i is a vector of household controls measured. household-level controls include indicators for family structure (family size, number of various kinds of dependence, including 0-5 year-old children, 6-15 year-old students, and 65 above senior people), accessibility of village amenities in the base year (measured by the direct distance to the paved road) and improvement during the treatment period (change of distance to the road) and household level direct transfers in the base year. The variable institution$_i$ is an indicator that takes the value 1 if i belongs to the poor village. The household controls are not necessary for identification but improve the efficiency of the estimation and can also serve as the robustness check. The coefficient β_1 captures the effect of the institutional reform on the outcome variable. The optimal bandwidth according to the method of Calonico et al. (2014) and Calonico et al. (2018). We use *epanechnikov* kernel function to construct the local-polynomial estimators. Results are robust in terms of the kernel function choices, other other bandwidth selection method, with or without the control variables.

4.2 Validity of the identification strategy

For the above mentioned strategy to be valid, there are three conditions. One is that the selection of the treatment is random, the other is that the households around the threshold (connected village borders) are comparable and continuously distributed. We've shown in the context section logically and empirically, that the selection of the treated villages is random. In this section, we will prove the second criteria one by one.

Actually, even if the selection of the villages are not random, it's hard to believe that the villagers just around the borders are the main determinants. In another word, we need to prove the individuals and families are comparable around the bordres. To prove this statement, we run a Probit regression of the households various characteristics on the probability of belonging to a treated village. If the regression has no prediction power over the treatment dummy, it's safe to say that the treatment is random around the connected village borders. As we can see in the Table 2, none of the family characteristics is significant, and none of the regression setup passes the chi-test with a Psedo R^2 consistently lower than 0.01.

Figure 8 presents the distribution of the poor population under 10% income level within each bin of 50 meters. As we can see, poor people scatter smoothly around the village borders.

variable	estimator	std.error	R^2	Wald Chi test
self-earned income (CNY)	-0.00002	(0.00004)	0.0002	0.16
Distance to road	0.00005	(0 .0002)	0.0002	0.07
Social transfer (CNY)	0.00003	0.00004	0.0007	0.47
Family size	0.010	(0.034)	0.0001	0.09
# adults	0.007	(0.049)	0.0000	0.02
# children < 5	0.005	0.0406	0.0000	0.01
# students	-0.0118	(0.0469)	0.0000	0.06
# sick	-0.037	(0.0597)	0.0004	0.39

[a] All regressions are run with robust standard errors and cluster in town level

Table 2: Comparison of family characteristics around the borders

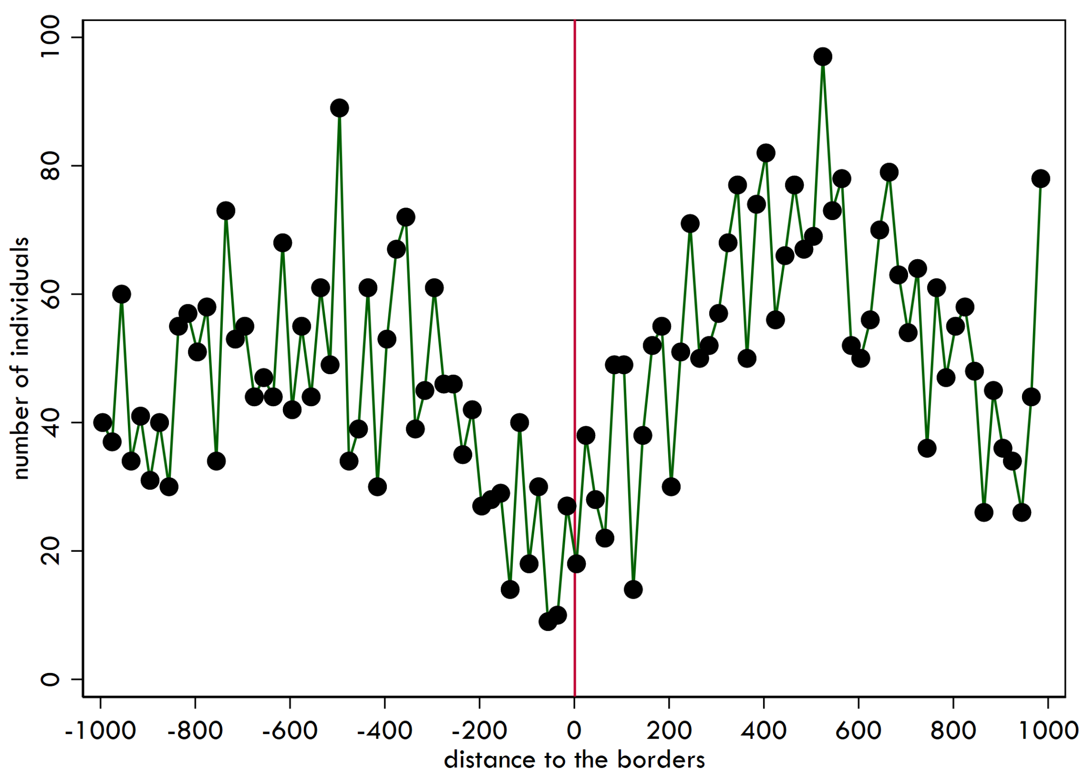


Figure 8: Distribution of number of individuals around the cutoffs

5 Institutional reform and favouritism within the family clan

In this paper, we employ the exogenous family network to proxy for unobserved social ties. By measuring connectedness through relatedness by blood, I derive a network measure that is both predetermined and objectively measurable using family surnames. The role of family ties is well documented in literature, for instance Ashraf & Bandiera (2017). Especially, family clans favoritism has been recorded in various Asian countires, including Vietnam (Do et al. (2017)), India (Khalil et al. (2021)) and also China. The use of family networks as a measure of connectedness is particularly suitable in my context, mainly because village cadres, since in rural China, village cadres possess significant power over the decision of public resources allocation, for instance the basic living allowances, and the entitlement of the "poor household".

We define a dummy variable *same_surname* as below

$$same\_surname_{ijt} = \begin{cases} 1, & suname_{ij} = cadre\_surname_j \text{ in } Year = t \\ 0, & suname_{ij} \neq cadre\_surname_j \text{ in } Year = t \end{cases} \tag{2}$$

in which i represents a household and j represents one village, hence $surname_{ij}$ is the surname of the family head (usually the oldest male) and $cadre\_surname_j$ is the surname of the village cadre in village j. Since the village cadres elections hold every three years, we define this dummy for each year t to take into consideration the change of village cadres. The village cadres tend to stay for more than one round, for instance, among the 260 Xin County village cadres who were in office in 2016, only 6.4% are newly elected cadres, and over 55.02% are in office for more than 2 rounds.

In this section, we first show that before the institutional reform, the village cadres exert significant power in terms of resource allocation within the village. Then, we employ the institutional reform introduced in the treated villages as a quasi-experiment to show that the village supervisors as an outsider with power, significantly weakens the favoritism of village cadres in the aspects of social security net provision.

5.1 Favoritism with only the village cadres

In this section, we'd like to show the favoritism of the original institutional system. As we introduced in the last section, the poor population roster were finalized by three rounds. The first round roster was solely determined by the village cadres, with the second round by outsiders (county, town officials etc.) and the last round by cross-comparison through administrative data. Hence we test the following hypothesis 1:

Hypothesis 1 (H1): *The favored group were more likely to be identified in the second round by the outsiders (since they were more likely to be falsely included in the first round).*
However, the third round shouldn't show any difference for the favored group and counterparts if the outsider participation is efficient.

We run the following multinomial probit model:

$$Pr(Round = j) = \alpha_0 + \alpha_1 same\_surname + \beta \mathbf{X} + \varepsilon \tag{3}$$

in which $Round = 2$ means identified in the second round, $Round = 3$ means in the third round and $Round = 0$ means not identified or truly poor. *same_surname* is the main explanatory variable, we use the *cadre_surname* in the end of 2014 as the cadres who nominated the first-round roster. **X** measures various household conditions, including household composition (number of children under 5, number of children in compulsory education (6-15), number of the old (over 65), number of healthy adult male and female, number of sick members) and household living conditions (within village income ranking based on village cadres reported income). Table 3 reports the regression results[11].

Variable	$Round = 2$			$Round = 3$		
same_surname	0.108***	0.110***	0.0950*	0.0764	0.0747	0.0642
	(0.0391)	(0.0392)	(0.0505)	(0.0597)	(0.0602)	(0.0729)
Family composition	No	Yes	Yes	No	Yes	Yes
Family income (reported)	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

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

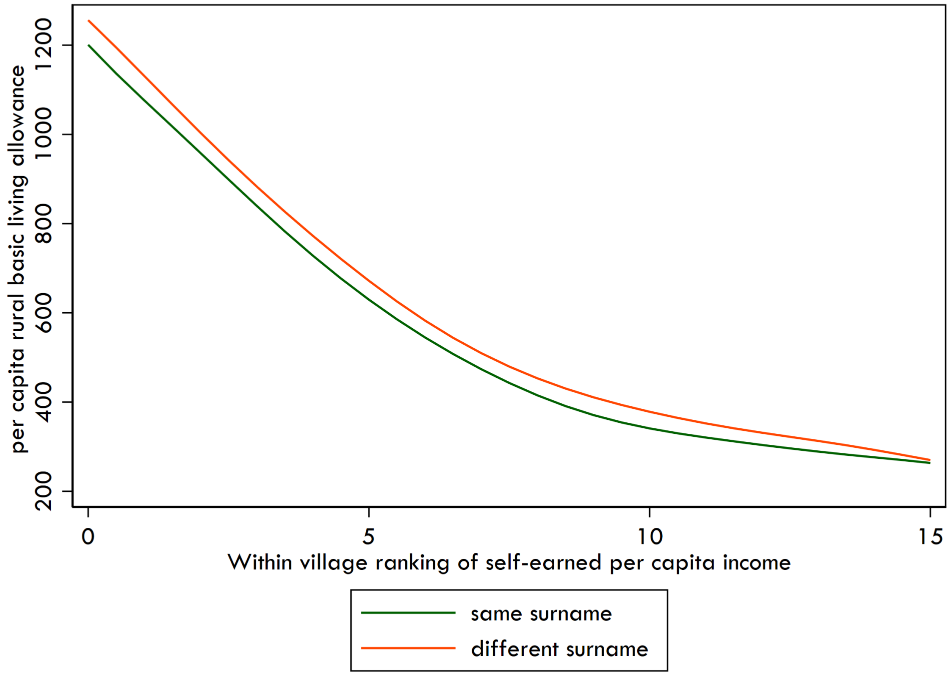
Table 3: Multinomial Probit regression results for poor households identification

5.2 Institutional reform effect

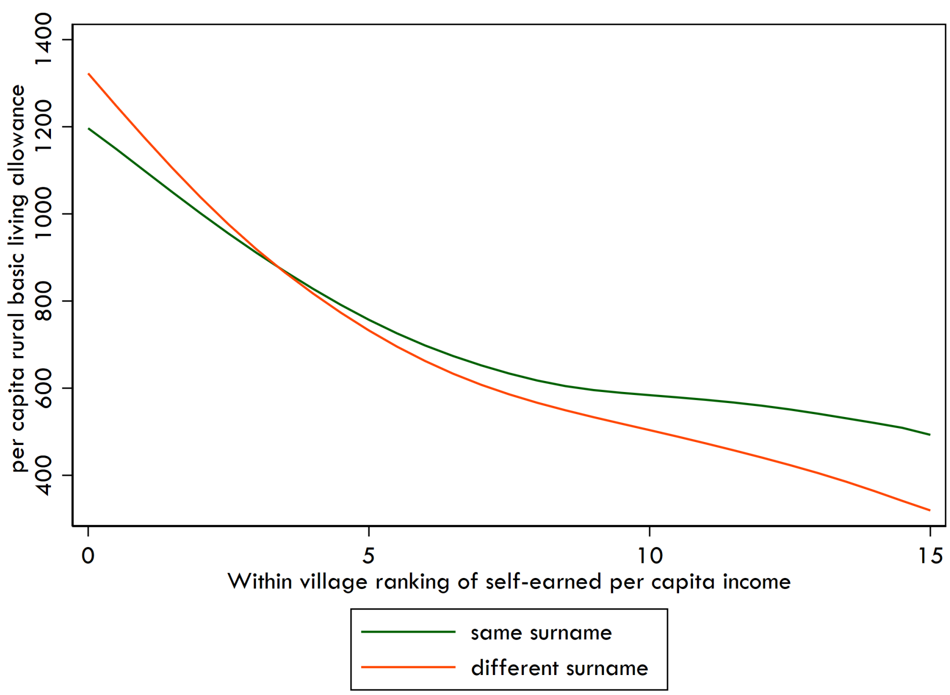
In this section, we study the difference between the treated villages and the untreated villages in terms of the resource allocation after the institutional reform. We use the basic living allowances as the measurement, as shown in Figure 9, the per capita amount of allowances each family get is highly correlated with last year's self-earn income. However in the treated village, the previously "favored" group and the "unprivileged" group are comparable for income in all levels, while in the untreated villages, even though the extremely poor (lowest 3%) are equally covered but for the slightly better families, *same_surname* group shows significant advantage, the higher the income (or in other words, the more arguable of the qualification), the larger the favor.

To get more precise results, we run regression for the same-surname families and different-surname families around the connected village borders separately. In Table 4, we can see the unprivileged group in the treated villages get significantly higher amount of allowances compared to the counterparts in the untreated villages, with the family conditions controlled. The extra amount of the unprivileged group get is comparable to the favored group lose.

[11] The full results in Online Appendix



(a) Treated villages



(b) Non-treated villages

Figure 9: After treatment: treated vs. non-treated villages

VARIABLES	lower than 5%		5%-15%	
	$same\_surname = 1$	$same\_surname = 0$	$same\_surname = 1$	$same\_surname = 0$
Conventional	-69.65	-83.18	-246.0	175.3*
	(224.0)	(177.1)	(171.5)	(102.4)
Bias-corrected	-31.43	-122.5	-272.3	215.6**
	(224.0)	(177.1)	(171.5)	(102.4)
Robust	-31.43	-122.5	-272.3	215.6*
	(275.0)	(214.7)	(207.1)	(113.8)
Observations	564	1,049	851	1,665
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Per capita allowances differences for the same and different surname groups

The difference is enlarging from 2017 to 2020 as depicted in Figure 10, we can see just as in 2018, the amount of positive difference for the different surname group in each year is comparable to the negative difference for the same surname group in each year, suggesting the strong re-allocation effect of the allowances among the two groups. In 2020, the allowance standard and scope is enlarged because of the COVID-19, we can see in this Figure that due to the institutional differences, the allocation of the extra allowances is significantly different for the treated and untreated villages.

6 Institutional reform, household income, within family labor reallocation and migration choices

6.1 Result 1: positive effect on income and labor exit from agriculture

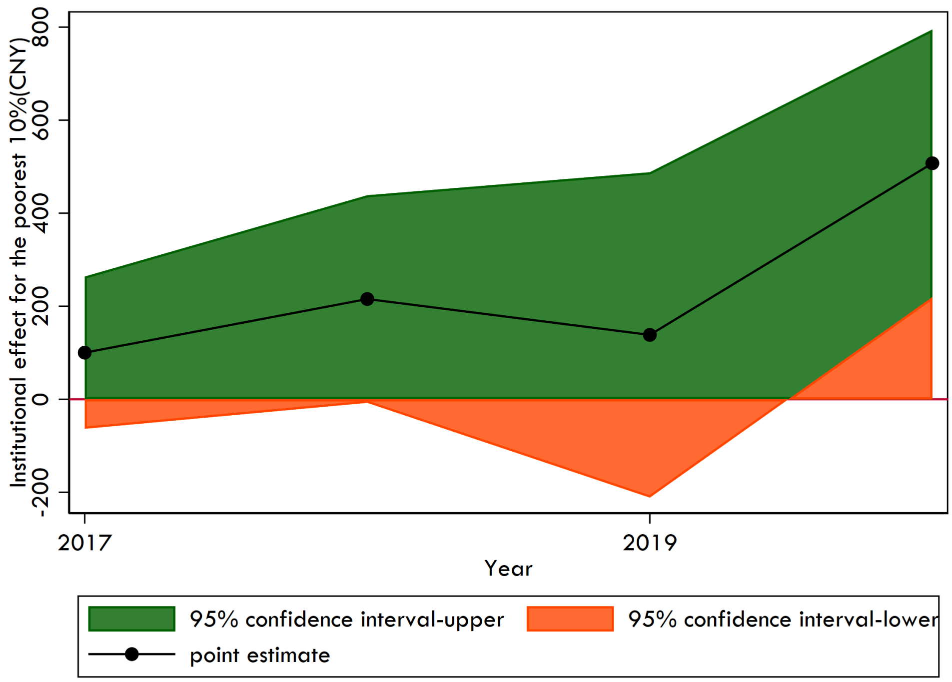
In this part, we show impact of the institutional reform on the household overall income and sources.

In order to show the heterogeneous effect for different types of households, We divided all the samples into the most vulnerable group and the relatively better group[12]. We employ two criteria for *vulnerability*, one as the per capita self-earned income in the base year (2015) and the other one as the dependency ratio.

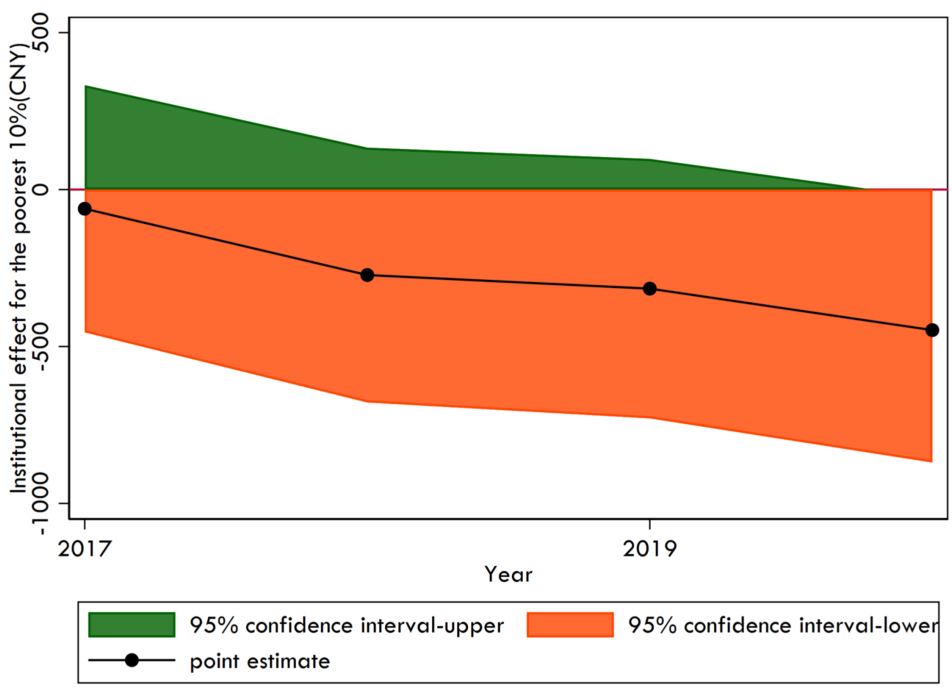
For the first criterion, we choose the lowest 10% as the most vulnerable group[13]. In the first two columns of Table 5, we present the outcome for the change of per capita

[12]better term?

[13]In Appendix I, we show the results for the lowest 2% and the lowest 8% for robustness



(a) Treated villages



(b) Non-treated villages

Figure 10: Spatial RD estimation results: treated vs. non-treated villages

self-earned overall income in 2018 relative to 2015, with and without control variables. The control variables include the base year income level (per capita self-earned income in 2015) and direct transfer level (per capita total direct transfer in 2015), families structure measurement (total number of family members, and the 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 2018 and the change of distance to the paved road from 2015 to 2018 (meters)). As we can see, the families in the reformed villages around the boundaries have significantly higher income 3 years after the treatment. In 2018, the lowest 10% families in the poor villages have around 1722.6[14] CNY more self-earned income per year per person (robust RD estimator), which is 35.63% of the annual minimum living standard[15]. Furthermore, we decompose the self-earned income into the per capita self-earned salary and per capita farming profits and attempt to find the source of the income increase. We show the impact on the salary in Column 3-4, and the impact on the farming income in Column 5-6. As we can see, that the self-earned salary income is significantly improved for the treated villagers relative to the untreated villager, with comparable magnitude of the overall income. While there is no significant difference for farming income, suggesting that the income increases mainly comes from more farmers switching to employed job and getting more salaries.

However, the high income group (lowest 10%-20%) only shows positive but not significant results, and the magnitude is much lower, about 50% of the low income group. Figure 11 Panel A, C E present graphical representations of each regression discontinuity estimate, showing the average growth of each income source (overall, salary and farming) as a function of distance from the village borders (treatment threshold), along with linear estimations on each side of the threshold and 95 percent confidence intervals. For Panel B, D and F, we first run regression of the growth with all the control variables and calculate the residuals, then we plot the average residuals as a function of distance from the village borders, to isolate the effect of the controls. In Appendix I Table **??**, we report the results of the original regressions.

For the second criterion, we choose the families with the high dependence ratio as the most vulnerable group. We choose the families with the dependence ratio higher or equal than 2/3 in the base year, which means there are 2 dependent family members and only 1 adult. For comparison, we repeat the same exercise for the families with low dependence ratio ($\leq \frac{1}{3}$, means that out of 3 families members, only 1 is dependence). As we can see in Table 13, this definition of vulnerable families give similar results compared to the first definition, that the most vulnerable families in the treated villages show significant higher income growth in overall income and the salary income, while the control group (low dependence ratio families) doesn't show any significant difference.

[14] literature

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

	Overall income		Salary income		Farming profits	
Panel A: lowest 10% in 2018						
Conventional	1550.075*	1557.091**	1536.033*	1500.759*	-16.639	1.402
	(828.597)	(789.127)	(838.544)	(788.664)	(125.144)	(132.724)
Bias-corrected	1760.640**	1722.657**	1751.433**	1663.125**	-37.374	8.741
	(828.597)	(789.127)	(838.544)	(788.664)	(125.144)	(132.724)
Robust	1760.640*	1722.657*	1751.433*	1663.125*	-37.374	8.741
	(983.511)	(931.262)	(1003.849)	(936.094)	(147.179)	(156.027)
Controls	No	Yes	No	Yes	No	Yes
Observations	3070	3018	3081	3029	3081	3029
Panel B: lowest 10%-20% in 2018						
Conventional	449.157	891.449	266.656	493.332	300.795	229.917
	(856.249)	(607.170)	(843.007)	(629.231)	(309.840)	(318.003)
Bias-corrected	298.892	814.900	219.566	407.713	258.247	171.760
	(856.249)	(607.170)	(843.007)	(629.231)	(309.840)	(318.003)
Robust	298.892	814.900	219.566	407.713	258.247	171.760
	(959.661)	(661.437)	(949.628)	(687.652)	(334.150)	(343.658)
Controls	No	Yes	No	Yes	No	Yes
Observations	1726	1725	1727	1726	1727	1726

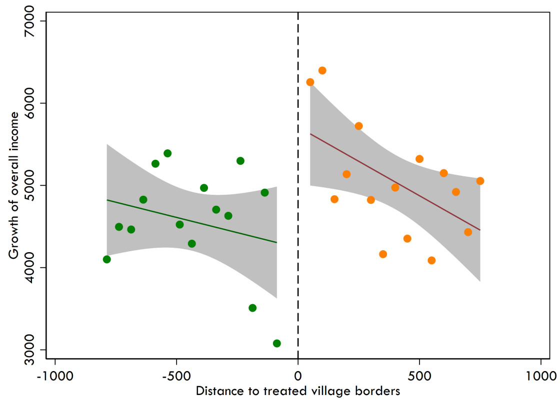
Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

[1] Column 1-2 presents the result for per-capita self-earned income, Column 3-4 presents that for per-capita self-earned salary, and Column 5-6 the per-capita farming profits (farming income - farming cost). Overall income is the sum the salary income and the farming profits.

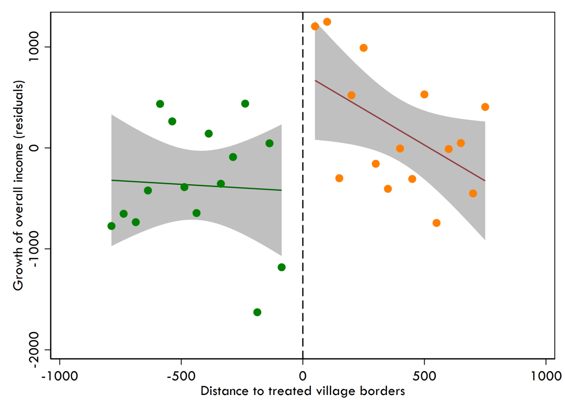
[2] The control variables include the base year income level (per capita self-earned income in 2015) and direct transfer level (per capita total direct transfer in 2015), families structure measurement (total number of family members, and the composition of the dependent members including the number 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 2018 and the change of distance to the paved road from 2015 to 2018 (meters)).

[3] All these results are obtained by *rdrobust* packages, which implements local polynomial Regression Discontinuity (RD) point estimators with robust bias-corrected confidence intervals and inference procedures developed in Calonico, Cattaneo and Titiunik (2014a), Calonico, Cattaneo and Farrell (2018), Calonico, Cattaneo, Farrell and Titiunik (2019), and Calonico, Cattaneo and Farrell (2020). We use *epanechnikov* kernel function to construct the local-polynomial estimators, and the MSE-optimal bandwidth selector *msecomb1*. Besides, we also apply heteroskedasticity-robust plug-in residuals variance estimator.

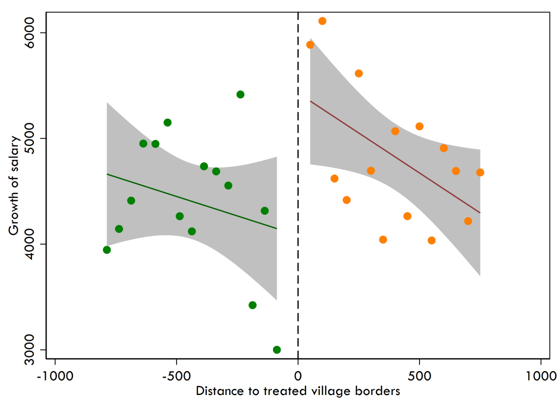
Table 5: Sharp RD results for self earned income and decomposition in 2018



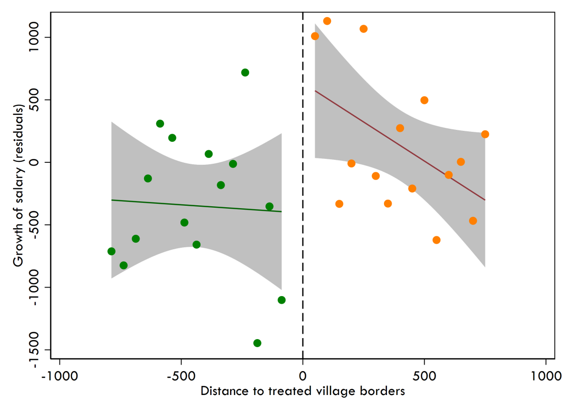
(a) Growth of Income



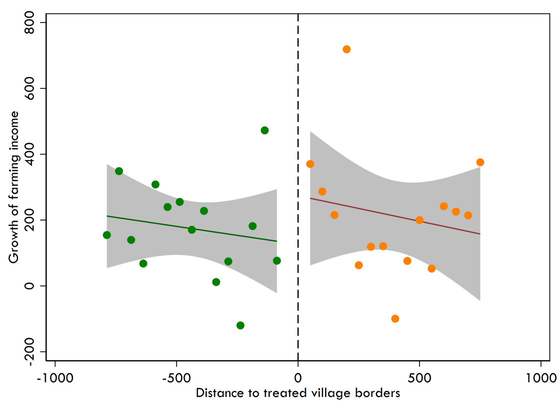
(b) Growth of Income (residuals)



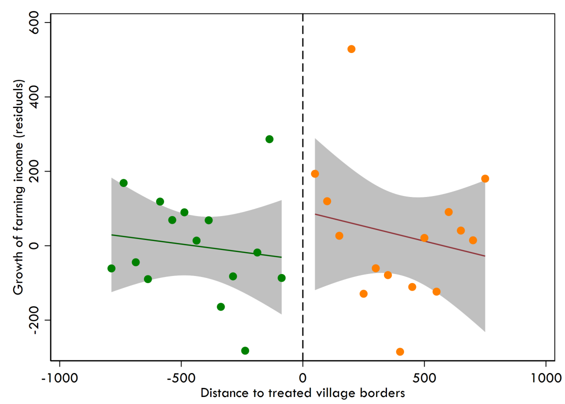
(c) Growth of salaries



(d) Growth of salaries (residuals)



(e) Growth of farming income



(f) Growth of farming income (residuals)

Figure 11: Reduced Form: Effect of institutional reform on various sources of income

	Overall income		Salary income		Farming profits	
Panel A: Dependence Ratio $\geq \frac{2}{3}$						
Conventional	1438.463*	1780.561**	1292.497	1431.572*	-72.051	1.402
	(857.411)	(874.800)	(830.347)	(842.174)	(171.547)	(132.724)
Bias-corrected	1775.902**	2093.820**	1552.219*	1705.759**	-20.332	8.741
	(857.411)	(874.800)	(830.347)	(842.174)	(171.547)	(132.724)
Robust	1775.902*	2093.820**	1552.219	1705.759*	-20.332	8.741
	(1009.501)	(1009.972)	(987.110)	(983.746)	(199.859)	(156.027)
Controls	No	Yes	No	Yes	No	Yes
Observations	1690	1678	1696	1684	1696	3029
Panel B: Dependence Ratio $\leq \frac{1}{3}$						
Conventional	-174.040	245.690	-303.123	163.266	146.672	229.917
	(1128.875)	(990.865)	(1207.259)	(1097.263)	(178.720)	(318.003)
Bias-corrected	-380.052	97.530	-467.681	60.971	157.285	171.760
	(1128.875)	(990.865)	(1207.259)	(1097.263)	(178.720)	(318.003)
Robust	-380.052	97.530	-467.681	60.971	157.285	171.760
	(1338.227)	(1197.010)	(1411.645)	(1297.623)	(211.477)	(343.658)
Controls	No	Yes	No	Yes	No	Yes
Observations	2061	2040	2065	2044	2065	1726

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[1] Column 1-2 presents the result for per-capita self-earned income, Column 3-4 presents that for per-capita self-earned salary, and Column 5-6 the per-capita farming profits (farming income - farming cost). Overall income is the sum the salary income and the farming profits.

[2] The control variables include the base year income level (per capita self-earned income in 2015) and direct transfer level (per capita total direct transfer in 2015), families structure measurement (total number of family members, and the 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 2018 and the change of distance to the paved road from 2015 to 2018 (meters)).

[3] All these results are obtained by *rdrobust* packages, which implements local polynomial Regression Discontinuity (RD) point estimators with robust bias-corrected confidence intervals and inference procedures developed in Calonico, Cattaneo and Titiunik (2014a), Calonico, Cattaneo and Farrell (2018), Calonico, Cattaneo, Farrell and Titiunik (2019), and Calonico, Cattaneo and Farrell (2020). We use *epanechnikov* kernel function to construct the local-polynomial estimators, and the MSE-optimal bandwidth selector *msecomb1*. Besides, we also apply heteroskedasticity-robust plug-in residuals variance estimator.

Table 6: Sharp RD results for self earned income and decomposition in 2018 (dependence ratio)

In order to show the time trend of the treatment effect identified by the spatial RD and also as a placebo exercise, we use instead of the change of income but the income level in each year as the dependent variable and repeat the previous exercise for the lowest 10% group for each year from 2014 to 2020 respectively. The selected optimal bandwidths for estimation and bias correction are reported in Appendix I, Table 14[16]. As we can see, the households around the selected village boundaries used to be comparable in 2014 and 2015, suggesting that our primary estimates can indeed be interpreted as resulting from the institutional reform. In the end of 2016 (one year after the treatment), the two groups are still comparable in terms of per-capita self-earned income, however, after two years, also with stronger treatment involved, the impact starts to reveal and begins to become significant and stabilized in 2018.
We repeat the placebo test for the second criterion, the dependence ratio. As we can see in Figure 12 Panel B, it shows very similar trend as the first criterion, suggesting the robustness of our results.

The two tables corroborate those graphs, showing significant treatment effects for households income and especially income from employment, but little and even negative impact on the agricultural production, suggesting labor exit from agriculture. These results broadly summarize the first finding of this paper: the most vulnerable families, with high dependence ratio or/and extremely low family income, are the biggest beneficiaries of higher quality of governance and more fair public insurance system in the villages. This institutional treatment leads to increases reallocation of labor out of agriculture after 2-3 years, and then increase of income.

To explain the impacts of institutional reform in more detail, in the next section, we continue to examine the labor participation choices and temporary migration location choices of family members.

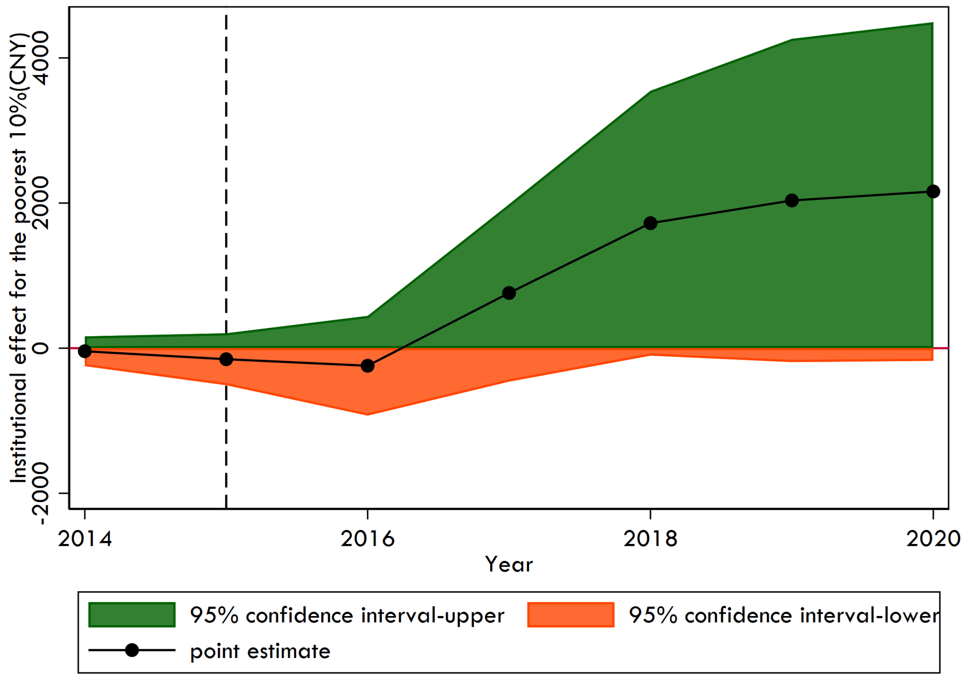
6.2 Result 2: temporary migration choices within households

In this section, we show the impact of the institutional reform on the labor supply and temporary migration choices within the family members. As argued in Munshi and Rosenzweig (2013) and other papers about the underdeveloped regions[17], there is a significant and persistent rural-urban wage gap in China as well. Especially in the mountain areas as in Xin County, there are limited labor demand within the commuting distances of the villages. Hence by temporary migrating to the urban areas, villagers can obtain higher income. In our data set, we cannot observe the individual-level salaries, but only the location and number of months employed for each family member. We define two categorical variables *employ* and *location* to describe one individual's labor supply and temporary migration choices:

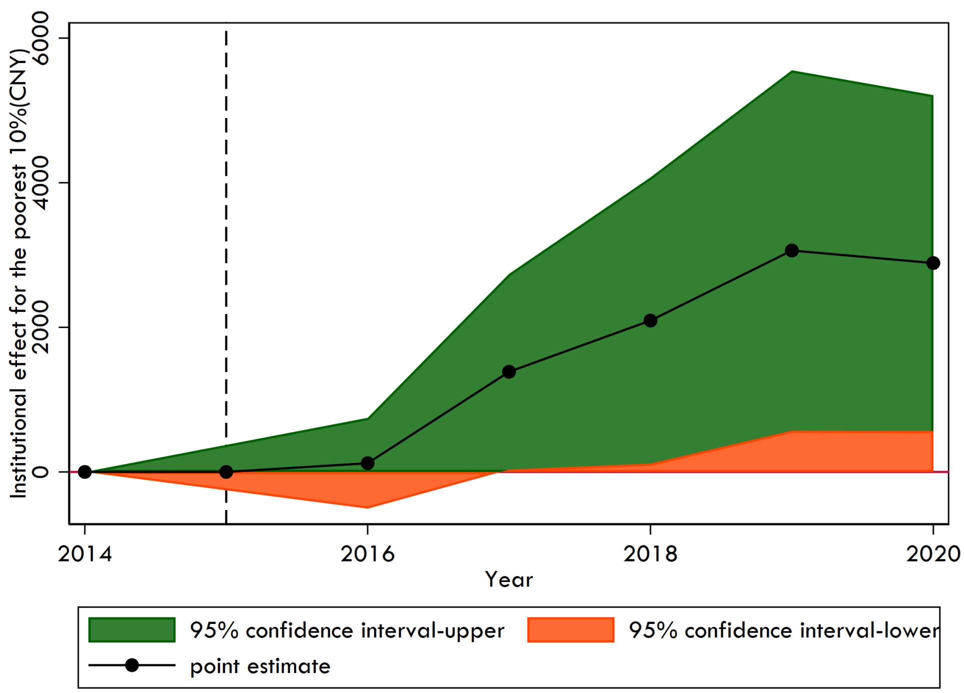
- Whether exit the agricultural sector and get a paid job or not (either unemployed or

[16]For year 2014 and 2015, we exclude the control variable base year income level since it's the dependent variable now

[17]literature



(a) lowest 10% households



(b) $\geq \frac{2}{3}$ dependence ratio households

Figure 12: Placebo tests

stay in the agricultural sector) :

$$employ = \begin{cases} E, & \text{get a paid job} \\ U, & \text{no paid job} \end{cases} \tag{4}$$

- Whether temporarily migrate to urban areas and get a paid job, or stay in the commuting distance (within town) and get a paid job, or not:

$$location = \begin{cases} Town, & \text{stay in town and get a paid job} \\ Migration, & \text{migrate to urban areas and get a paid job} \\ U, & \text{Don't have a paid job} \end{cases} \tag{5}$$

In Appendix II.3 Table ??, we report some indirect evidence to show the rural-urban wage gap in this county and also the gender and age pay gap. Based on the OLS regression, the coefficient of the young males' total working months in urban areas is 14.13% higher than that of the young female's and 84.8% higher than that of the old males. Only for the old males, the coefficient is negative, suggesting that compared to staying in the agricultural sector, get a paid job in the rural areas is potentially unprofitable. Based on the wage gap, if the whole family cannot permanently migrate to the urban areas all together, the optimal allocation of the labor within household should follow the wage order, i.e., the young male and female should temporarily migrate to the urban as more as possible. In the rest of the section, we prove that the institutional reform in the village leads to better allocation of labor supply and migration choices within households.

Following Xu (2017)'s method on RD with categorical outcome, we report the results for *employ* and *location* in 2018 in Table 7 and 8. As we can see in Table 7, relative to the reference status (U), the young female shows significant higher rate of exiting from agricultural sector and getting paid jobs. We can cooperate two tables, and find the higher labor participation of the young female can be decomposed into higher rate of migration (ATE = 18.96%) and lower rate of working in rural areas (ATE = -7.85%) . The higher rate of migration of young males is also significant (ATE = 14.73%). The old family members, both females and males show negative labor participation in both locations. Those results combined show better labor allocation within the households. In Appendix II.4, Table 16 and 17, we report the regression results for 2014. As we can see, the four types of family members didn't show any significant differences in either the labor supply choices or the migration choices.

6.3 Result 3: Stronger effect for the unprivileged group

As we show in the last section, that the resources allocation in the treated villages has been improved by the institutional reform, especially for the previously unprivileged group, represented by the families with different surnames with the village cadres. In this section, we show that the income effect and migration effect for this unprivileged group is even stronger compared to the average. As shown in Table 9, not only the effect is larger for

	Male		Female	
	16-50 year-old	51-75 year-old	16-50 year-old	51-75 year-old
Panel A:τ (employed)				
$ATEs(\hat{\tau})$	0.0403	-0.1685	0.0957	0.1055
90% CI	[-0.0387, 0.1884]	[-0.3042, -0.1073]	[0.0348, 0.2039]	[-0.0653, 0.1955]
Robust 90% CI	[-0.0847, 0.2344]	[-0.3187, -0.0928]	[0.0313 ,0.2074]	[-0.0800, 0.2103]
t-test	1.0839	-3.4375	2.3214	0.8216
t-test P-value	0.2784	0.0006	0.0203	0.4113
Robust t-test	0.7715	-2.9960	2.2292	0.7381
Robust t-test P-value	0.4404	0.0027	0.0258	0.4605
Sample Size	2,109	1,678	1,507	1,294
$\hat{h}$	808.4903	617.7516	567.5615	575.0811
Wald test	1.1749	11.8165	5.3891	0.6751
Wald test p-value	0.2784	0.0006	0.0203	0.4113
Robust Wald test	0.5953	8.9761	4.9694	0.5447
Robust Wald test p-value	0.4404	0.0027	0.0258	0.4605

Table 7: Regression Discontinuity for employment choice in 2018

	Male		Female	
	16-50 year-old	51-75 year-old	16-50 year-old	51-75 year-old
Panel A:τ_1 (work in town)				
$ATEs(\hat{\tau})$	-0.0187	-0.0178	-0.0785	-0.0950
90% CI	[-0.0972 , 0.0284]	[-0.1006, 0.0482]	[-0.1820 , -0.0191]	[-0.1945, -0.0501]
Robust 90% CI	[-0.0992, 0.0303]	[-0.2852 , 0.2328]	[-0.1954 , -0.0057]	[-0.2285,-0.0161]
t-test	-0.9010	-0.5790	-2.0309	-2.5306
t-test P-value	0.3676	0.5626	0.0423	0.0114
Robust t-test	-0.8745	-0.1663	-1.7439	-2.2914
Robust t-test P-value	0.3819	0.8679	0.0812	0.0219
Panel B:τ_2 (work out-of-town)				
$ATEs(\hat{\tau})$	0.1473	-0.0029	0.1896	-0.0293
90% CI	[0.0702 , 0.2827]	[-0.0803 , 0.0627]	[0.0381 , 0.2662]	[-0.1126 , 0.0191]
Robust 90% CI	[0.0673 , 0.2856]	[-0.2581 , 0.2405]	[0.0125 , 0.2918]	[-0.1203 , 0.0268]
t-test	2.7323	-0.2029	2.1946	-1.1680
t-test P-value	0.0063	0.8392	0.0282	0.2428
Robust t-test	2.6590	-0.0582	1.7926	-1.0457
Robust t-test P-value	0.0078	0.9536	0.0730	0.2957
Sample Size	2,109	1,678	1,507	1,294
$\hat{h}$	447.6272	1374.6	650.6572	631.4855
Wald test	7.6905	0.5012	7.2515	8.3149
Wald test p-value	0.0214	0.7783	0.0266	0.0156
Robust Wald test	7.2970	0.0412	5.0610	6.7752
Robust Wald test p-value	0.0260	0.9796	0.0796	0.0338

Table 8: Regression Discontinuity for location choice in 2018

the lowest 10% group, compared to the average, the lowest 10%-20% group also shows significant positive improvement in terms of income. This helps to prove once again the impact of institutional reforms on the allocation equality and therefore on people's behaviors.

We look into the individual-level work and migration choices, and concentrate on those who used to not work in 2015 from this unprivileged group. The result is shown in Table 10. As expected, the young male and female show strong trend of migrating out instead of staying at home and working in the commuting distance.

6.4 Result 4: Interplay of institution and infrastructure

On top of the average treatment effect of the institutional reform, we'd like to know the heterogeneous effect of the institution for families with different access to infrastructure. With the exact house location and the road GIS information in 2015 and 2018, we can calculate the distance to the nearest paved roads as crow flies in these two years. Since those villages are connected by roads and near the border, they should share similar infrastructure like internet connection etc., hence the road access can measure the general infrastructure each household can access, including but not limited to the accessibility to hospitals, schools, irrigation system etc.. Based on the distance in 2015, we can classify the households into "road access" group and "insufficient road access and no improvement" group in the base year:

$$road = \begin{cases} \text{access}, & \text{Distance} \leq 150 \text{ m} \\ \text{insufficient}, & \text{Distance} > 150m \& \Delta \text{ Distance} \leq 10m \end{cases} \tag{6}$$

As we can see in Table 11, the households with road access show significantly higher increase in income compared to the average treatment effect, suggesting the cooperative effect between the institution and infrastructure. With better infrastructure, institution's labor allocation effect is intensified.

We should be careful about the interpretation of results for those with insufficient infrastructure access. In the previous section, we've already proved the institutional reform delivers a much fair public goods allocation system, with the direct transfer as an example. The road access is also one public goods, hence for the households without road access in 2015, the road improvements are also determined by the two different institutional system around the village borders. In other words, the sample of no road access in 2015 and no improvement within the three years are endogenous selected samples on the two sides, which are potentially affected by the institutional reform.

To summarize, we observe a better allocation of labor within households driven by the better institution, with a large shift of young female out of agricultural work and into wage

	Overall income		Salary income		Farming profits	
Panel A: lowest 10% in 2018						
Conventional	2250.979**	2194.174**	2326.387**	2410.666**	-44.900	-95.109
	(1041.554)	(937.667)	(1060.891)	(964.472)	(174.172)	(171.604)
Bias-corrected	2549.954**	2463.261***	2662.835**	2744.461***	-52.868	-104.733
	(1041.554)	(937.667)	(1060.891)	(964.472)	(174.172)	(171.604)
Robust	2549.954**	2463.261**	2662.835**	2744.461**	-52.868	-104.733
	(1218.846)	(1096.963)	(1256.298)	(1137.200)	(206.003)	(204.058)
Controls	No	Yes	No	Yes	No	Yes
Observations	2038	1989	2045	1996	2045	1996
Panel B: lowest 10%-20% in 2018						
Conventional	1548.620**	1494.931*	1725.469**	1492.503*	-199.314	-10.404
	(788.348)	(791.891)	(758.458)	(804.166)	(357.157)	(358.744)
Bias-corrected	1497.702*	1484.611*	1712.264**	1366.503*	-251.443	3.504
	(788.348)	(791.891)	(758.458)	(804.166)	(357.157)	(358.744)
Robust	1497.702*	1484.611	1712.264**	1366.503	-251.443	3.504
	(902.959)	(940.801)	(859.543)	(921.866)	(372.687)	(493.949)
Controls	No	Yes	No	Yes	No	Yes
Observations	1161	1160	1162	1161	1162	1161

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

1 Column 1-2 presents the result for per-capita self-earned income, Column 3-4 presents that for per-capita self-earned salary, and Column 5-6 the per-capita farming profits (farming income - farming cost). Overall income is the sum the salary income and the farming profits.

2 The control variables include the base year income level (per capita self-earned income in 2015) and direct transfer level (per capita total direct transfer in 2015), families structure measurement (total number of family members, and the 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 2018 and the change of distance to the paved road from 2015 to 2018 (meters)).

3 All these results are obtained by *rdrobust* packages, which implements local polynomial Regression Discontinuity (RD) point estimators with robust bias-corrected confidence intervals and inference procedures developed in Calonico, Cattaneo and Titiunik (2014a), Calonico, Cattaneo and Farrell (2018), Calonico, Cattaneo, Farrell and Titiunik (2019), and Calonico, Cattaneo and Farrell (2020). We use *epanechnikov* kernel function to construct the local-polynomial estimators, and the MSE-optimal bandwidth selector *msecomb1*. Besides, we also apply heteroskedasticity-robust plug-in residuals variance estimator.

Table 9: Sharp RD results for self earned income and decomposition in 2018, hhs with different surnames

	Male		Female	
	16-50 year-old	51-75 year-old	16-50 year-old	51-75 year-old
Panel A:τ_1 (work in town)				
$ATEs(\hat{\tau})$	-0.1214	0.0531	-0.1521	0.0313
90% CI	[-0.3358, -0.0137]	[-0.0555, 0.3296]	[-0.2460, -0.0253]	[-0.0541, 0.0568]
Robust 90% CI	[-0.3670 , 0.0175]	[-0.0676, 0.3417]	[-0.2572 , -0.0141]	[-0.0693, 0.0720]
t-test	-1.7849	1.1705	-2.0213	0.0402
t-test P-value	0.0743	0.2418	0.0433	0.9679
Robust t-test	-1.4951	1.1014	-1.8356	0.0316
Robust t-test P-value	0.3819	0.8679	0.0812	0.0219
Panel B:τ_2 (work out-of-town)				
$ATEs(\hat{\tau})$	0.2677	0.0967	0.2005	-0.0135
90% CI	[0.1159 ,0.5464]	[-0.0101 ,0.2174]	[0.1211 ,0.3629]	[-0.1075 -0.0138]
Robust 90% CI	[0.0612, 0.6012]	[-0.01,88 0.2261]	[0.1031, 0.3809]	[-0.112,8 -0.0085
t-test	2.5303	1.4992	3.2933	-2.1292
t-test P-value	0.0114	0.1338	0.0010	0.0332
Robust t-test	2.0174	1.3925	2.8650	-1.9132
Robust t-test P-value	0.0437	0.1638	0.0042	0.0557
Sample Size	546	587	904	649
$\hat{h}$	501.3000	426.2573	582.0986	609.0680
Wald test	7.4809	4.1743	13.3429	4.5341
Wald test p-value	0.0237	0.1240	0.0013	0.1036
Robust Wald test	4.8787	3.6470	10.3152	3.6606
Robust Wald test p-value	0.0872	0.1615	0.0058	0.1604

Table 10: Regression Discontinuity for location choice in 2018, different surnames

	Overall income		Salary income		Farming profits	
Panel A: lowest 10% with road access						
Conventional	3062.194**	3701.953***	2674.187*	3236.693**	416.628	501.387**
	(1347.956)	(1313.663)	(1380.999)	(1321.105)	(269.541)	(224.441)
Bias-corrected	3443.679**	4216.561***	2932.805**	3617.396***	538.591**	652.227***
	(1347.956)	(1313.663)	(1380.999)	(1321.105)	(269.541)	(224.441)
Robust	3443.679**	4216.561***	2932.805*	3617.396**	538.591*	652.227**
	(1618.086)	(1604.899)	(1687.876)	(1623.225)	(310.675)	(273.435)
Controls	No	Yes	No	Yes	No	Yes
Observations	1063	1044	1065	1046	1065	1046
Panel B: lowest 10% without road access						
Conventional	1134.271	726.297	1507.821	1531.427	-519.338**	-473.775*
	(1831.201)	(1892.886)	(1706.537)	(1814.489)	(246.228)	(267.918)
Bias-corrected	1031.948	521.144	1597.664	1492.317	-601.662**	-567.378**
	(1831.201)	(1892.886)	(1706.537)	(1814.489)	(246.228)	(267.918)
Robust	1031.948	521.144	1597.664	1492.317	-601.662**	-567.378*
	(2152.539)	(2225.452)	(2057.751)	(2163.109)	(274.484)	(307.523)
Controls	No	Yes	No	Yes	No	Yes
Observations	626	512	630	516	630	516

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

[1] Column 1-2 presents the result for per-capita self-earned income, Column 3-4 presents that for per-capita self-earned salary, and Column 5-6 the per-capita farming profits (farming income - farming cost). Overall income is the sum the salary income and the farming profits.

[2] The control variables include the base year income level (per capita self-earned income in 2015) and direct transfer level (per capita total direct transfer in 2015), families structure measurement (total number of family members, and the 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). We don't include the change of distance to the paved road from 2015 to 2018 (meters) as in the main regression, is because that the change within each sample is very small.

[3] All these results are obtained by *rdrobust* packages, which implements local polynomial Regression Discontinuity (RD) point estimators with robust bias-corrected confidence intervals and inference procedures developed in Calonico, Cattaneo and Titiunik (2014a), Calonico, Cattaneo and Farrell (2018), Calonico, Cattaneo, Farrell and Titiunik (2019), and Calonico, Cattaneo and Farrell (2020). We use *epanechnikov* kernel function to construct the local-polynomial estimators, and the MSE-optimal bandwidth selector *msecomb1*. Besides, we also apply heteroskedasticity-robust plug-in residuals variance estimator.

Table 11: Sharp RD results for self earned income and decomposition in 2018, with and withour road access

work, and more young male and female temporarily migrate into urban areas to obtain higher wage. This occupational change and migration choices change lead to significant increases in income. The impact starts to reveal in 2-3 years and get stabilized. In the meantime, we observe the cooperative effect of the institution and the infrastructure, that households with better infrastructure access show higher improvement in income.

6.5 Robustness

In this section we examine the robustness of our results to alternative specifications and explanations.

A major threat to our identification could come from any other treated village specific policy or characteristics that also have discontinuity around the village borders. As we argue in the context section, we constrain our discussion within the connected villages in order to make sure the infrastructure accessibility is continuous around the village borders, then anything that could be discontinuous around the village borders even with the connected roads will be the potential threat to our identification.

In fact, one potential channel could be that, the better institution system in the treated villages with the additional rural collective economy funding may boost the village economy. Even though all public infrastructure is inclusive in terms of the residence, the village collective firms maybe favor the residents from the same village, inducing de facto discontinuity in terms of job opportunities across the village borders. Besides, we also collect the staff rosters of all village firms from one village in Xin county, and present the staff village origin distribution in Appendix II.5. As we can see, the favoritism towards the people from the same village is significant.

It is unlikely that this program is spuriously driving our results for two reasons. First of all, in the main results, we observe larger migration flow in the treated villages, there is little theoretical reason to believe that increased local labor demand could drive large increases in migration to urban areas. Secondly, as we proved in our paper Li et al. (2021b), the institution reform and extra funding can only increase the firm number in the short run but not the actual labor demand. In other words, whether the village local labor demand will be increased in the short run can be seen as a random event conditional on village characteristics. Hence this shouldn't significantly affect our results.

To furthermore examine the robustness of our results, we exclude those villages with with more than 33%[18] accumulated increase in total nonresidential electricity consumption from 2015 to 2018. As shown in Figure 13, we can classify all villages not only by the treatment type but also by the local economy condition. The light pink villages are the treated villages but no significant improvement in terms of village economy, while the light blue villages are the untreated villages without economy improvement. As we can see, the geographical distribution of the villages are even around the whole county.

[18]On average, this means 10% annual increase.

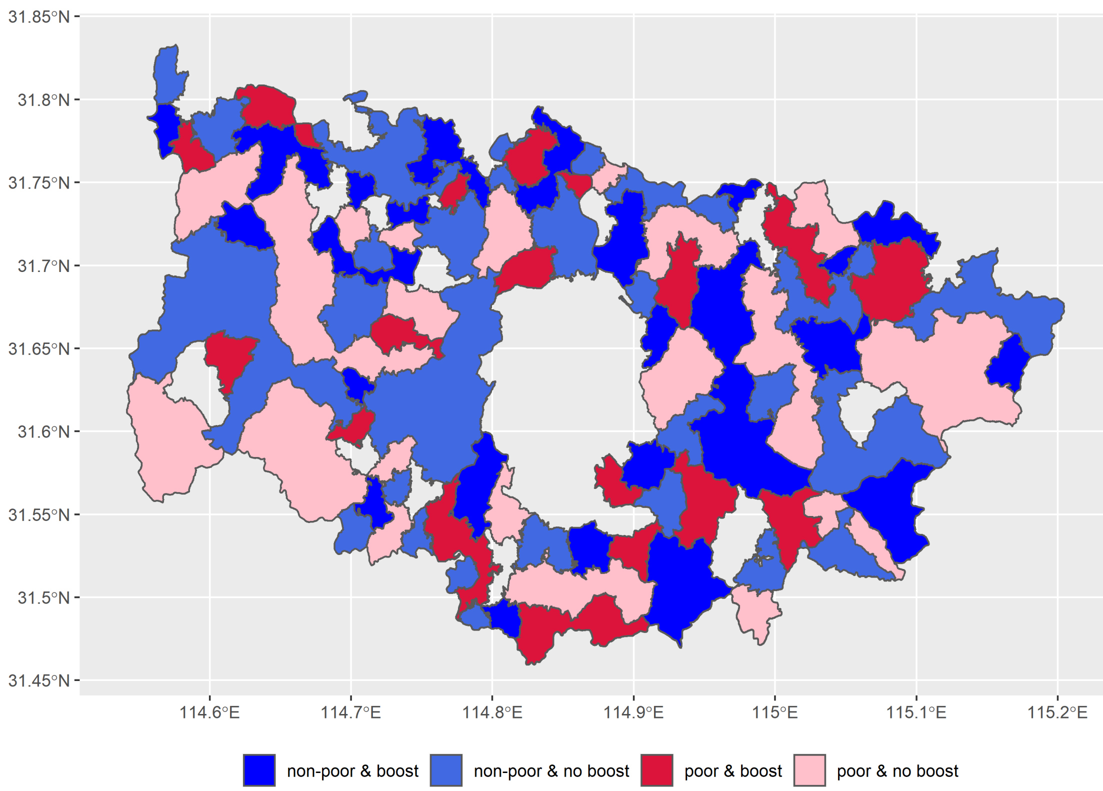


Figure 13: Treatment type and local economy condition

We report the spatial RD regression results in Table 12 for the two definitions of most vulnerable groups. As we can see, the main results stay the same with this more constrained subsample and confirm furthermore that the pure direct institutional effect on households through institutional quality, without improvement in local economy condition.

7 Conclusions

In this paper, we employ a quasi-experiment in rural China that send county officials (outsiders) to villages as village supervisors to estimate the impact of the improved formal insurance and local governance quality on the households' migration choices and therefore

	Overall income		Salary income		Farming profits	
Panel A: lowest 10% group						
Conventional	2695.800**	2859.383**	2706.849**	2856.314**	-53.076	-77.536
	(1250.813)	(1187.662)	(1263.265)	(1200.525)	(182.675)	(169.535)
Bias-corrected	2980.760**	3041.557**	3041.734**	3067.243**	-73.446	-106.927
	(1250.813)	(1187.662)	(1263.265)	(1200.525)	(182.675)	(169.535)
Robust	2980.760**	3041.557**	3041.734**	3067.243**	-73.446	-106.927
	(1452.588)	(1360.265)	(1486.514)	(1381.469)	(219.881)	(204.524)
Controls	No	Yes	No	Yes	No	Yes
Observations	2141	2112	2152	2123	2152	2123
Panel B: $\geq \frac{2}{3}$ dependence ratio group						
Conventional	1438.463*	1780.561**	1292.497	1431.572*	-72.051	-48.907
	(857.411)	(874.800)	(830.347)	(842.174)	(171.547)	(175.430)
Bias-corrected	1775.902**	2093.820**	1552.219*	1705.759**	-20.332	-0.109
	(857.411)	(874.800)	(830.347)	(842.174)	(171.547)	(175.430)
Robust	1775.902*	2093.820**	1552.219	1705.759*	-20.332	-0.109
	(1009.501)	(1009.972)	(987.110)	(983.746)	(199.859)	(203.499)
Controls	No	Yes	No	Yes	No	Yes
Observations	626	512	630	516	630	516

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[1] Column 1-2 presents the result for per-capita self-earned income, Column 3-4 presents that for per-capita self-earned salary, and Column 5-6 the per-capita farming profits (farming income - farming cost). Overall income is the sum the salary income and the farming profits.

[2] The control variables include the base year income level (per capita self-earned income in 2015) and direct transfer level (per capita total direct transfer in 2015), families structure measurement (total number of family members, and the 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). We don't include the change of distance to the paved road from 2015 to 2018 (meters) as in the main regression, is because that the change within each sample is very small.

[3] All these results are obtained by *rdrobust* packages, which implements local polynomial Regression Discontinuity (RD) point estimators with robust bias-corrected confidence intervals and inference procedures developed in Calonico, Cattaneo and Titiunik (2014a), Calonico, Cattaneo and Farrell (2018), Calonico, Cattaneo, Farrell and Titiunik (2019), and Calonico, Cattaneo and Farrell (2020). We use *epanechnikov* kernel function to construct the local-polynomial estimators, and the MSE-optimal bandwidth selector *msecomb1*. Besides, we also apply heteroskedasticity-robust plug-in residuals variance estimator.

Table 12: Sharp RD results for self earned income and decomposition in 2018, no local economy boosted

income. By combining the variation around the arbitrary village borders with administrative household-level data geo-referenced to the exact location around the borders, we manage to identify this institutional effect through spatial regression discontinuity design given the fact that all other factors are continuous around the village borders. First, we provide evidence that the introduction and deep involvement of outsiders into the daily village governance, significantly improves the efficiency of the resources allocation, especially the social security net provision. This result provides micro empirical evidence of the is meaningful for the poverty alleviation worldwide, The We find that: (1) the introduction of village supervisors can improve the efficiency and fairness of the formal insurance, by diminishing the favoritism by the original village cadres. (2) the improvements in formal insurance decrease the spatial misallocation of labor by substantially increasing the ultra-poor's migration to the urban areas, especially young males and females. (3) the migration helps increase the family income and serves as the main drive of the poverty alleviation in rural China from 2016 to 2020. This effect is heterogeneously stronger for the previously unprivileged group. Last but not least, the interplay of the improved institutional system and infrastructure is strong. With better infrastructure which facilitates the daily life of the dependence that left behind, the migration trend is stronger for the healthy young and middle-aged adults.

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Appendix I

7.1 Descriptive Statistics

TO BE CONTINUE

Appendix II

Robustness check for different definition of the vulnerable group

In this section, we show the results for the lowest 2% and lowest 8% as a robustness check for the main results. As shown in Table 13, the main result is robust for different definition of the "vulnerable" group. The result for the lowest 8% group is comparable to the lowest 10% group in the main result, while the lowest 2% group shows significant higher impact relative to the lowest 10% group, showing the impact is more significant for the most vulnerable households.

Robustness check for the time trend result of the lowest 10% households

In Figure 12 and Figure **??**, we run the spatial RD regression for each year separately, hence the selected optimal bandwidths are slightly different for each year, ending up with slightly different samples for each year's regression. In Table **??**, we show the selected optimal bandwidth used for estimation of the regression function estimator and the optimal bandwidth used for estimation of the bias of the regression function estimator, which is suggested by Calonico, Cattaneo and Titiunik (2014a), Calonico, Cattaneo and Farrell (2018, 2020) and Calonico, Cattaneo, Farrell and Titiunik (2019). The optimal bandwiths are shown in Table 14. To make sure the results don't change with the small sample changes, we repeat the exercise for each year with the 2014 optimal bandwidths. The results are shown in Figure **??** and Figure **??**, as we can see, when we keep the sample the same, it delivers similar results.

Indirect evidence of different wage levels of different locations, genders and ages

We calculate the household-level aggregate working months for different locations, genders and ages. In Table **??**, we report our definition of each category, summary statistics and the OLS regression coefficient for the household overall self-earned income and the aggregate working month of each type of family members in 2018. The regression equation is:

$$Income_i = \alpha_m + \beta_m Month_i^m + \varepsilon_i^m \tag{7}$$

in which i represents each household, $Income_i$ is the household overall self-earned income of household i, $Month_i^m$ is total working months of type m family member in household i.

	Overall income		Salary income		Farming profits	
Panel A: lowest 2% in 2018						
Conventional	3205.142	4354.761**	3607.778*	4152.903**	-177.535	-176.847
	(2026.614)	(2168.156)	(2121.083)	(2083.345)	(421.184)	(399.389)
Bias-corrected	4442.570**	5363.114**	4934.629**	5354.423**	-238.696	-203.962
	(2026.614)	(2168.156)	(2121.083)	(2083.345)	(421.184)	(399.389)
Robust	4442.570*	5363.114**	4934.629*	5354.423**	-238.696	-203.962
	(2529.537)	(2589.396)	(2639.769)	(2575.932)	(512.831)	(491.624)
Controls	No	Yes	No	Yes	No	Yes
Observations	586	571	590	575	590	575
Panel B: lowest 8% in 2018						
Conventional	1663.790*	1888.181**	1695.486*	1869.725**	-50.141	-46.181
	(967.910)	(943.792)	(971.821)	(933.365)	(143.790)	(153.037)
Bias-corrected	1949.732**	2169.836**	1991.648**	2150.064**	-82.541	-36.118
	(967.910)	(943.792)	(971.821)	(933.365)	(143.790)	(153.037)
Robust	1949.732*	2169.836*	1991.648*	2150.064*	-82.541	-36.118
	(1157.537)	(1121.669)	(1174.611)	(1120.380)	(167.161)	(183.091)
Controls	No	Yes	No	Yes	No	Yes
Observations	2583	2535	2593	2545	2593	2545

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13: Sharp RD results for self earned income and decomposition in 2018 (robustness check)

We divide all family members by gender, age and working location.

Since the choices of getting a paid job and migration are all endogenous, the OLS regression cannot be interpreted as the causal effect. However, those coefficients can shed some lights on the wage differences between different working location, gender and age. Based on the summary statistics, we can see that the young male is the main working force in the family with on average 5.35 months per family. Besides, conditional on exiting the agricultural sector, 92.4% of the young males choose to migrate with only 7.6% choose to stay in the rural areas. The working ratio and the migration ratio is much lower for the other types, with 13.97% of young female and 25% of the old work in the rural areas. The

Year	2014	2015	2016	2017	2018	2019	2020
	Panel A: Optimal bandwidths for the lowest 10% group						
h_{opt}	828.659	738.118	867.806	856.249	752.053	777.078	743.40
b_{opt}	1477.308	1520.610	1660.647	1544.151	1437.154	1424.623	1402.473
Nobs	3033	3029	3028	3015	3018	3024	3019
	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	714.423	873.816
b_{opt}	1360.877	1389.260	1463.210	1298.105	1321.802	1379.586	1548.810
Nobs	1754	1684	1683	1675	1678	1682	1674

[1] This table reports the optimal bandwidths of estimation and bias correction for Table 5 and 13 Column 2.

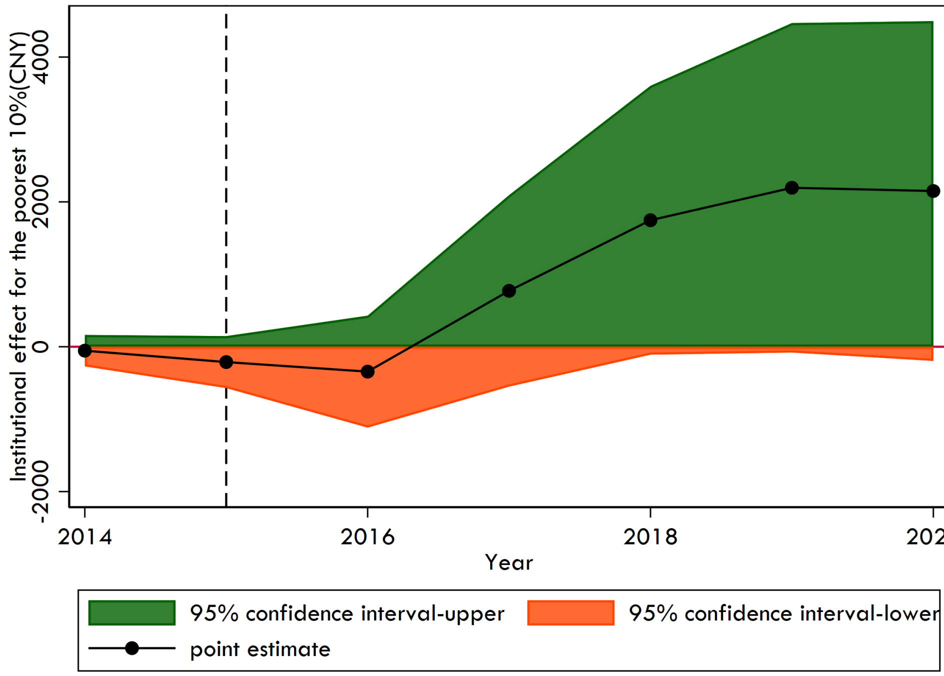
[2] For the base year 2014 and 2015, the control variables include direct transfer level (per capita total direct transfer in 2015), families structure measurement (total number of family members, and the composition of the dependent members including the number of children under 5 years old, children 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.

[3] All these results are obtained by *rdrobust* packages, which implements local polynomial Regression Discontinuity (RD) point estimators with robust bias-corrected confidence intervals and inference procedures developed in Calonico, Cattaneo and Titiunik (2014a), Calonico, Cattaneo and Farrell (2018), Calonico, Cattaneo, Farrell and Titiunik (2019), and Calonico, Cattaneo and Farrell (2020). We use *epanechnikov* kernel function to construct the local-polynomial estimators, and the MSE-optimal bandwidth selector *msecomb1*. Besides, we also apply heteroskedasticity-robust plug-in residuals variance estimator.

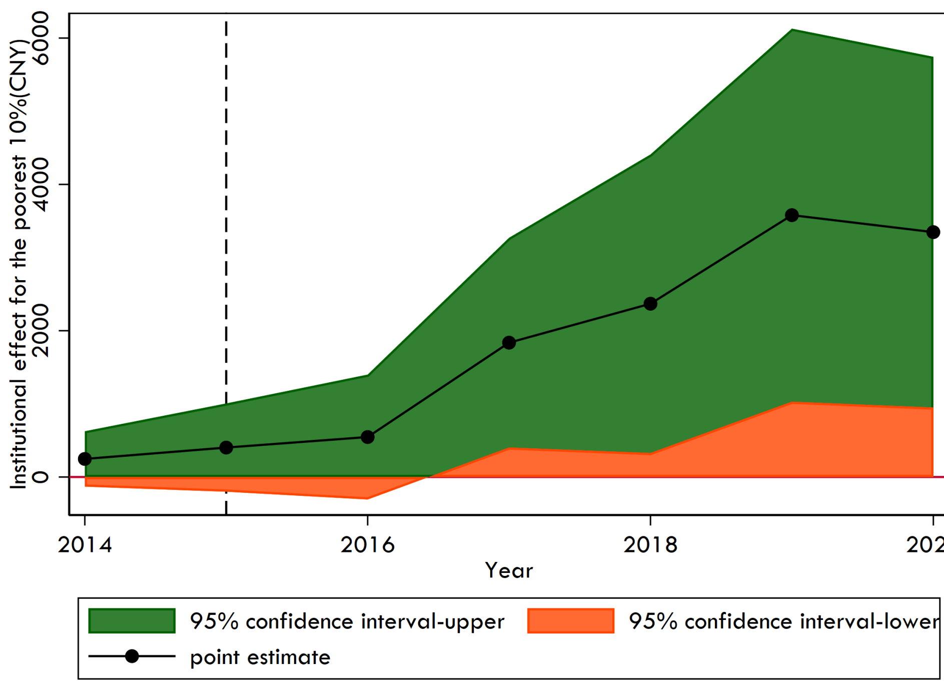
Table 14: Optimal bandwidth selected in Figure 12 nad Figure **??**

overall working months for the young females are 2.87 months, less than 50% of the young male. The old female work the least, with an average of 0.8 months per family, which is only 32.5% of the old male and 27.9% of the young female. In terms of the wage level suggested by the OLS regression coefficients, the urban wage is several folds higher than the rural wages, besides, the young male reveal the highest urban wage level.

Labor supply and migration choices in 2014



(a) for the lowest 10% households, with the same bandwidth



(b) for the $\geq \frac{2}{3}$ dependence ratio households, with the same bandwidth

Figure 14: Placebo test, with the same bandwidth

member type		Mean (std)	OLS coefficient	Adj R^2	Nobs
Male (51-75 year-old)	Town	0.622	-283.0**	0.001	
		(2.208)	(-3.17)		
	Urban	1.84	1042.1***	0.03	11972
		(3.604)	(19.33)		
Female (51-75 year-old)	Town	0.215	525.4***	0.001	11972
		(1.357)	(3.59)		
	Urban	0.586	1442.5***	0.021	11972
		(2.169)	(16.05)		
Male (16-50 year-old)	Town	0.443	347.9***	0.001	11972
		(2.004)	(3.54)		
	Urban	5.362	1920.4***	0.246	11972
		(5.58)	(62.55)		
Female (16-50 year-old)	Town	0.401	964.2***	0.007	11972
		(1.879)	(9.22)		
	Urban	2.47	1682.6***	0.121	11972
		(4.466)	(40.64)		

Table 15: Summary statistics of working months for each type

	Male		Female	
	16-50 year-old	51-75 year-old	16-50 year-old	51-75 year-old
Panel A:τ (employed)				
$ATEs(\hat{\tau})$	0.0403	-0.1685	0.0957	0.1055
90% CI	[-0.0387, 0.1884]	[-0.3042, -0.1073]	[0.0348, 0.2039]	[-0.0653, 0.1955]
Robust 90% CI	[-0.0847, 0.2344]	[-0.3187, -0.0928]	[0.0313 ,0.2074]	[-0.0800, 0.2103]
t-test	1.0839	-3.4375	2.3214	0.8216
t-test P-value	0.2784	0.0006	0.0203	0.4113
Robust t-test	0.7715	-2.9960	2.2292	0.7381
Robust t-test P-value	0.4404	0.0027	0.0258	0.4605
Sample Size	2,109	1,678	1,507	1,294
$\hat{h}$	808.4903	617.7516	567.5615	575.0811
Wald test	1.1749	11.8165	5.3891	0.6751
Wald test p-value	0.2784	0.0006	0.0203	0.4113
Robust Wald test	0.5953	8.9761	4.9694	0.5447
Robust Wald test p-value	0.4404	0.0027	0.0258	0.4605

Table 16: Regression Discontinuity for employment choice in 2014

	Male		Female	
	16-50 year-old	51-75 year-old	16-50 year-old	51-75 year-old
Panel A:τ_1 (work in town)				
$ATEs(\hat{\tau})$	-0.0187	-0.0178	-0.0785	-0.0950
90% CI	[-0.0972 , 0.0284]	[-0.1006, 0.0482]	[-0.1820 , -0.0191]	[-0.1945, -0.0501]
Robust 90% CI	[-0.0992, 0.0303]	[-0.2852 , 0.2328]	[-0.1954 , -0.0057]	[-0.2285,-0.0161]
t-test	-0.9010	-0.5790	-2.0309	-2.5306
t-test P-value	0.3676	0.5626	0.0423	0.0114
Robust t-test	-0.8745	-0.1663	-1.7439	-2.2914
Robust t-test P-value	0.3819	0.8679	0.0812	0.0219
Panel B:τ_2 (work out-of-town)				
$ATEs(\hat{\tau})$	0.1473	-0.0029	0.1896	-0.0293
90% CI	[0.0702 , 0.2827]	[-0.0803 , 0.0627]	[0.0381 , 0.2662]	[-0.1126 , 0.0191]
Robust 90% CI	[0.0673 , 0.2856]	[-0.2581 , 0.2405]	[0.0125 , 0.2918]	[-0.1203 , 0.0268]
t-test	2.7323	-0.2029	2.1946	-1.1680
t-test P-value	0.0063	0.8392	0.0282	0.2428
Robust t-test	2.6590	-0.0582	1.7926	-1.0457
Robust t-test P-value	0.0078	0.9536	0.0730	0.2957
Sample Size	2,109	1,678	1,507	1,294
$\hat{h}$	447.6272	1374.6	650.6572	631.4855
Wald test	7.6905	0.5012	7.2515	8.3149
Wald test p-value	0.0214	0.7783	0.0266	0.0156
Robust Wald test	7.2970	0.0412	5.0610	6.7752
Robust Wald test p-value	0.0260	0.9796	0.0796	0.0338

Table 17: Regression Discontinuity for location choice in 2014