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# Evaluating the Impact of China's Fourth Round of Poverty Alleviation Program

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**Abstract**

This study evaluates the impact of China's fourth round of poverty alleviation program, which targeted 14 contiguous destitute areas containing 680 counties and a population of 240 million. From 2012 to 2019, China allocated a total of 813.6 billion yuan (US$126.1 billion), primarily to economic development programs within these 14 areas. Using county-level data from 2006 to 2019, our difference-in-differences and difference-in-discontinuities estimates suggest that the program increased GDP per capita in the 14 areas by over 45% from 2012 to 2019, with substantial gains observed in both the agricultural and non-agricultural sectors. Our preferred estimates suggest a rate of return to the program ranged from 155.8% to 165.8%. By using data from over 14,500 rural households from 2006 to 2015, we find that the program significantly elevated rural income and reduced rural poverty. While the income growth of extremely poor households was propelled more by agricultural income growth, the income growth of relatively poor households primarily resulted from non-agricultural sources.

**JEL Codes:** I32, R58, I38, O15

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# 1 Introduction

China's national poverty rate decreased from 87.1% to 0.08% between 1981 and 2019, with estimates suggesting that China contributed to nearly 75 percent of the global reduction in extreme poverty during this period (World Bank, 2018). Besides rapid economic growth, poverty alleviation policies may have been a major cause of the dramatic poverty reduction in China (Rozelle et al. 1998, Montalvo & Ravallion 2010). Over the past four decades, China has implemented four major rounds of poverty alleviation programs (World Bank, 2022). Understanding the impact of these programs holds significant implications for other developing countries with high poverty rates.

This study examines the impact of China's fourth round of poverty alleviation program spanning from 2012 to 2020. The program's total investment reached an unprecedented level of 813.6 billion yuan (US$126.1 billion), equivalent to about 1.67% of China's 2011 GDP and roughly matching the GDP of Bangladesh in the same year. The program targeted 14 contiguous destitute areas containing 680 counties and a population of 0.24 billion, aiming to address common causes of regional poverty. The program channeled poverty alleviation funds primarily toward economic development and revenue-generating activities, instead of direct consumption, in order to permanently lift the poor out of poverty.

Abundant data are accessible during the fourth round of poverty alleviation, allowing us to examine the following crucial questions that were difficult to answer previously: To what extent did the geographic targeting program enhance economic growth in the destitute areas? Did the substantial program investment yield a sufficiently high return to cover the costs? Did the program effectively decrease rural poverty? Was poverty alleviation primarily driven by agricultural or non-agricultural income growth? What were the varying impacts of the program on extremely poor, relatively poor, and non-poor rural residents?

We evaluate the impact of the program on economic growth based on county-level data from 2006 to 2019. We first estimate a difference-in-differences (DID) model that compares counties within and outside the 14 contiguous destitute areas (CDAs) both before and after 2012 (i.e., the beginning of the program). We discover that despite the greater poverty of counties within the CDAs compared to those outside, their growth trends were parallel during 2006–2011. Our DID estimates indicate that the program raised the GDP per capita of the average county within the CDAs by 0.39 log points by

2019. The effect on agricultural GDP per capita amounted to 0.32 log points, whereas the effect on non-agricultural GDP per capita stood at 0.43 log points. These estimates remain robust when subjected to alternative income measures, sub-samples, spillover effects, controls for other preexisting and contemporary policies, and permutation tests. Additionally, we demonstrate that the policy led to a significant increase in budget expenditure, fixed asset investment, agricultural productivity, enterprise growth, number of middle schools, and savings account balances within the CDA counties.

To further address the concern regarding the comparability of counties inside and outside the CDAs, we also present the difference-in-discontinuities (DID-RD) estimates. The DID-RD design rests on a much weaker assumption that in the absence of the policy, counties located just inside and just outside the CDA borders should exhibit the same growth trends, conditional on county-fixed effects. We have provided strong evidence to support this assumption. Our preferred estimate using the DID-RD approach, which explicitly considers spillover effects, indicates that the program increased GDP per capita by 0.38 log points by 2019.

Our DID and DID-RD estimates indicate that the rate of return on the program's total investment ranged from 155.8% to 165.8%. Put simply, each dollar invested generated a benefit ranging from 1.56 to 1.66 dollars. The estimated rate of return is significantly higher than what was estimated in earlier studies that focused on China's initial rounds of poverty alleviation programs. Specifically, Park et al. (2002) and Meng (2013) estimated that the rate of return for the first and second rounds of poverty alleviation programs was 15.5% and 42.4%, respectively. This substantial difference can be explained by the fact that due to the lack of early data, Park et al. (2002) and Meng (2013) estimated the rate of return based only on the impact on rural income. By focusing on the impact on agricultural GDP, our estimates would similarly suggest a rate of return ranged from 34.4% to 49.6%.

Based on a nationally representative random sample of more than 14.5 thousand rural households from 2006 to 2015, we find that the program significantly increased rural household income by 0.23 log points during 2012–2015, with income growth being propelled by both farm and off-farm income expansion. We also find that the program substantially shifted rural households from extreme poverty (below US$1.90 per capita per day) to relative poverty (above US$1.90 but below US$5.50 per capita per day), but had little effect on the proportion of relatively rich households (above US$5.50 per capita per day). Additionally, we have found that the income growth of extremely poor households was driven more by agricultural income growth, while the income growth of relatively poor households was mainly driven by off-farm income growth.

This study contributes to the literature on evaluating the impact of China's poverty alleviation programs. China has implemented four major rounds of poverty alleviation programs since 1986, and several important studies have examined the impact of these programs. For example, Park et al. (2002) examined the first round of poverty alleviation program, which targeted 328 poor counties from 1986 to 1993, and found that the program significantly increased rural income; Meng (2013) examined the second round of poverty alleviation program, which targeted 592 poor counties from 1994 to 2000, and similarly found that the program significantly increased rural income; Park & Wang (2010) examined the third round of poverty alleviation, which targeted poor villages from 2001 to 2011, and found that the policy increased the income of relatively rich but not poor rural households. Despite the unprecedented scale of the fourth round of poverty alleviation, only few studies have examined its impact. Zhang et al. (2023) examined the impact of the Anti-Poverty Relocation Program, which accounted for about 5% of the investments during the fourth round of poverty alleviation, based on household data from Xin County of Henan Province during 2014–2018 and found that the program increased the participant's income by about 10%. Freije et al. (2022) exploited the provincial-level data on poverty rate and poverty alleviation funds during 2010–2017 and found that a 10 percent increase in antipoverty funds brings a reduction of poverty rate of 0.16 to 0.77 percent range. To the best of our knowledge, our study is the first to examine the overall impact of the fourth round of poverty alleviation based on county-level data and nationally representative household data.

This study also contributes to understanding the impact of geographic poverty targeting, which attempts to reduce poverty through promoting economic development of poor areas via public investment (e.g., Ravallion & Datt 1996, Ravallion 1993, Park et al. 2002). Governments often exploit geographic factors in the design of targeting schemes because poverty may be more concentrated in some areas of a country (Elbers et al. 2007) and spatial factors are key determinants of poverty (Ravallion & Jalan 1999). China's fourth round of poverty alleviation targeted CDAs that each contains dozens of counties, while the initial three rounds of poverty alleviation targeted individual poverty counties or villages. Targeting a larger geographic area could be justified if the marginal product of individual capital increases with common geographic capital (Ravallion & Jalan 1999) or if the saved administrative costs compensate for the "roughness" of targeting (Lipton & van der Gaag 1993).[1] This study estimates that

[1]While existing simulation studies generally suggest a large poverty reduction from targeting smaller administrative units (e.g., Elbers et al. 2007, Baker & Grosh 1994), Hillebrecht et al. (2023) showed that higher-level targeting is as efficient as lower-level targeting when taking into account the lagged benefits.

the rate of return from targeting destitute areas is not lower than that from targeting poor counties. Our findings demonstrate that this is attributable to the substantial enhancement of non-agricultural growth achieved by targeting destitute areas. The high return could also be explained by the observed scale effect of the geographic targeting: we find that the impact of the program increases with the number of counties, total population, and total GDP contained in each targeted destitute area.

In addition, this study is closely connected to the literature on the association between agricultural growth and poverty alleviation. Abundant evidence has shown that agricultural growth serves as the primary driver of poverty reduction in developing countries (e.g., Fan et al. 2000, Crandall & Weber 2004, Tiffin & Irz 2006, Moyo et al. 2007, Dercon et al. 2009, Loayza & Raddatz 2010), because poverty in developing countries is mainly rural poverty and many of the poor in rural areas depend on agriculture. However, an important question is that while agricultural development could be efficient in reducing extreme poverty, is it efficient in reducing relative poverty? The answer to this question is ambiguous because farm sizes in developing countries are generally small (Vollrath 2007, Gollin et al. 2014) and labor reallocation to non-agricultural sectors is usually necessary for sustained income growth (Duarte & Restuccia 2010, Herrendorf et al. 2014, Christiaensen & Todo 2014). The findings of this study demonstrate that while the reduction of extreme poverty relies on both agricultural and non-agricultural growth, the reduction of relative poverty primarily hinges on non-agricultural growth.

Finally, while China is fairly unique in its ability to fund such a massive poverty alleviation program, the findings from China's fourth round of poverty alleviation have at least two important implications actionable for other developing countries. Firstly, targeting a larger geographic poverty region may be more efficient than targeting subareas within the poverty region. Our estimates suggest a scale effect of geographic targeting: the return to the program increases with the economic and population size of the targeted contiguous destitute areas. Additionally, we find a much higher rate of return for the fourth round of poverty alleviation, which targeted 14 contiguous destitute areas containing 680 counties, as compared to previous rounds that mainly targeted individual poverty counties or villages. Given that geographic poverty caused by common spatial factors is prevalent in many other developing countries (Elbers et al. 2007, Ravallion & Jalan 1999), it is possible for these countries to improve the efficiency of poverty alleviation programs by targeting larger geographic poverty regions. Secondly, the focus of the poverty alleviation program should be adjusted according to the change in the degree of poverty. China's fourth round of poverty

alleviation contains various types of programs that focus differently on improving the growth of agricultural and non-agricultural sectors. We find that, on average, these programs led to more growth in non-agricultural sectors than in agricultural sectors. Further examination of rural households from different income groups reveals that while the income growth of extremely poor households was driven more by agricultural income growth, the income growth of relatively poor households was mainly driven by off-farm income growth. These findings suggest that developing countries should shift the focus of their poverty alleviation programs from agricultural to non-agricultural sectors after extreme poverty has been eliminated.

The remainder of the study proceeds as follows. Section 2 presents the background of the study, Section 3 describes the data used, Section 4 presents the empirical strategy, Section 5 presents county-level estimates, Section 6 presents household-level estimates, and Section 7 is concluding remarks.

# 2 Background

## 2.1 Poverty alleviation in China

China has witnessed an unprecedented historical trend of poverty reduction in terms of both speed and scale. As presented in Appendix Figure A.1, the rate of extreme poverty (below US$1.90 per day) in China decreased from 87.1% to 0.08% from 1981 to 2019. The poverty reduction is similarly astonishing when measuring poverty by higher poverty lines or focusing on the rural population. According to the World Bank (2018), China has accounted for almost 75 percent of the global reduction in the number of people living in extreme poverty during this period. While China has almost eradicated extreme poverty by 2019, 24.0% of its rural population (or 140 million) were still under relative poverty according to the poverty line of US$ 5.5 per day.

## 2.2 China's first three rounds of poverty alleviation programs

China's approach to poverty reduction was based on two pillars (World Bank, 2022). The first was rapid economic growth, which raised average incomes and provided new economic opportunities for the poor. The second was government policies to alleviate poverty. Figure 1 presents the four major rounds of poverty alleviation programs in China: the first round from 1986 to 1993, the second round from 1994 to 2000, the third round from 2001 to 2011, and the fourth round from 2012 to 2020.

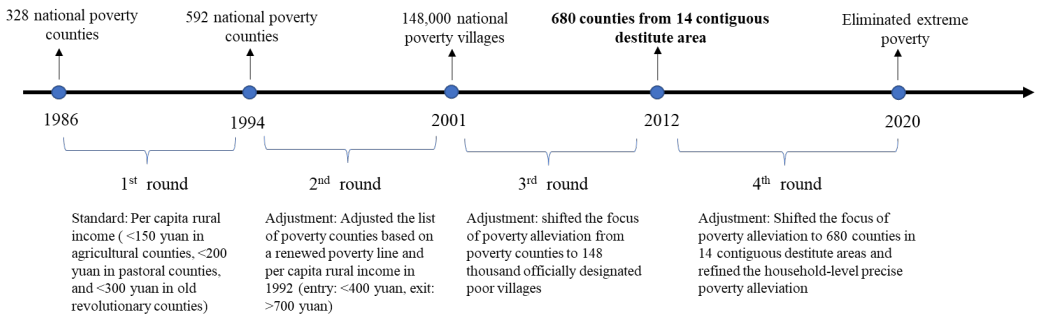


**Figure 1:** Four major rounds of poverty alleviation programs in China

**The first round of poverty alleviation.** In 1986, the Chinese government established the inter-ministerial Leading Group for Economic Development in Poor Areas with the aim of overseeing an ambitious program to eliminate rural poverty. The Leading Group adopted a mixed set of poverty lines to identify 328 national poor counties eligible for the new poverty alleviation funds.[2] Poverty alleviation resources were primarily channeled toward economic development and revenue-generating activities instead of direct consumption. Funds were allocated via three targeted poverty alleviation programs: a subsidized loan program (tiexi daikuan), a public works program called Food-for-Work (yigong daizhen), and a budgetary grant program (fazhan zijin). Please refer to Park et al. (2002) for more details.

**The second round of poverty alleviation** (i.e., the 8-7 Plan). In 1993, partly in response to the criticism that the targeting of poor counties was heavily compromised by politics (Park et al. 2002), the Leading Group adopted a renewed poverty line and designated 592 counties as national poor counties.[3] The goal of the 8-7 Plan had been to permanently lift the poor out of poverty through promoting economic development and income growth. Similar to the first round of poverty alleviation, the 8-7 Plan allocated poverty alleviation funds to the designated poverty counties via the three targeted poverty alleviation programs. Please refer to Meng (2013) for more details.

**The third round of poverty alleviation.** In 2001, China shifted its poverty alleviation focus from poor counties to poor villages. About 148 thousand villages,

[2]The Leading Group designated 258 counties as national poor counties in 1986, of which 83 had rural incomes per capita below 150 yuan, 82 between 150 and 200 yuan, and 93 between 200 and 300 yuan. By 1988, the number of poor counties had reached 328. Three counties in Hainan Province were added to the list of national poor counties in 1989 when Hainan was separated from Guangdong Province (Park et al. 2002).

[3]In principle, poor counties are those with per capita rural net income below 400 yuan in 1992. However, faced with pressure from previously designated counties, the central government decided to raise the poverty line to 700 for counties labeled as "poor" before 1993.

accounting for 21% of all villages in China, were officially designated as poor villages.[4] The designation entitles these villages to targeted investment funds financed by the same three programs adopted since 1986 (i.e., the subsidized loan program, the Food-for-Work program, and the budgetary grant program). The government committed to completing investments in public projects chosen by each poor village by the decade's end. Please refer to Park & Wang (2010) for more details.

## 2.3 China's fourth round of poverty alleviation

China's poverty alleviation strategy underwent significant adjustments in 2012 following the issuance of the "Outline of Poverty Alleviation and Development in Rural China (2011-2020)," hereafter referred to as the 2011 Outline. The focus of the poverty alleviation attention shifted to 14 contiguous destitute areas (CDAs) that consist of 680 counties.[5] The 14 CDAs are identified based on three poverty indexes during 2008–2010: per capita GDP, per capita general budget revenue, and per capita net income of households. If the three poverty indexes of any county are lower than the respective average of China's western provinces, then the county is deemed a destitute county. CDAs are then determined according to whether a destitute county is contiguous to at least one other destitute county. The left panel of Figure 2 presents the distribution of the 14 CDAs, and the right panel shows that counties with the lowest GDP per capita are generally in the 14 CDAs. Appendix Table A.1 presents the name, counties contained, population size, and poverty rate of each CDA.

[4] Concurrently, several other poverty alleviation initiatives were in effect during 2001–2011, benefiting all residents, not solely those from poor villages. For example, China adopted the rural cooperative healthcare system in 2003, ended the agricultural tax in 2006, and implemented the rural minimum living security system in 2007.

[5] At the same time, there were 152 national poverty counties outside the CDAs, which are designated in the second round of poverty alleviation (the other 440 national poverty counties from the second round are in the CDAs). Although the 152 poverty counties outside the CDAs remain entitled to receive the same support from the central government as before, the 2011 Outline explicitly stipulated that new poverty alleviation funds should be primarily allocated to the 14 CDAs. Appendix Figure A.2 presents the 152 poverty counties outside the CDAs. These counties will be excluded from our main analysis.

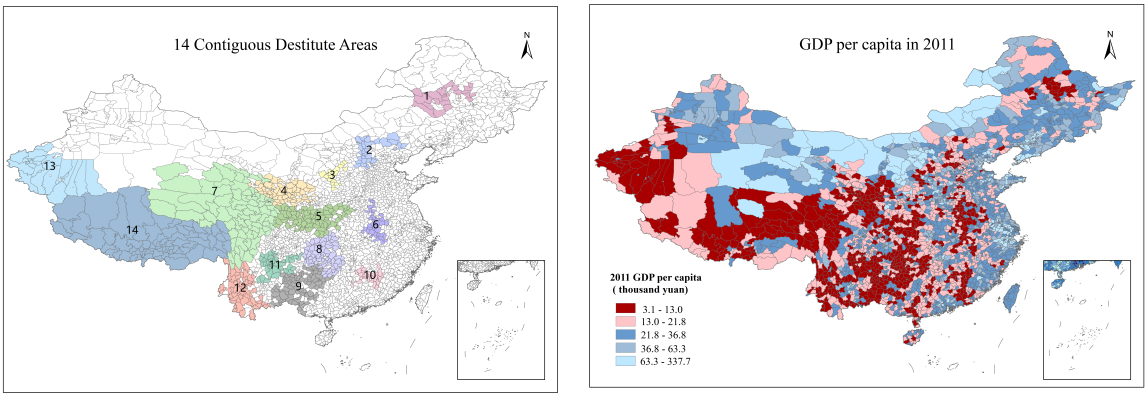


**Figure 2:** 14 contiguous destitute areas and county-level GDP per capita

*Notes*: The left panel presents the 14 contiguous destitute areas, and the right panel presents the distribution of GDP per capita across counties in 2011.

The 14 CDAs have become the main battlegrounds for tackling poverty in China since 2012 (Li et al. 2016, Liu et al. 2018). The decision to concentrate poverty alleviation efforts on the CDAs stems from the stark reality that these areas bear a high concentration of poverty due to their specific geographic and socioeconomic attributes. These CDAs are generally characterized by underdeveloped local infrastructure, limited business prospects, and challenging geographical conditions. In 2011, while the 14 CDAs accounted for only 17.5% of China's total population, they accounted for more than 50% of China's extremely poor population (World Bank 2022). Investing in regional capital to address common causes of regional poverty (i.e., lack of infrastructure, high transport costs, and fragile ecology) may generate a high rate of return (Park et al. 2002).

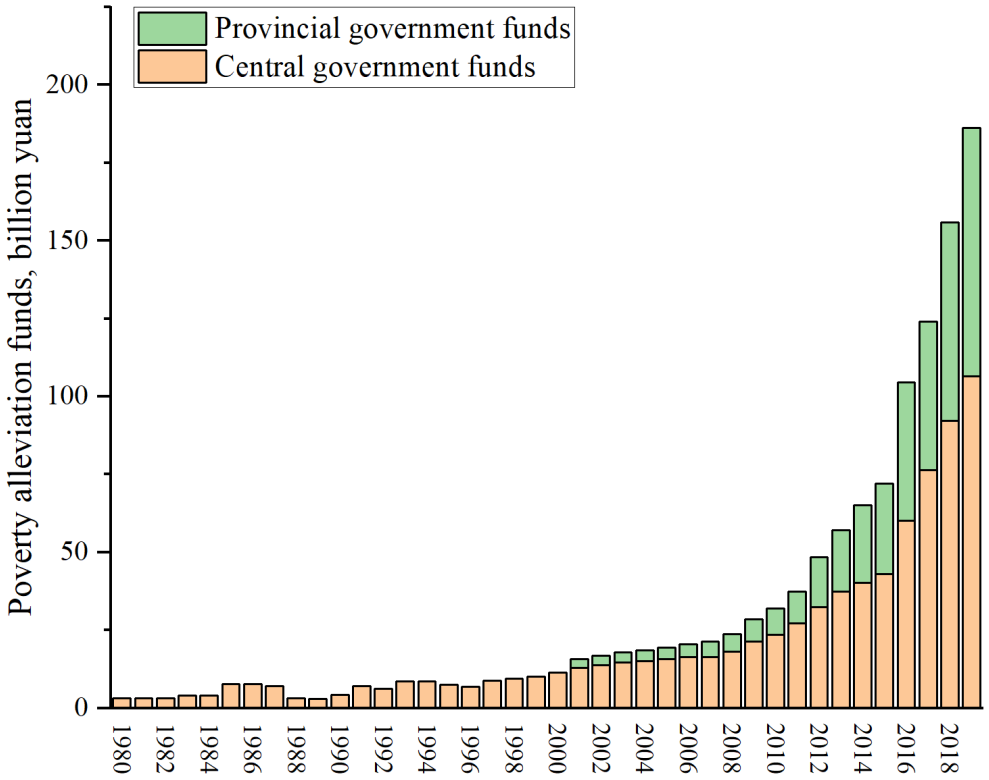


**Figure 3:** Regional poverty in China in 2011

*Note*: The data are derived from World Bank (2022). All values are measured in 2011 constant billion yuan. The data for provincial funds before are not available.

Public investments in poverty alleviation dramatically increased during the fourth round of poverty alleviation. Illustrated in Figure 3, poverty alleviation funds from central and provincial governments reached 186.2 billion yuan (or US$27.0 billion) in 2019, representing 0.35% of GDP in China in that year. In contrast, the funds had not surpassed 50 billion yuan in any individual year prior to 2012. Remarkably, the investment in each year following 2016 exceeded the total investment made during the entire two decades spanning from 1980 to 1999. As stipulated in the 2011 Outline, additional governmental specialized poverty alleviation funds after 2012 should be primarily invested in programs in the 14 CDAs.[6]

[6]Figure 3 only presents the governmental specialized anti-poverty funds, which are fiscal budgets specially earmarked for poverty alleviation in poverty-stricken areas (i.e., the 14 CDAs since 2012). Poverty alleviation funds could also come from the Industry Funds and Social Poverty Funds for poverty alleviation. The Industry Funds refer to all funds from relevant government functional departments, which are not earmarked for poor areas but play a role in poverty alleviation via their work in poor areas. The Social Poverty Funds refer to poverty alleviation efforts exerted by entities other than the governments, including social organizations, enterprises, and individuals. Due to the

These governmental specialized anti-poverty funds were allocated to programs of poverty alleviation and development concentrating on creating economic opportunities for poverty reduction. These generally consist of investments in infrastructure of all types (e.g., roads, irrigation systems, housing), training programs, and financial support for the creation (or growth) of firms and jobs. No handouts or transfers are included within this type of programs, with the exception of living expenses and other emoluments for students and trainees of skills and job training programs. The funds at the highest level are composed of several sub-funds. The three major sub-funds are the Development fund (70–80% of the total), the Minority Development fund (10% of the total), and the Food-for-Work fund (5% of the total). The Development fund is often heavily invested in programs to construct local infrastructures in designated poor areas. The Minority Development fund helps improve the living conditions of impoverished minorities in rural regions of China. The Food-for-Work fund is used to construct local infrastructures by employing local rural residents and thereby promoting income growth.

Each of these sub-funds was then allocated to various poverty alleviation programs according to the local poverty alleviation needs. The major flagship programs that received a significant amount of funding are Industrial Poverty Alleviation, Whole Village Advancement, Relocation, and Food-for-Work. The Industrial Poverty Alleviation programs, which are perhaps the most important component of China's poverty alleviation, focused on spurring economic development by providing financial resources, tax preferences, access to land, and other benefits to incentivize the creation or expansion of enterprises in selected industries. The Whole Village Advancement programs focused on constructing infrastructures such as roads, drinking water, and irrigation for poor villages. The Relocation programs focused on reallocating households to new living areas with better living conditions from areas where basic living conditions and possibilities of production are too harsh due to remoteness, fragile ecology, and lack of infrastructure and services for human development. The Food-for-Work programs were designed for the construction of small-scale rural infrastructure and providing short-term employment opportunities to unskilled workers. The share of funds allocated to each of these flagship programs varied across poverty areas, depending primarily on the local causes of poverty. Please refer to Freije et al. (2022) for more details on these programs.

Note that since 2013, China has refined and extended its household-level precise

dearth of data, it is difficult to gauge the size of poverty alleviation funds from the latter two sources (Freije et al. 2022).

poverty alleviation, a crucial component of the fourth round of poverty alleviation. Specifically, the local bureaus create archives and issue cards for each poor household to record its family status. Assistance projects are then targeted at these poor households. Although the projects applied to all poor households nationwide, they were most intensively implemented in the 14 CDAs, which had much higher poverty incidences than other areas (World Bank 2022). However, to the extent that the precise poverty alleviation projects are implemented in areas outside the CDAs, this study tends to underestimate the impact of the fourth round of poverty alleviation. Due to the dearth of data, it is harder to gauge how much of the governmental specialized anti-poverty funds were allocated to precise poverty alleviation outside the CDAs.

To sum up, the fourth round of poverty alleviation primarily focused on the 14 CDAs. Massive governmental specialized anti-poverty funds were allocated to these areas to alleviate poverty via programs that promote local economic development. However, we do not know the amount of funds from other sources that had been invested in poverty alleviation in the CDAs. In addition, due to the extension of the precise poverty alleviation projects, not all of the governmental specialized anti-poverty funds were invested in the CDAs. To address the uncertainty about the total poverty alleviation funds invested in the 14 CDAs, we assume two "extreme" scenarios of the investment when examining the possible range of the rate of return.

# 3 Data and Summary Statistics

## 3.1 County-level data

Our county-level analysis depends on data from 1764 counties from 2006 to 2019. The data are derived from the China County Statistical Yearbook. Excluding data before 2006 helps mitigate the confounding effect of the third round of poverty alleviation, which was most intensively implemented from 2001 to 2005 (World Bank, 2022). Excluding data after 2019 aims to avoid the confounding effect of COVID-19. Among the 1764 counties, 674 are from the 14 CDAs,[7] and the remaining 1090 are outside the 14 CDAs (used as the control group). The control group has excluded county-level municipal districts as they are much richer and less comparable to the poor CDA counties.[8] The control group has also excluded the 152 national poverty counties designated in

[7]There are a total of 680 counties in the CDAs, and the remaining 6 are omitted due to lack of economic data.

[8]In 2019, China had a total of 2,843 county-level administrative divisions, among which 977 are county-level municipal districts and the remaining 1866 are county-level counties or cities.

the second round of poverty alleviation (see Footnote 5 for details). Appendix Table A.2 summarizes key variables respectively for sample counties inside and outside the CDAs.

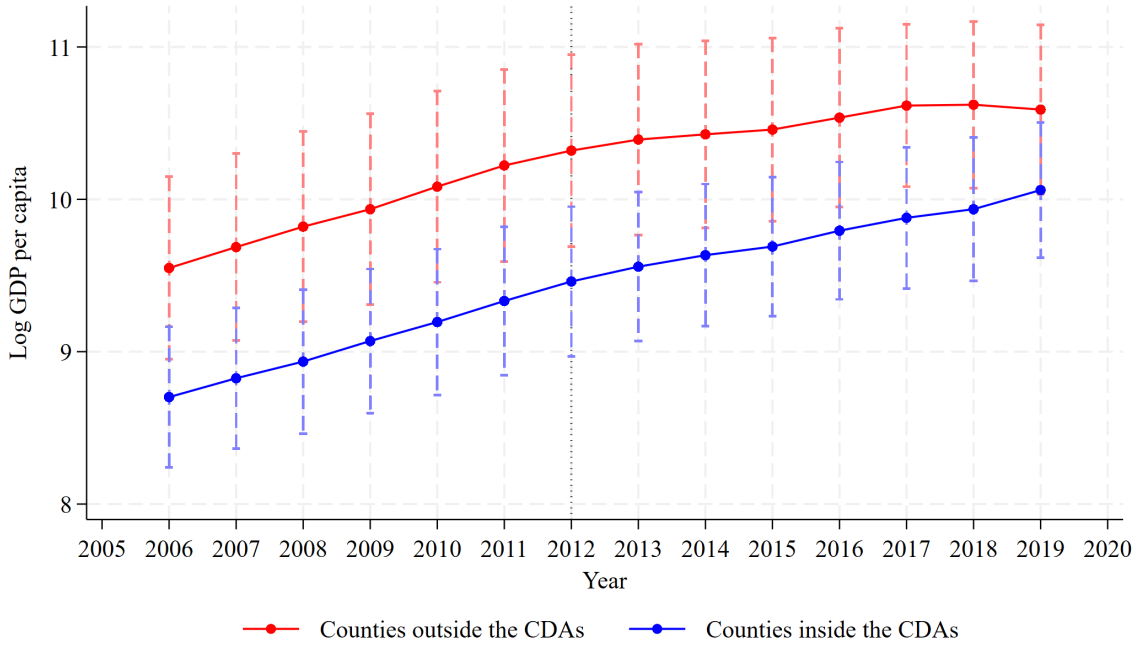


**Figure 4:** Trends of log GDP per capita for counties inside and outside the CDAs

*Notes*: This figure presents the average log GDP per capita in each year and the accompanying standard deviation for 1090 counties outside the CDAs (red) and 674 counties inside the CDAs (blue).

Figure 4 presents the average log GDP per capita in each year for the 1090 counties outside the CDAs (the red line) and the 674 counties inside the CDAs (the blue line), respectively. The figure shows that although counties inside the CDAs on average had much lower GDP per capita than counties outside the CDAs, these two groups of counties were on the same growth trend before 2012. More specifically, before the policy, these two groups of counties did not exhibit a convergence or divergence trend, indicating that the relative poverty of the CDA areas is determined by time-invariant factors. In other words, the poor CDA counties were perhaps trapped before 2012, and an exogenous boost seems to be necessary. This finding is important for identifying the causal effect of the policy by adopting the DID strategy, which relies on the parallel trends assumption.

The figure also shows that counties inside the CDAs grew faster than counties outside the CDAs after 2012, suggesting a positive effect of the policy on the economic

growth of the CDA counties. Note that after 2012, the non-CDA counties were on a declining growth trend, reflecting the decreasing growth rate of China over the recent decade. If not for the poverty alleviation policy, we would expect that the CDA counties would also follow this declining growth trend. In contrast, we find that the CDA counties grew much faster than the non-CDA counties after 2012, leading to an obvious convergence between these two groups of counties. To exclude the possibility that these relative changes in growth trends were caused by other factors instead of the poverty alleviation policy, we will adopt DID and DID-RD strategies to identify the causal effect of the policy.

## 3.2 Household-level data

Our household-level analysis relies on data from the National Fixed Point Survey (NFP), a panel survey conducted by the Research Center of Rural Economy in China. NFP villages were selected for representativeness based on various factors such as region, income, cropping pattern, population, and non-farm activities. Within each village, a random sample of households was selected, typically ranging from 50 to 100 households, depending on village size.[9] The NFP data contains more than 18 thousand households in each year from roughly 350 villages. Appendix Figure A.3 presents the counties where the sample villages are located. The NFP data, widely employed in the literature (e.g., Kinnan et al. 2018, Chari et al. 2021), has been demonstrated to be of high quality (Benjamin et al. 2005). We use data from annual waves of the survey from 2006 to 2015 for 292 villages. Data after 2015 are not available for us. Our data excludes 56 NFP villages located in municipal districts and households that were present for less than 5 years during 2006–2015. Our data includes an average of 14.6 thousand households in each year, among which 23.4% were from the 14 CDAs. Appendix Table A.3 summarizes key variables respectively for sample households inside and outside the CDAs.

[9]If a sample household permanently relocated, it was replaced by a randomly selected new household within the same village, receiving a new household ID. The dataset constitutes an unbalanced panel, with 99.6% of the sample households having data for at least two years, and 91.2% having data for at least 5 years.

# 4 Empirical Strategy

## 4.1 County-level DID analyses

We estimate the impact of the poverty alleviation policy by comparing counties inside and outside the 14 CDAs before and after the policy based on the following flexible DID model:

$$lny_{it} = \eta_i + \gamma_t + \sum_{k=2006, k\neq 2011}^{2019} \beta_k Treat_i \times D(t=k) + X_{it}\theta + \mu_{it} \quad (1)$$

where $\ln y_{it}$ is one of the outcome variables in county $i$ and year $t$. The key outcome variables examined are log GDP per capita, log agricultural GDP per capita, and log non-agricultural GDP per capita. The dummy variable $Treat_i$ equals 1 for all counties inside the CDAs and equals 0 otherwise. The dummy variable $D$ takes a value of 1 when $t = k$, and 0 otherwise (the base year is set at 2011).

The model also controls for the county-fixed effects ($\eta_i$), year-fixed effects ($\gamma_t$), and a vector of county-level control variables ($X_{it}$). The control variables in the main analysis are two exogenous climatic factors: annual mean temperature and annual total precipitation. In robustness checks, we also control for county-level average elevation, distance to the provincial capital, distance to the nearest port, and an indicator of ethnicity. To account for their potential time-varying effects, we follow the literature (e.g., Nunn & Qian 2011) to interact the four time-invariant geographic control variables with a full set of year dummies. These control variables are important growth determinants that are unlikely to be affected by the policy. Finally, the error term ($\varepsilon_{it}$) is clustered at the county level to address the potential bias from serial correlation.[10]

The flexible DID model captures the causal effect of the policy based on the assumption that the growth trends of counties inside and outside the 14 areas would be the same without the policy (i.e., the parallel trends assumption). Evidence supporting this assumption can be provided by examining the coefficients $\beta_k$ for $k < 2012$. If the hypothesis is satisfied, we should observe that the coefficients $\beta_k$ are not significantly different from zero for years before the policy. As presented in Figure 5, the estimates of $\beta_k$ are all close to zero and statistically insignificant for years $k < 2012$. Therefore, the coefficients $\beta_k$ for years $k \geq 2012$ capture the accumulated causal effects of the policy over time. For example, the estimate of $\beta_{2019}$ captures the accumulated effect

[10] We also conducted an additional analysis by clustering the error term at the CDA level using the bootstrap approach proposed by Conley (1999) and observed smaller standard errors.

of the policy on log GDP per capita from 2012 to 2019, relative to the log GDP per capita in 2011.

When conducting robustness tests and examining the mechanisms of the impact, we will also estimate the following simplified DID model that captures the average effect of the policy:

$$lny_{it} = \eta_i + \gamma_t + \beta_1 Treat_i \times Post_t + X_{it}\theta + \varepsilon_{it} \tag{2}$$

where the dummy variable $Post_t$ equals 1 for $t \geq 2012$ and equals 0 otherwise, and all other variables are the same as defined before. Note that we do not include the dummy control variables of $Treat_i$ and $Post_t$ in the model, as the county-fixed effects ($\eta_i$) and year-fixed effects ($\gamma_t$) are sufficient to fully account for their effects. The coefficient $\beta_1$ captures the average causal effect of the policy from 2012 to 2019. Although we are more interested in the accumulated effects estimated from the flexible model 1, the average effect estimated from model 2 can help us simplify our analysis.

## 4.2 County-level DID-RD analyses

To further address concerns regarding the comparability of counties inside and outside the CDAs, we also adopt a spatial difference-in-discontinuities (DID-RD) design, which compares counties situated around the border of CDAs before and after the policy implementation. The DID-RD design has been widely used to evaluate the impact of regional policies (i.e., Briant et al. 2015, Shenoy 2018, Lu et al. 2019). The main additional assumption of the DID-RD design, on top of continuity, is the parallel trends assumption from DID. However, this assumption is much weaker when focusing on borders than when comparing units further apart (Shenoy 2018). The DID-RD design also addresses potential biases from compound treatments, which can affect the cross-sectional regression discontinuity design (Butts 2021, Keele & Titiunik 2015). Specifically, compared to the DID design, the advantage of the DID-RD design is that it only requires counties around the borders to satisfy the parallel trends assumption. This is more likely to be valid because closer counties are more similar in geographic and climatic conditions. Additionally, compared to the cross-sectional regression discontinuity design, the DID-RD design can use the first difference to remove potential preexisting discontinuities around the border that are time-invariant.

We follow the literature (e.g., Lee & Lemieux 2010, Shenoy 2018) to estimate the

following flexible DID-RD model:

$$lny_{it} = \eta_i + \gamma_t + \sum_{k=2006,k\neq 2011}^{2019} \beta_k^1 Treat_i \times D(t=k) + \sum_{k=2006,k\neq 2011}^{2019} \beta_k^2 Dist_i \times D(t=k) + X_{it}\theta + \varepsilon_{it}, \tag{3}$$

where $Dist_i$ is the running variable used to capture the impact of the distance of each county's centroid to the CDA border. Specifically, we set $Dist_i = \omega_1 Distance_i + \omega_2 Distance_i \times Treat_i$, in which $Distance_i$ is the distance of county $i$'s centroid to the nearest border of the CDA, and $Treat_i$ again signifies the dummy variable for being inside the CDA. The specification of $Dist_i$ allows the effect of the distance to be different inside $(\omega_1 + \omega_2)$ and outside $(\omega_1)$ the CDA. We interact the running variable with a full set of year dummies $(D)$ to allow the impact of the distance changes over time. All other variables are the same as defined in the flexible DID model. We also cluster the error term of the model at the county level.

Our main analysis set the bandwidth at 100km, and we will show that the estimates are robust to alternative bandwidths. We have also followed Dell (2010) to adopt the alternative running variable of the third-order polynomial in the latitude and longitude of each county's centroid and found a comparable result (presented in Appendix Figure A.7). In robustness tests, we will also estimate the following simplified DID-RD model:

$$lny_{it} = \eta_i + \gamma_t + \beta_1 Treat_i \times Post_t + \sum_{k=2006,k\neq 2011}^{2019} \beta_k^2 Dist_i \times D(t=k) + X_{it}\theta + \varepsilon_{it}, \tag{4}$$

where the dummy variable $Post_t$ equals 1 for $t \geq 2012$ and equals 0 otherwise, and all other variables are the same as defined in the flexible DID-RD model.

Based on the assumption that counties around the border have parallel growth trends if without the policy, the coefficient $\beta_k^1$ (for $k \geq 2012$ ) of model 3 captures the accumulated causal effect of the policy around the border. The identification assumption is supported by our finding that all estimates of $\beta_k^1$ for $k < 2012$ are close to zero and statistically insignificant (Figure 6). In addition, we also follow the literature (e.g., Keele & Titiunik 2015, Butts 2021) to test the parallel trends assumption by plotting the first difference of the dependent variable around the border. A continuous first difference at the border before the policy suggests that the discontinuity of the first difference after the policy can be attributed to the policy. As presented in Appendix Figure A.8, the first difference of log GDP per capita is continuous at the border before the policy (Panels A and B) but increasingly discontinuous after the policy (Panels C and D).

## 4.3 Household-level analyses

Similar to the county-level analysis, we depend on the following flexible DID model to estimate the impact of the policy on household income and poverty incidence:

$$y_{ivt} = \eta_i + \gamma_t + \sum_{k=2006, k\neq 2011}^{2015} \beta_k Treat_v \times D(t=k) + Z_{ivt}\theta + \mu_{ivt}, \tag{5}$$

where $y_{ivt}$ is the outcome of interest for household $i$ from village $v$ in year $t$. The dummy variable $Treat_v$ equals 1 for all sample villages inside the CDAs and equals 0 otherwise. The dummy variable $D$ takes a value of 1 when $t = k$, and 0 otherwise. The model controls for the household-fixed effects ($\eta_i$), year-fixed effects ($\gamma_t$), and a vector of control variables ($Z_{ivt}$).[11] The key outcome variables are household total income, agricultural income, off-farm income, and poverty indicators. As some households have zero off-farm income or farm income, we follow the suggestion of the recent literature (Cohn et al. 2022, Chen & Roth 2023) to estimate the percentage effect by Poisson quasi-maximum likelihood regression. Specifically, we estimate model (5) by Poisson regression and then calculate the percentage effect according to the log point transformation of $exp(\beta_k) - 1$.

To interpret the estimate of $\beta_k$ (for $k \geq 2012$ ) from model 5 as the causal effect of the policy, we need to assume that households inside and outside the CDAs would have the same growth trends if the policy were absent. This hypothesis is expected to be satisfied because we have shown that county-level evidence supports the parallel trends assumption and because the NFP villages and households are randomly selected for national representativeness. As expected, Figure 7 shows that the estimates of $\beta_k$ from model 5 are close to zero and statistically insignificant when $k < 2012$. When examining the heterogeneity of the impact, we will also use the following simplified DID model:

$$y_{ivt} = \eta_i + \gamma_t + \beta_1 Treat_v \times Post_t + Z_{ivt}\theta + \mu_{ivt} \tag{6}$$

where the dummy variable $Post_t$ equals 1 for $t \geq 2012$ and equals 0 otherwise. We also estimate model (6) by Poisson regression to obtain the percentage effect.

[11] The control variables are village-level annual mean temperature, annual total precipitation, average elevation, distance to the provincial capital, distance to the nearest port, and an indicator of ethnicity of the household. The last four time-invariant variables are interacted with a full set of year dummies.

# 5 Impact on Economic Growth

## 5.1 Difference-in-differences estimates

The credibility of the DID estimate depends crucially on the parallel trends assumption that without the policy, the treated counties (i.e., those inside the CDAs) should have the same growth trends as the control counties (i.e., those outside the CDAs). If this hypothesis is satisfied, we should expect to see that the estimates of $\beta_k$ from the flexible DID model 1 is close to zero and statistically insignificant for years before 2012. Figure 5 presents the estimates of the flexible models with the dependent variables of log GDP per capita (Panel A), log agricultural GDP per capita (Panel B), and log non-agricultural GDP per capita (Panel C). In line with the parallel trends assumption, the estimated coefficients for years before 2012 are all very close to zero and statistically insignificant.[12]

[12]The calculation of per capita agricultural GDP involves dividing the county-level total agricultural output by the total county population, as county-level data on agricultural employment are not available for the majority of sample counties. Similarly, per capita non-agricultural GDP is calculated by dividing the county-level total non-agricultural output by the total population of the county due to the lack of sectoral employment data. These measures of sectoral GDP per capita are also preferred in this study because they not affected by the impact of the policy on labor reallocation across sectors. For example, if faster growth in non-agricultural sectors caused by the policy reallocates labor out of agriculture, measuring agricultural GDP per capita by the ratio of agricultural output to agricultural labor could find that the policy increased per capita agricultural GDP even if the total agricultural output is unaffected.

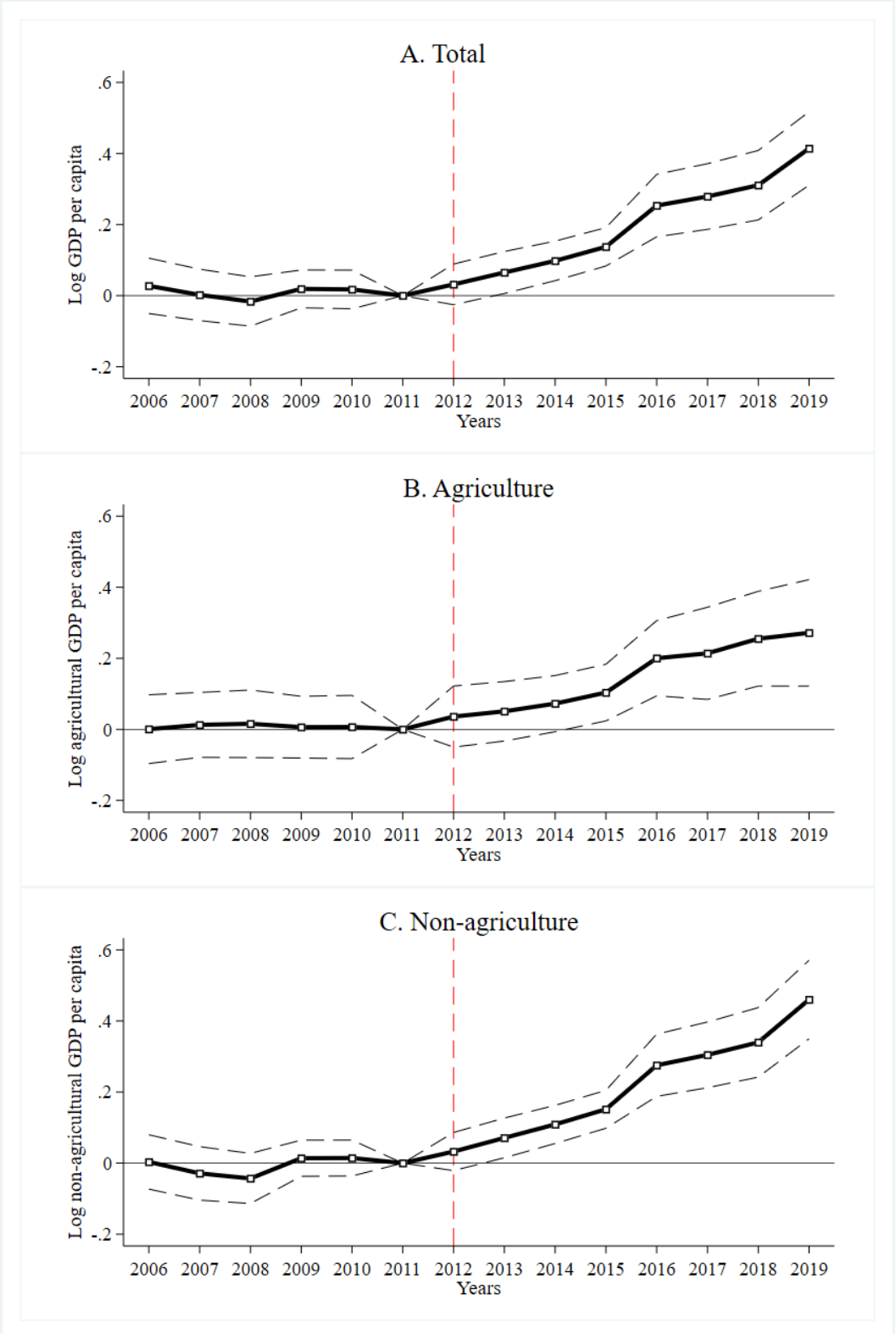


**Figure 5:** Parallel pre-trends and dynamic impact of the policy

*Notes*: This figure presents the estimates of $\beta_k$ from model 1 and the corresponding 95% confidence intervals. The dependent variables in Panels A, B, and C are county-level log GDP per capita, agricultural GDP per capita, and non-agricultural GDP per capita, respectively. All regressions include the year- and county-fixed effects and the two climatic control variables. The dashed vertical line indicates the first year of the policy. The confidence intervals are computed based on standard errors that are clustered at the county level.

Based on the parallel trends assumption, the estimates of $\beta_k$ after 2012 presented in Figure 5 can be interpreted as the dynamic causal impact of the policy relative to the base year of 2011. Panel A shows that the policy increased the GDP per capita of an average treated county by 0.39 log points (or 47.7 percentage points, i.e., $100*(\exp(0.39)-1)$) from 2012 to 2019. Similarly, Panels B and C show that by 2019 the policy increased agricultural and non-agricultural GDP per capita by 0.32 and 0.43 log points, respectively. The corresponding point estimates are also reported in Appendix Table A.4. Combined with the 2011 agricultural GDP per capita (3.34 thousand yuan, in 2011 constant value) and non-agricultural GDP per capita (11.7 thousand yuan) in the CDA counties, these estimates suggest that the policy has increased agricultural GDP per capita by 1.26 thousand yuan and non-agricultural GDP per capita by 6.11 thousand yuan from 2012 to 2019.[13] These estimates are significant relative to the average GDP per capita of 71.3 thousand yuan in 2019 in China. We postpone the discussion of the effect size of the policy to subsection 5.5.

We examine the robustness of the estimated impact on GDP per capita to an alternative income measure, control variables, sub-samples, spillover effects, controls for other preexisting and contemporary policies, and permutation tests. To simplify the analyses, all robustness checks are based on the DID model 2 which captures the average impact of the policy from 2012 to 2019. Column 1 of Table 1 presents the baseline DID estimates, which suggest that the average impact on GDP per capita from 2012 to 2019 was 0.14 log points. Each robustness check maintains the same model settings as the baseline regression, with the exception of the specific modification detailed for each check.

Column 2 replaces the dependent variable by log rural income per capita, which measures the per capita income of rural residents from both farm and off-farm works. The estimated impact is slightly larger than the baseline estimate presented in column 1. We do not use rural income per capita as the dependent variable in our main analysis because it only captures part of the impact of the policy. Recall that the policy provided substantial funds to spur economic development through constructing infrastructures and incentivizing the creation or expansion of enterprises in the poor counties, so non-agricultural sectors must have benefited from the policy.

[13] We transformed the log points to percentage points according to $percentage = exp(logpoint) - 1$.

**Table 1:** Robustness checks

| | (1) Log GDP per capita | (2) Log rural income per capita | (3) Including additional control variables | (4) Excluding Xinjiang and Tibet | (5) Excluding minority counties | (6) Excluding historical revolutionary bases | (7) Excluding border counties | (8) Control for spillover effects | (9) Control for land titling | (10) Excluding second-round targeted counties |
|---|---|---|---|---|---|---|---|---|---|---|
| $Treat_i \times Post_t$ | 0.14*** | 0.16*** | 0.14*** | 0.16*** | 0.13*** | 0.14*** | 0.15*** | 0.15*** | 0.14*** | 0.10*** |
| | [0.02] | [0.01] | [0.02] | [0.02] | [0.02] | [0.03] | [0.03] | [0.02] | [0.02] | [0.02] |
| Ring [-10, 0)×$Post_t$ | | | | | | | | 0.09** | | |
| | | | | | | | | [0.04] | | |
| Ring [-20, -10)×$Post_t$ | | | | | | | | 0.05** | | |
| | | | | | | | | [0.02] | | |
| Ring [-30, -20)×$Post_t$ | | | | | | | | 0.05 | | |
| | | | | | | | | [0.03] | | |
| Titling | | | | | | | | | 0.03** | |
| | | | | | | | | | [0.02] | |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 23,306 | 20,482 | 23,439 | 21,616 | 21,848 | 20,163 | 19,965 | 23,306 | 23,306 | 18,896 |
| R-squared | 0.940 | 0.976 | 0.940 | 0.941 | 0.941 | 0.939 | 0.942 | 0.941 | 0.941 | 0.942 |

*Notes*: This table examines the robustness of the impact on GDP per capita based on the simplified DID model 2. Column 1 presents the baseline estimates, column 2 replaces the dependent variable by log rural income per capita, column 3 additionally controls for the interaction between the four time-invariant control variables and a full set of year dummies, column 4 excludes sample counties from Xinjiang and Tibet provinces, column 5 excludes 107 counties that are dominated by minorities, column 6 excludes 230 counties that are historical revolutionary bases, column 7 excludes 246 counties outside the CADs that are connected to the CAD borders, column 8 explicitly controls for spillover effects by indicators of nearby counties outside the CDAs (e.g., Ring [-10, 0) is a dummy variable indicating whether the county is within the 10 km ring outside the CDAs), column 9 controls for the dummy of land titling for each county, and column 10 excludes 315 counties within the CDAs that were targeted in the last round of county-level poverty alleviation (1994–2001) and were still classified as poverty counties in 2012. The sample size is smaller in column 2 because the data are missing for about one-fifth of counties in some years. Standard errors reported in square brackets are clustered at the county level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Column 3 examines the robustness of the baseline estimate to omitted variables by additionally controlling for the interactions between four time-invariant determinants of economic growth (i.e., the distance to the provincial capital, distance to the nearest port, average elevation, and an indicator of ethnic county) and a full set of year dummies. If the baseline estimation is primarily driven by the potential time-varying effects of these time-invariant factors, we should observe a significant change in the DID estimate. Nonetheless, the resulting estimate is virtually identical to the baseline estimate, suggesting that there are no significant confounding effects from these factors.

Columns 4–6 examine the robustness of the results when excluding sub-samples. Column 4 excludes sample counties from Xinjiang and Tibet, two provinces with much lower population densities and incomes than other provinces. Column 5 excludes 107 counties where the majority of the population consists of minorities to address the concern that the minority counties may not be comparable to other counties. Column 6 excludes 230 counties that were historical revolutionary bases to address the concern that they received massive special development funds beside the poverty alleviation funds. All resulting estimates are comparable to the baseline estimate.

Columns 7 and 8 examine the spillover effects of the policy. Positive or negative spillover effects on counties outside the CDAs could result in an underestimation or overestimation of the policy impact. To examine the spillover effects, column 7 excludes sample counties that are connected to the external borders of the CDAs, as they are most susceptible to spillover effects. The resulting estimate is slightly larger, suggesting a mild positive spillover effect. Column 8 explicitly controls for the spillover effects following the literature (e.g., Zheng et al. 2017, Lu et al. 2019). We extend model 2 to permit separately identifying the treatment effect and the spillover effects on a set of rings around the CDAs:

$$lny_{it} = \eta_i + \gamma_t + \beta_1 Treat_i \times Post_t + \sum_{n=1}^{3} \delta_n Ring(10(n-1), 10n)_i \times Post_t + X_{it}\theta + \varepsilon_{it} \ , \quad (7)$$

where the only difference from model 2 is including $\sum_{n=1}^{3} \delta_n Ring(10(n-1), 10n)_i \times Post_t$, in which $Ring(10(n-1), 10n)_i$ is a dummy variable indicating whether or not county $i$ is located in the $n$th ring that is between $10(n-1)$ and $10n$ km from its nearest CDA border, $n = 1, 2, 3$. Therefore, $\beta_1$ captures the impact of the policy after controlling for the spillover effects, and $\delta_n$ captures the spillover effect on the nearby $n$th ring. We find significantly positive spillover effects over counties within rings 10 km and 20 km, and the spillover effect becomes insignificant for counties further than that. The estimate

of interest, $\beta_1$, is only slightly larger than the baseline estimate (0.15 versus 0.14). In the following calculation of the rate of return, we will use the estimates that account for spillover effects.

Columns 9 and 10 address the concern that the estimated effect of the policy could have been primarily driven by other contemporary or past policies. Column 9 controls for the dummy of land titling that gradually rolls out after 2009 across counties. The data on county-level timing of land titling from 2009 to 2019 are obtained from the Chinese Ministry of Agricultural and Rural Affairs. The estimates suggest that controlling for land titling does not significantly alter the estimated effect of the policy, although we find that land titling increased GDP per capita by 3 log points. Column 10 excludes the 315 counties within the CDAs that were targeted in the last round of county-level poverty alleviation (1994–2001) and were still classified as poverty counties in 2012. Note that as detailed in the baseline analysis, poverty counties of the last round located outside the CDAs have already been excluded. The resulting estimate still suggests that the policy significantly increased the income of counties within the CDAs, although the estimated effect is about one-third smaller than the baseline estimate. We believe that the smaller estimated effect is more likely caused by excluding from the sample the poorest counties that could benefit most from the policy, rather than the confounding effect of the last round of poverty alleviation.

Finally, we conducted permutation tests and present the results in Appendix Figure A.4. We randomly select 674 counties as the treated counties and estimate model 2 to obtain a placebo estimate of $\beta_1$. We repeat this process 1000 times and plot the density function of the placebo estimates in the figure. The estimate from the actual data (indicated by the vertical red dashed line) is far to the right of the placebo distribution and thus unlikely to have arisen by chance.

## 5.2 Mechanisms of the impact

We examine the mechanisms of the impact of the policy by estimating versions of model 2 with different dependent variables. Specifically, we examine the effect of the policy on the growth of government investments, agricultural input and output, enterprises, education, and savings. As presented in Table 2, the DID estimates suggest that the policy significantly increased per capita budget expenditure by 0.15 log points (column 1), fixed asset investment by 0.31 log points (column 2), crop sown area by 0.04 log points (column 3), crop output by 0.09 log points (column 4), agricultural mechanical power by 0.21 log points (column 5), the number of large industrial enterprises by

0.17 log points (column 6),[14] the output of large industrial enterprises by 0.23 log points (column 7), the number of middle schools by 0.09 log points (column 8), and savings account balances by 0.20 log points (column 9).[15] Appendix Figure A.5 presents the corresponding dynamic event study for each of these variables. The event study estimates confirm that the policy has no significant effects before the start of the policy but significantly increases each of these variables afterward.

These mechanism analyses help us to gain more understanding of how the policy significantly increased the growth of the targeted counties. Specifically, the estimated effects of the mechanism variables are consistent with the main targets of the flagship programs (i.e., Industrial Poverty Alleviation, Whole Village Advancement, Relocation, and Food-for-Work, see subsection 2.3 for details) of the fourth round of poverty alleviation: to improve infrastructure of all types (e.g., roads, irrigation systems, housing, and education) and to spur industry growth through providing financial resources and other benefits to incentivize the creation or expansion of enterprises. The significant growth of budget expenditure, fixed asset investment, and the number of schools confirms that the policy has substantially increased government investments in the targeted areas. The estimated increases in agricultural mechanization and agricultural output suggest that substantial funds have been invested in infrastructures important for agricultural mechanization and agricultural growth, such as rural roads and irrigation systems, although we do not have data to directly examine these more specific effects. Finally, the significant growth of industrial enterprises and savings account balances is consistent with the main target of the policy of spurring industry growth through providing financial resources and other benefits.

[14]The large industrial enterprises refer to those generating annual revenue exceeding 20 million yuan.

[15]There are many other interesting mechanisms that could have been examined, such as the effects on transportation, agricultural technology, financial services, and information and communications technology. However, the county- or city-level data on these variables are generally unavailable, especially for years before the policy, which is necessary for our DID estimation.

**Table 2:** Mechanisms of the impact

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | All dependent variables are measured in log and per capita | | | | | | | | |
| | Budget expenditure of local finance | Fixed asset investment | Crop sown area | Crop output | Agricultural mechanical power | Number of large enterprises | Output of large enterprises | Number of middle schools | Savings account balances |
| $Treat_i \times Post_t$ | 0.15*** | 0.31*** | 0.04*** | 0.09*** | 0.21*** | 0.17*** | 0.23*** | 0.09*** | 0.20*** |
| | [0.02] | [0.04] | [0.01] | [0.02] | [0.02] | [0.02] | [0.03] | [0.02] | [0.01] |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes | -0.73*** | 9.74*** |
| Year-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | [0.07] | [0.06] |
| County-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 17,282 | 17,171 | 12,929 | 23,483 | 21,686 | 25,014 | 19,673 | 11,347 | 21,547 |
| R-squared | 0.925 | 0.931 | 0.978 | 0.972 | 0.951 | 0.937 | 0.913 | 0.800 | 0.955 |

*Notes*: This table reports the estimates of different versions of model 2 with the dependent variable listed in the header of each column. All regressions include the year- and county-fixed effects and the two climatic control variables. Standard errors reported in square brackets are clustered at the county level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

## 5.3 Heterogeneity of the impact

We examine the heterogeneity of the impact based on the following DID model 2:

$$lny_{it} = \eta_i + \gamma_t + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i \times Post_t \times Index_i + X_{it}\theta + \varepsilon_{it} \ , \qquad (8)$$

where the only difference from the baseline DID model 1 is that we include the interaction between a factor of interest ($Index_i$) and the DID component ($Treat_i \times Post_t$). The $Index_i$ is demeaned, so that the coefficient $\beta_1$ has the same interpretation as that in the baseline DID model. The coefficient of the new interaction term, $\beta_2$, captures the heterogeneity of the impact across the factor of interest. As presented in Table 3, we examined the heterogeneity across counties with different initial incomes and agricultural shares and across CDAs with different economic sizes and precision of targeting.

Column 1 reveals that counties with lower initial GDP per capita in 2011 benefited more from the policy. Specifically, a reduction of one thousand yuan in the initial GDP per capita would lead to an increase in impact by 0.0039 log points. Combining this estimate with the standard deviation (SD) of 13.3, suggests that a 1-SD higher initial income reduced the impact by 0.052 log points. Therefore, the policy not only increased economic growth but also reduced income inequality among the treated counties.

Column 2 indicates that the county-level initial share of agriculture in GDP had no significant moderating effect on the impact of the policy. This result could be explained by our previous findings that the policy increased agricultural growth less than non-agricultural growth and that counties with lower initial income (generally those with a higher agricultural share) gained more from the policy. These two opposing effects of a higher initial agricultural share could have offset each other. This finding contrasts with the expectation that agricultural counties could have benefited more from the policy because of their lower initial income.

**Table 3:** Heterogeneity of the impact (Dependent variable: Log GDP per capita)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | $Index_i$ | | | | | |
| | County-level GDP per capita in 2011 (thousand yuan) | County-level share of agricultural in GDP in 2011 | Number of counties in each CDA | Total population in each CDA (10 million) | Total GDP in each CDA (10 billion yuan) | Rate of mis-targeting of each CDA |
| $Treat_i \times Post_t$ | 0.1378*** | 0.1380*** | 0.1386*** | 0.1329*** | 0.1344*** | 0.1349*** |
| | [0.0120] | [0.0122] | [0.0121] | [0.0117] | [0.0118] | [0.0119] |
| $Treat_i \times Post_t \times Index_i$ | -0.0039*** | -0.0480 | 0.0015*** | 0.0800*** | 0.0057*** | -0.0061*** |
| | [0.0010] | [0.0944] | [0.0005] | [0.0089] | [0.0007] | [0.0008] |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| County-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 23,285 | 23,295 | 23,306 | 23,306 | 23,306 | 23,306 |
| R-squared | 0.941 | 0.940 | 0.941 | 0.941 | 0.941 | 0.941 |

*Notes*: This table examines the heterogeneity of the impact of the policy based on model 8. Each column header presents the factor for which the heterogeneous impact is examined. These factors are demeaned, so that the coefficient of the DID measure has a usual interpretation. Standard errors reported in square brackets are clustered at the county level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

A potential concern regarding the estimates presented in columns 1 and 2 of Table 3 is that counties with different initial income and agricultural share may not be comparable to each other. In other words, although we have shown the parallel trends for the whole sample (Figure 5), the estimates in columns 1 and 2 depend on the additional assumption that counties inside the CDAs are comparable to counties with similar initial income and agricultural share outside the CDAs. We provide evidence supporting this additional assumption by examining the parallel trends separately for counties initially with low income, high income, low agricultural share, and high agricultural share.[16] As presented in Appendix Figure A.6, the event-study estimates are all close to zero and statistically insignificant before 2012 for each of these sub-groups, supporting the parallel trends assumption. Additionally, Appendix Table A.5 further addresses this concern by replicating Table 3 using the DID-RD estimation. The DID-RD estimates are very close to the DID estimates.

Columns 3–5 show that CDAs with a larger economic size gained more from the policy. We measure the economic size of CDA by the number of counties contained (column 3), the initial total population (column 4), and the initial total GDP (column 5).[17] The estimates suggest that all these measures have a significantly positive interaction effect on the impact of the policy. Combining the estimates with the corresponding SDs we find that the interaction effects are economically large (0.029, 0.085, and 0.077 log points). This finding rationalizes the targeting of CDAs: enlarging the economic size of the area targeted could enhance the impact of the policy.

However, a potential trade-off is that targeting CDAs could result in a low precision of targeting, which in turn reduces the impact of the policy (Elbers et al. 2007, Baker & Grosh 1994). To examine this we follow Park et al. (2002) to construct a measure of the precision of targeting for each CDA:

$$T_j = 100 * \frac{1}{N_j} \sum_{i=1}^{N_j} I_{ij}(P_{ij} = 1, Y_{ij} > Z)$$

where $T_j$ is the targeting precision of CDA$j$, $N_j$ is the number of counties in CDA$j$, $I_{ij}$

[16] Specifically, we estimate model (1) separately for each of these four groups of counties. We define low-income counties as those with 2011 GDP per capita below the 70th percentile, and high-income counties as those with 2011 GDP per capita above the 30th percentile. We do not define the low- and high-income counties according to the median of 2011 GDP per capita because most of the poor counties are located within the CDAs; defining according to the median would lead to a too small sample in the control or treated groups. Similarly, the agricultural- and non-agricultural counties are defined based on the 70th and 30th percentiles of 2011 share of agriculture in GDP.

[17] For counties outside the CDAs, these variables are set to zero to ensure a usual interpretation of $\beta_1$. To do so does not affect the estimated coefficient of $\beta_2$ because it is determined by counties inside the CDAs (i.e., $Treat_i = 1$).

is an indicator variable that equals one if county $i$ is in the CDA ($P_{ij} = 1$) but its GDP per capita ($Y_{ij}$) is above the poverty line ($Z$). As the fourth round of poverty alleviation has no explicit poverty line (see Section 2.3 for details), we define the poverty line as the mean GDP per capita in 2011 of counties ranked at the second half of all sample counties outside the CDAs. Thus, $T_j$ can be interpreted as the percentage of counties in CDA $j$ that are mistargeted. The mean and SD of $T_j$ are 16.2% and 11.1%, respectively. As presented in column 6, a 1% increase in $T_j$ reduces GDP per capita by 0.0061 log points. Therefore, a 1-SD increase in the mistargeting would reduce the impact by 0.0677 log points.[18]

## 5.4 Difference-in-discontinuities estimates

We further validate the impact of the policy on GDP per capita by estimating the DID-RD model 3. The dependent variable is log GDP per capita and the bandwidth is set at 100km.[19] The estimates are presented in Figure 6 and repeated in Appendix Table A.6. These estimates are all close to zero and statistically insignificant for years before 2012, supporting the parallel trends assumption. The estimated accumulated effect by 2019 is 0.33 log points, which is smaller than the DID estimate of 0.39 log points. This is potentially because the DID-RD estimate captures only the local average treatment effect, while counties closer to the CDA center were likely to be poorer and benefited more from the policy. Another potential explanation is the downward bias resulting from the spillover effects, which are described in detail below.

[18]Because we do not know the precision of alternative targeting strategies, we cannot compare the loss from the (additional) mistargeting and the gain from the larger economic sizes targeted under the CDA targeting strategy.

[19]We also estimated the impact on log agricultural GDP per capita (Appendix Table A.6) and tested the robustness of bandwidths (Table 4).

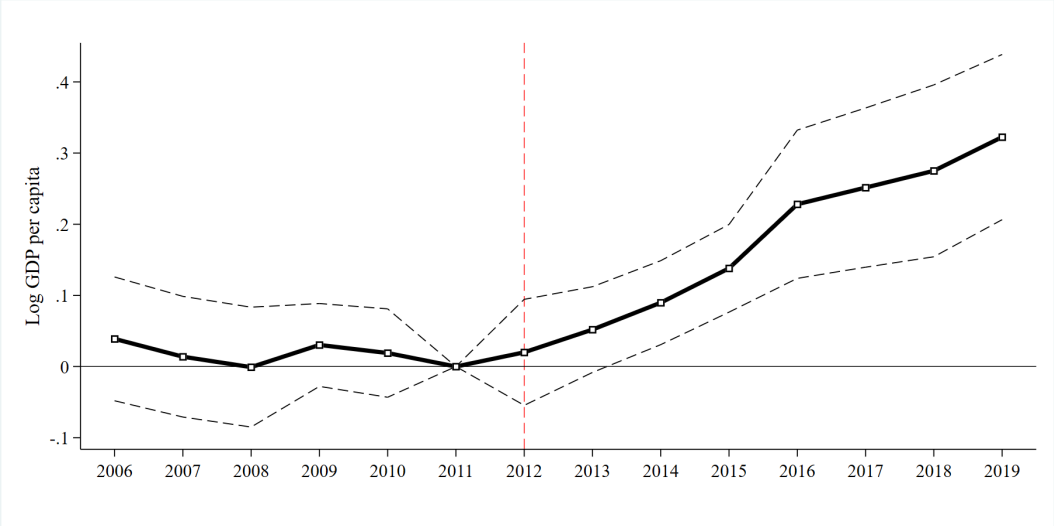


**Figure 6:** Dynamic impact of the policy on log GDP per capita, DID-RD estimates
*Notes*: This figure presents the DID-RD estimates and 95 percent confidence intervals based on model 3. The dependent variable is log GDP per capita , and the bandwidth is set at 100km. The regression includes the year- and county-fixed effects and the two climatic control variables. The dashed vertical line shows the first year of the policy. The confidence intervals are computed from standard errors clustered at the county level.

We examine the sensitivity of the DID-RD estimates to the choice of bandwidths and the spillover effects based on the simplified DID-RD model 4. Column 1 of Table 4 presents the baseline DID-RD estimates with a bandwidth of 100km. Columns 2 and 3 raise the bandwidths to 125km and 150km, respectively, and find identical estimated effects. Columns 4 and 5 reduce the bandwidths to 75km and 50km, respectively, and find smaller estimated effects. A potential explanation of the smaller estimated effect is that the local average treatment effect declines with the bandwidth. In addition, a narrower bandwidth could amplify the downward bias from the positive spillover effects.

**Table 4:** Sensitive to bandwidths and spillover effects

| | (1) 100 km | (2) 125 km | (3) 150 km | (4) 75 km | (5) 50 km | (6) 100 km, control for spillover |
|---|---|---|---|---|---|---|
| $Treat_i \times Post_t$ | 0.15*** | 0.15*** | 0.15*** | 0.13*** | 0.11*** | 0.20*** |
| | [0.03] | [0.03] | [0.03] | [0.03] | [0.03] | [0.04] |
| Ring [-10, 0)×$Post_t$ | | | | | | 0.11** |
| | | | | | | [0.05] |
| Ring [-20, -10)×$Post_t$ | | | | | | 0.09** |
| | | | | | | [0.04] |
| Ring [-30, -20)×$Post_t$ | | | | | | 0.09** |
| | | | | | | [0.05] |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| County-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 13,582 | 14,555 | 15,459 | 11,917 | 9,694 | 13,582 |
| R-squared | 0.940 | 0.940 | 0.941 | 0.938 | 0.936 | 0.941 |

*Notes*: Column 1 presents the baseline DID-RD estimates based on model 4 with the dependent variable of log GDP per capita and a bandwidth of 100km. Columns 2–5 test the sensitivity to alternative bandwidths. Column 6 additionally controls for indicators of spillover effects. All regressions include the year- and county-fixed effects and the two climatic control variables. Standard errors reported in square brackets are clustered at the county level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

To examine the importance of spillover effects under the DID-RD model setting, column 6 replicates the baseline estimation but additionally controls for the spillover effects by indicators of 10km, 20km, and 30km rings (i.e., $\sum_{n=1}^{3} \delta_n Ring(10(n-1), 10n)_i \times Post_t$ introduced in model 7). The estimated spillover effects are significantly positive and the estimated impact of the policy becomes larger. Appendix Tables A.6 and A.4 show that the accumulated effect estimated based on the flexible DID-RD model that controls for the spillover effects is very close to the corresponding DID estimate (i.e., 0.38 versus 0.40 log points).

## 5.5 Rate of return

We evaluate the rate of return on public poverty alleviation investments based on the DID and DID-RD estimates, controlling for spillover effects. As reported in column 3 of Tables A.4 and A.6, the DID and DID-RD estimates indicate that the policy increased log GDP per capita by 0.40 and 0.38 log points, respectively, by 2019. Combining these two estimates (after transforming to percentage points) with the GDP per capita of the 674 CDA counties in 2011, we calculate that the policy increased per capita

GDP by 5.56 and 5.23 thousand yuan by 2019. The central and provincial poverty alleviation funds during 2012–2019 amount to 813.6 billion yuan, which corresponds to 3.35 thousand yuan per capita. Therefore, as presented in columns 1 and 2 of Table 5, the DID and DID-RD estimates indicate that the rates of return are 165.8% and 155.8%, respectively. In other words, every yuan spent yielded between 1.56 and 1.66 yuan of benefit.

A major concern of the above calculation is that the exact investment in poverty alleviation is unknown. As detailed in subsection 2.3, we only know that the central and provincial poverty alleviation funds during 2012–2019 amount to 813.6 billion yuan and that the majority of these funds are invested in the 14 CDAs. However, the CDAs also received support from the Industry Funds and the Social Poverty Funds, which are not earmarked for the 14 CDAs but were likely substantially invested in these particular areas. Due to the dearth of data, we do not know how much of the funds from these latter two sources were invested in the CDAs. Additionally, due to the extension of the precise poverty alleviation projects, not all of the governmental specialized anti-poverty funds were invested in the CDAs. To address this concern, we adopt two alternative investment scenarios, one double and the other half the total investment of 813.6 billion yuan. As reported in rows 2 and 3 of Table 5, the calculated rate of return is doubled or halved.

**Table 5:** Returns to public investments (%)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | GDP per capita | | Agricultural GDP per capita | |
| Investment Scenarios | DID | DID-RD | DID | DID-RD |
| (1) All funds | 165.8 | 155.8 | 49.6 | 34.4 |
| (2) Half funds | 331.6 | 311.7 | 99.2 | 68.8 |
| (3) Double funds | 82.9 | 77.8 | 24.8 | 17.2 |

*Notes*: Columns 1 and 2 present the rates of return calculated based on the estimated impacts on GDP per capita reported in column 3 of Tables A.4 and A.6. Columns 3 and 4 report the rates of return calculated based on the estimated impacts on agricultural GDP per capita reported in column 4 of Tables A.4 and A.6. Row 1 assumes that the total poverty alleviation funds invested in the 14 CDAs are the 813.6 billion yuan of governmental specialized anti-poverty funds, row 2 assumes that the investment was half of this amount, and row 3 assumes that the investment was double of this amount.

Regardless of which investment scenarios are adopted, the estimated rates of return are much higher than those estimated in the literature based on China's previous rounds of poverty alleviation programs. Specifically, Park et al. (2002) and Meng (2013) estimated that the rates of return to the first and second rounds of China's

poverty alleviation programs were 15.5% and 42.4%, respectively.[20] This difference can be explained by the fact that while geographic targeting poverty alleviation programs promoted both agricultural and non-agricultural growth, their rates of return estimation were solely based only on the impact on rural income.[21] As presented in columns 3 and 4 of Table 5, when calculating the rate of return based on the estimated impact on agricultural GDP (reported in column 4 of Tables A.4 and A.6), we find rates of return comparable to those from the literature.

# 6 Impact on Household Income and Poverty Incidence

## 6.1 Income and poverty incidence

A major advantage of our study compared with studies evaluating China's previous rounds of geographic targeting poverty alleviation programs is that our study is able to evaluate the impact on rural households. This enables us to answer the following important questions: Did the program that substantially increased macro economic growth significantly reduce rural poverty? Is rural poverty reduction mainly driven by agricultural or non-agricultural income growth? What are the differences in the impact on households from different income groups?

Figure 7 presents the estimated impact of the policy on rural household income based on the flexible DID model 5. Again, the assumption of parallel trends is supported by the insignificant impact of the policy before 2012. The policy increased household income over time after 2012, and the accumulated impact reached 0.23 log points by 2015 (i.e., the end of our data set). This impact is larger than the estimated county-level impact on economic growth by 2015 of 0.13 log points, reported in Figure 5 and Appendix Table A.4. Therefore, we confirm that the benefit from the poverty alleviation policy had been disproportionately gained by households from rural areas, where poverty incidences were much higher.

[20] Due to the lack of data on funds from other sources, they also depended only on governmental specialized anti-poverty funds to calculate the rate of return.

[21] This is probably due to the county-level GDP or non-agricultural GDP data not being available in the early years.

**Figure 7:** Dynamic impact of the policy on household-level income

*Notes*: This figure presents the percentage effect and the corresponding 95% confidence intervals estimated based on model 5 using Poisson regression and the log point transformation of $exp(\beta_k) - 1$. The dashed red vertical line indicates the first year of the policy.

We move on to examine the impact of the policy on household income from different sources and on poverty incidence based on the simplified DID model 6. As reported in Table 6, the policy significantly increased household total income (column 1), agricultural income (column 2), and off-farm income (column 3). The impact on off-farm income (0.25 log points) is larger than the impact on agricultural income (0.11 log points). Therefore, we conclude that the impact of the policy on rural income was driven by both agricultural income growth and off-farm income growth, but the latter contributed more. The agricultural income is defined as the net income from all agricultural activities (i.e., food and cash crops planting, livestock farming, and fisheries) for all family members in a calendar year.[22] The off-farm income is defined as the sum of all non-agricultural incomes of the family, which include mainly incomes from off-farm employment and off-farm self-employed business. The total income is the sum of agricultural and off-farm incomes.

[22]The agricultural income also includes agricultural subsidies in the form of direct transfer payments or indirect subsidies on prices and inputs.

**Table 6:** Impact of the policy on rural household income and poverty incidence

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Total income | Agricultural income | Off-farm income | Income indicator < \$1.9 | Income indicator [\$1.9, \$3.2) | Income indicator [\$3.2, \$5.5) | Income indicator > \$5.5 |
| $Treat_v \times Post_t$ | 0.22*** | 0.11*** | 0.25*** | -0.09*** | 0.04*** | 0.04*** | 0.01 |
| | [0.02] | [0.03] | [0.03] | [0.01] | [0.01] | [0.01] | [0.01] |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Household-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 141,140 | 141,140 | 141,140 | 141,140 | 141,140 | 141,140 | 141,140 |

*Notes*: This table presents the estimates of various versions of model 6 with the depend variables of total income (column 1), agricultural income (column 2), off-farm income (column 3), and four indicators of income groups (columns 4–7) as defined in the main text. The estimates reported in columns 1–3 are the percentage effects estimated based on model (6) using Poisson regression and the log point transformation of $exp(\beta_k) - 1$. The estimates reported in columns 4–7 are the panel fixed effect estimates. All monetary values are measured in 2011 constant yuan. All regressions include the year- and household-fixed effects and the two climatic control variables. Robust standard errors are reported in square brackets. Significance levels are *** p $<$ 0.01, ** p $<$ 0.05, and * p $<$ 0.1.

Columns 4–7 in the table examine the impact of the policy on poverty incidence. We construct dummy variables indicating whether a rural household belongs to a certain income group in each year. We define four income groups based on the poverty lines of per day per capita income of US$1.90 (for extreme poverty), US$3.20 (for poverty in lower-middle-income countries), and US$5.50 (for poverty in upper-middle-income countries).[23] For example, the indicator of the extreme poverty group equals 1 for all households with per day per capita income below US$1.90 in a year and equals 0 for all other households in the same year. The indicator changes from 1 to 0 for a household that moved out of the group. We estimate the impact of the policy on each of these indicators based on model 6. We find that the policy reduced the share of households under extreme poverty by 9 percentage points during 2012–2015 (column 6). Combining with the share of rural population in the CDAs in the extreme poverty group in 2011 (0.452), this estimate suggests that the policy has reduced the share of the rural population in extreme poverty by 4.07 percentage points from 2012 to 2015. The reduced extreme poor households switched into the groups of relative poverty (columns 7 and 8) rather than the group of non-poverty (column 9).

## 6.2 Heterogeneity of the impact

A more comprehensive examination of the heterogeneous impacts across income groups is presented in Table 7. Instead of constructing income groups based on the income in each year, which is suitable when examining the impact on poverty incidence, here we construct income groups based on the initial income before the policy. Specifically, we classify sample households into four groups based on their average income during 2009–2011 and on the poverty lines of US$1.90, US$3.20, and US$5.50.[24] We then estimate the impact of the policy on the income of households from each group separately based on model 6.

The estimates suggest that the policy raised the income of households in all income groups, and the largest percentage income growth is observed in the poorest group. Specifically, columns a1, b1, c1, and d1 show that the policy increased the income of households from the first to the last groups, respectively, by 0.21, 0.15, 0.18, and 0.16 log points. The higher impact on households with a lower initial income suggests that more benefits of the policy were captured by the poorer. This finding stands in stark

[23] The average shares of households in the groups of < \$1.9, [\$1.9, \$3.2), [\$3.2, \$5.5), and > \$5.5 are 0.27, 0.32, 0.30, and 0.11, respectively, during 2006–2015.

[24] We define the initial income based on data from three years prior to the policy to reduce the influence of annual income shocks.

opposition to the conclusion drawn by Park & Wang (2010), who showed that the third round of poverty alleviation (which targeted poor villages) increased the income of the relatively rich but not poor rural households during 2001–2004.

We also find that while the income growth of the extremely poor households was driven more by agricultural income growth, the income growth of the relatively poor households was driven mainly by off-farm income growth. Specifically, columns a2, b2, c2, and d2 show that the impacts on the agricultural income of the first to the fourth groups were, respectively, 0.26, 0.04, 0.01, and 0.05 log points. Columns a3, b3, c3, and d3 show that the impacts on the off-farm income of the first to the fourth groups were, respectively, 0.19, 0.11, 0.17, and 0.26 log points. These findings imply that poverty alleviation programs could reduce extreme poverty by increasing both agricultural and non-agricultural growth. However, when extreme poverty has been eliminated (such as the claimed case in China after 2020), the focus of poverty alleviation should be shifted to promoting non-agricultural growth.

**Table 7:** Heterogeneity of the impact across income groups

| | (a1) | (a2) | (a3) | (b1) | (b2) | (b3) | (c1) | (c2) | (c3) | (d1) | (d2) | (da3) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | < US$1.90 | | | [US$1.90, US$3.20) | | | [US$3.20, US$5.50] | | | > US$5.50 | | |
| | Log total income | Log agri-cultural income | Log off-farm income | Log total income | Log agri-cultural income | Log off-farm income | Log total income | Log agri-cultural income | Log off-farm income | Log total income | Log agri-cultural income | Log off-farm income |
| $Treat_v \times Post_t$ | 0.21*** | 0.26*** | 0.19*** | 0.15*** | 0.04*** | 0.11*** | 0.18*** | 0.01 | 0.17*** | 0.16*** | 0.05 | 0.26*** |
| | [0.04] | [0.07] | [0.05] | [0.05] | [0.01] | [0.03] | [0.06] | [0.01] | [0.07] | [0.07] | [0.10] | [0.10] |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 70,724 | 70,724 | 70,724 | 35,178 | 35,178 | 35,178 | 22,919 | 22,919 | 22,919 | 16,477 | 16,477 | 16,477 |

*Notes*: This table presents the estimates of model 6 based on data for households from each income group. Specifically, the estimates reported are the percentage effects estimated based on model (6) using Poisson regression and the log point transformation of $exp(\beta_k) - 1$. We classified sample households into four groups based on their average income during 2009–2011 and the poverty thresholds of US$1.90, US$3.20, and US$5.50. All regressions include the year- and household-fixed effects and the time-variant control variables. Robust standard errors are reported in square brackets. Significance levels are *** p < 0.01, ** p < 0.05, and * p < 0.1.

# 7 Concluding Remarks

Although China has carried out four major rounds of poverty alleviation programs over the past four decades, evidence on the impact of these programs is still limited. Importantly, while several studies have examined the impact of the programs on rural income growth, few studies have directly examined the impact on poverty incidence due to the lack of microdata. In addition, studies that have examined previous rounds of poverty alleviation programs tend to underestimate the return rate primarily because they focused on the impact on county-level rural income growth, lacking county-level data on non-agricultural growth. The availability of rich micro and macro data during the fourth round of poverty alleviation enables us to directly examine the impact on poverty incidence and more precisely evaluate the cost-effectiveness of the program.

Our county-level estimates suggest that the program substantially increased the economic growth of poor counties, driven by both agricultural and non-agricultural growth. When considering the impact of the program on both agricultural and non-agricultural growth, we estimate a rate of return much higher than that estimated in previous studies that only accounted for the impact on rural income growth. Our household-level estimates indicate that the program substantially increased the income of rural households and reduced rural poverty. Additionally, we find that while the alleviation of extreme poverty depends more on agricultural growth, the alleviation of relative poverty depends mainly on non-agricultural growth.

We conclude by highlighting three limitations of this study. First, due to the lack of exact data on the total investment of the fourth round of poverty alleviation program in the 14 CDAs, we could have overestimated or underestimated the rate of return. To address this issue, we can only mitigate this concern by assuming two "extreme" scenarios of investments and then examine the possible range of the rate of return. Second, although the fourth round of poverty alleviation explicitly targeted the 14 CDAs, the extension of the household-level precise poverty alleviation (which was part of the fourth round of poverty alleviation) started in 2013, which should have shifted part of the poverty alleviation efforts to areas outside the CDAs. To the extent that the precise poverty alleviation projects were implemented in areas outside the CDAs, this study tends to underestimate the impact of the fourth round of poverty alleviation program. Finally, as our microdata end in 2015, we do not know the final distributional effects of the program across rural households with different initial incomes.

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# 8 Appendix for Online Publication

## 8.1 Summary statistics

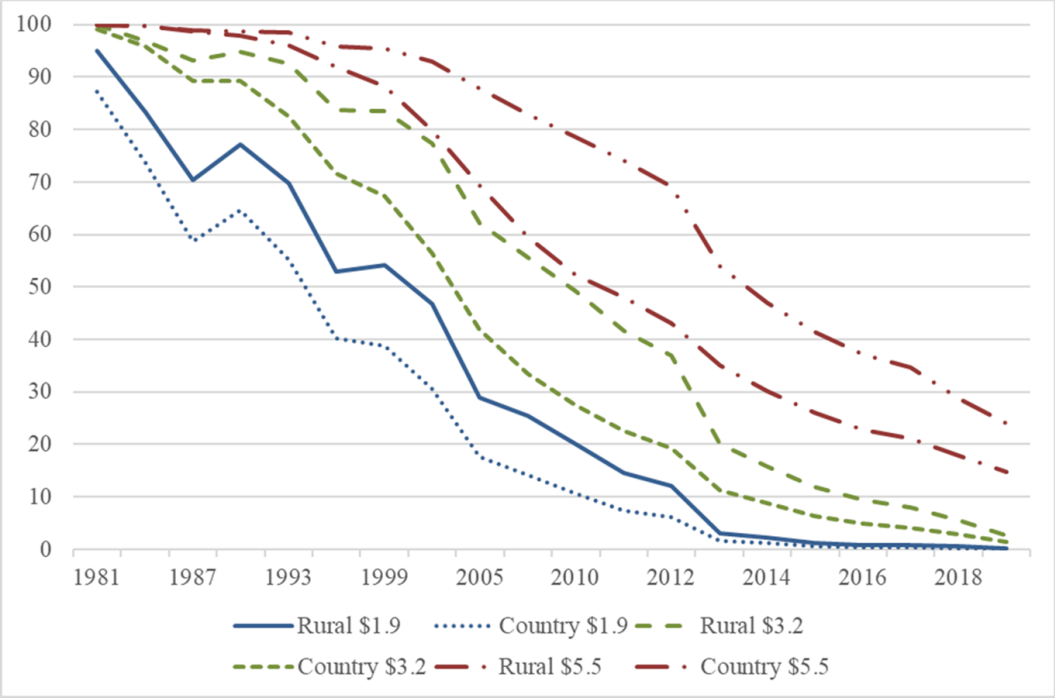


**Figure A.1:** Poverty headcount ratio in China

*Note*: The US$1.90 per day threshold is the international poverty line defined by the World Bank, the US$3.20 per day line is typical of lower-middle-income countries, and the US$5.50 per day line is typical of upper-middle-income countries. The data are derived from PovcalNet 1981–2019 (https://gdc.unicef.org/).

**Table A.1:** Details of contiguous destitute areas

| Area code | Contiguous destitute area | Number of counties contained | Total population (thousand) | Total poor population (thousand) | Poverty headcount ratio (%) |
|---|---|---|---|---|---|
| 1 | South of greater Khingan mountain | 19 | 6849.3 | 857.4 | 12.52 |
| 2 | Yanshan-Taihang mountain area | 33 | 11022.2 | 2724.6 | 24.72 |
| 3 | Lvliang mountain area | 20 | 4052.7 | 1157.7 | 28.57 |
| 4 | Liupan mountain area | 61 | 21372.3 | 5204.6 | 24.35 |
| 5 | Qinba mountain area | 75 | 35009.8 | 5951.1 | 17.00 |
| 6 | Dabie mountain area | 36 | 36194.1 | 4559.4 | 12.60 |
| 7 | Tibetan ethnic areas | 77 | 5570.4 | 1017.1 | 18.26 |
| 8 | Wuling mountain area | 64 | 34781.7 | 6551 | 18.83 |
| 9 | Yunnan-Guizhou-Guangxi Karst Mountain area | 80 | 29706.6 | 7020.5 | 23.63 |
| 10 | Luoxiao mountain area | 23 | 11424.5 | 1912.3 | 16.74 |
| 11 | Wumeng mountain area | 38 | 23626.7 | 4547.4 | 19.32 |
| 12 | Mountainous borderland of Western Yunnan | 56 | 15704.5 | 2710.5 | 17.26 |
| 13 | Three-district south of Xinjiang | 24 | 6976.5 | 1981.9 | 28.41 |
| 14 | Tibet area | 74 | 2960.6 | 796.8 | 26.91 |
| / | Sum of the 14 CDAs | 680 | 245251.9 | 46992.3 | 20.65 |

*Notes*: The data are derived from China Poverty Alleviation and Development Report (2016).

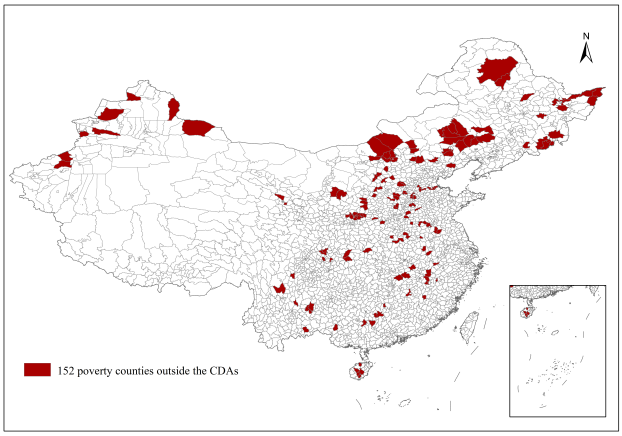


**Figure A.2:** 152 national poverty counties outside the contiguous destitute areas

*Notes*: There were 832 poverty counties in 2012, among which 680 were in the 14 contiguous destitute areas (presented in Figure 2), and the remaining 152 were outside the contiguous destitute areas (presented in this figure).

**Table A.2:** Summary statistics of county-level key variables

| | Inside CDAs | | | Outside CDAs | | |
|---|---|---|---|---|---|---|
| | N | Mean | S.D. | N | Mean | S.D. |
| GDP (1,000 yuan) | 8963 | 15.1 | 13.3 | 14343 | 35.4 | 33.5 |
| Agricultural GDP (1,000 yuan) | 7265 | 5.2 | 3.3 | 10261 | 8.0 | 5.9 |
| Non-agricultural GDP (1,000 yuan) | 7187 | 8.6 | 8.6 | 10205 | 26.0 | 34.1 |
| Rural income (1,000 yuan) | 7220 | 5.1 | 2.5 | 12889 | 9.2 | 4.3 |
| Fixed asset investment (1,000 yuan) | 7118 | 14.4 | 15.9 | 11872 | 24.4 | 31.0 |
| Budget expenditure (1,000 yuan) | 8963 | 7.0 | 7.2 | 14343 | 5.7 | 5.9 |
| Crop sown area (ha) | 4336 | 0.2 | 0.1 | 8766 | 0.2 | 0.2 |
| Crop output (kg) | 8410 | 445.8 | 433.8 | 14110 | 655.1 | 646.5 |
| Mechanical power (kWh) | 7955 | 0.8 | 0.8 | 13370 | 1.0 | 0.7 |
| Number of firms (per 10,000 people)* | 8478 | 0.8 | 0.8 | 14343 | 2.7 | 2.8 |
| Firms outputs (1,000 yuan) | 7136 | 9.4 | 18.2 | 12478 | 48.6 | 68.2 |

*Notes*: This table presents summary statistics of county-level key variables from 1764 counties from 2016 to 2019. Some variables have a smaller number of observations due to missing data. The county-level data are derived from the China County Statistical Yearbook. *The number of firms refers to the number of large industrial firms per 10,000 people, calculated as the ratio of the number of large industrial firms to the population.

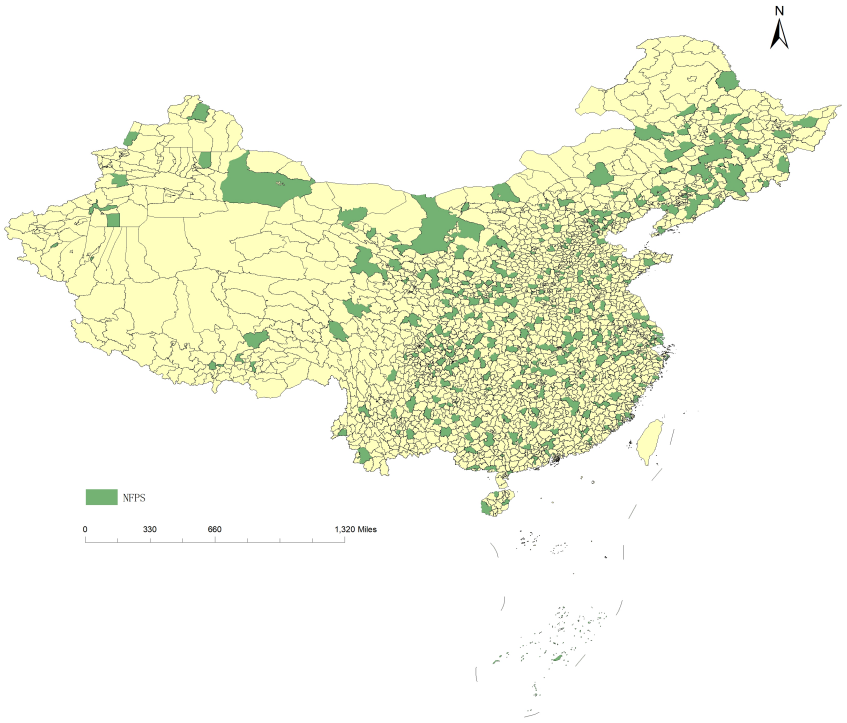


**Figure A.3:** Map of the NFP sample counties

*Note:* The 348 sample villages from the NFP would be points within the 332 counties highlighted in green.

**Table A.3:** Summary statistics of household-level key variables

| | Inside CDAs | | | Outside CDAs | | |
|---|---|---|---|---|---|---|
| | N | Mean | S.D. | N | Mean | S.D. |
| Agricultural income (1,000 yuan) | 30774 | 7.4 | 11.3 | 106378 | 10.2 | 15.0 |
| Off-farm income (1,000 yuan) | 30774 | 15.9 | 27.4 | 106378 | 19.2 | 35.3 |
| Total income (1,000 yuan) | 30774 | 23.3 | 28.6 | 106378 | 29.4 | 36.5 |
| Income per day (yuan/ per capita) | 30774 | 15.3 | 19.8 | 106378 | 20.6 | 27.3 |

*Notes*: This table presents summary statistics of household-level key variables from more than 13,000 households in each year from 2006 to 2015. The household-level data is derived from the National Fixed Point Survey. All monetary values are measured in 2011 constant yuan.

## 8.2 DID estimates

**Table A.4:** Dynamic impact of the policy (DID estimates)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | DID estimates | | DID estimates, control for spillovers | |
| | Log GDP per capita | Log agricultural GDP per capita | Log GDP per capita | Log agricultural GDP per capita |
| 2006 dummy×$Treat_i$ | 0.05 | 0.03 | 0.05 | 0.03 |
| | [0.03] | [0.04] | [0.03] | [0.04] |
| 2007 dummy×$Treat_i$ | 0.02 | 0.05 | 0.02 | 0.05 |
| | [0.03] | [0.04] | [0.03] | [0.04] |
| 2008 dummy×$Treat_i$ | -0.00 | 0.05 | -0.00 | 0.05 |
| | [0.03] | [0.04] | [0.03] | [0.04] |
| 2009 dummy×$Treat_i$ | 0.04 | 0.04 | 0.04 | 0.04 |
| | [0.03] | [0.03] | [0.03] | [0.03] |
| 2010 dummy×$Treat_i$ | 0.02 | 0.02 | 0.02 | 0.02 |
| | [0.02] | [0.03] | [0.02] | [0.03] |
| 2012 dummy×$Treat_i$ | 0.03 | 0.03 | 0.04 | 0.04 |
| | [0.03] | [0.03] | [0.03] | [0.03] |
| 2013 dummy×$Treat_i$ | 0.06** | 0.05 | 0.07** | 0.06 |
| | [0.03] | [0.04] | [0.03] | [0.04] |
| 2014 dummy×$Treat_i$ | 0.09*** | 0.08** | 0.10*** | 0.09*** |
| | [0.02] | [0.03] | [0.02] | [0.03] |
| 2015 dummy×$Treat_i$ | 0.13*** | 0.11*** | 0.13*** | 0.12*** |
| | [0.02] | [0.03] | [0.02] | [0.03] |
| 2016 dummy×$Treat_i$ | 0.22*** | 0.21*** | 0.23*** | 0.22*** |
| | [0.04] | [0.04] | [0.04] | [0.04] |
| 2017 dummy×$Treat_i$ | 0.21*** | 0.22*** | 0.22*** | 0.23*** |
| | [0.05] | [0.05] | [0.05] | [0.05] |
| 2018 dummy×$Treat_i$ | 0.24*** | 0.28*** | 0.25*** | 0.28*** |
| | [0.05] | [0.05] | [0.05] | [0.05] |
| 2019 dummy×$Treat_i$ | 0.38*** | 0.32*** | 0.39*** | 0.33*** |
| | [0.05] | [0.05] | [0.05] | [0.05] |
| Ring [-10, 0)×$Post_t$ | | | 0.09** | 0.04 |
| | | | [0.04] | [0.03] |
| Ring [-20, -10)×$Post_t$ | | | 0.05** | 0.05** |
| | | | [0.02] | [0.02] |
| Ring [-30, -20)×$Post_t$ | | | 0.05 | 0.08*** |
| | | | [0.03] | [0.03] |
| Observations | 23,306 | 23,302 | 23,306 | 23,302 |
| R-squared | 0.943 | 0.922 | 0.943 | 0.922 |

*Notes*: Columns 1 and 2 present the DID estimates reported in Figure 5 (Panels A and B). Columns 3 and 4 repeat columns 1 and 2 but additionally control for dummies capturing the spillover effects. All regressions include the year- and county-fixed effects and the two climatic control variables. Standard errors reported in square brackets are clustered at the county level. Significance levels are *** p < 0.01, ** p < 0.05, and * p < 0.1.

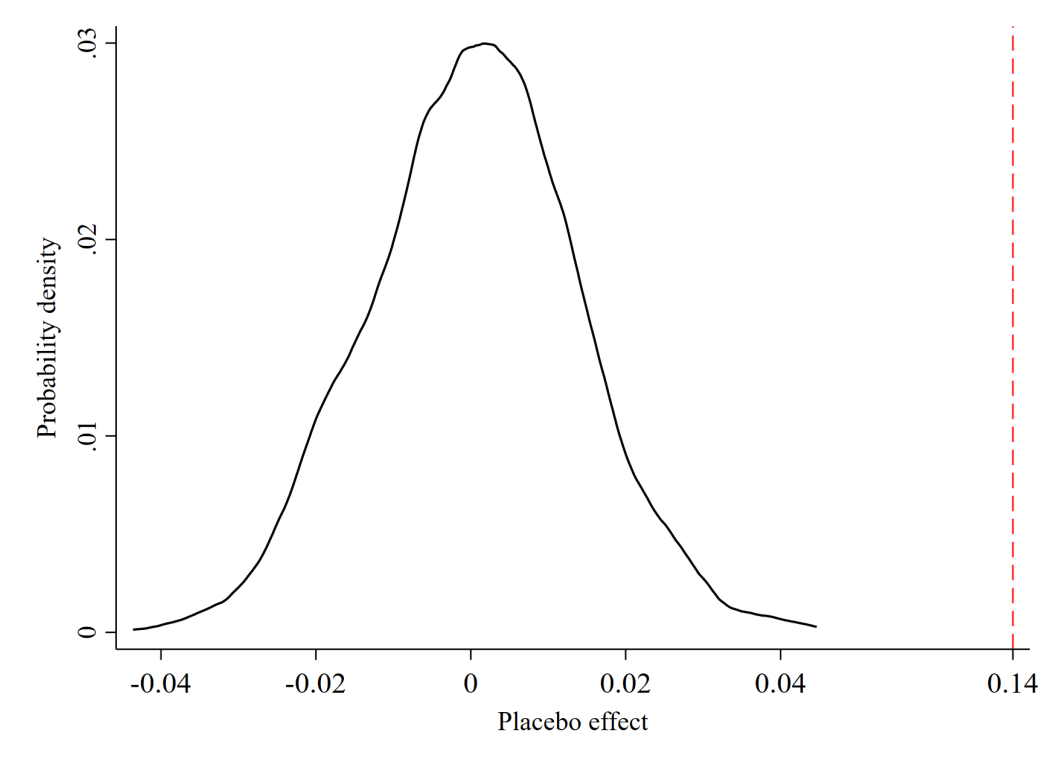


**Figure A.4:** Distribution of Placebo Effects

*Notes*: The figure presents the kernel density plot of the distribution of 1000 placebo estimates based on the model 2. The estimate from the actual data is indicated by the vertical red dashed line at 0.14. The scale of the red line has been adjusted to make the figure look more compact.

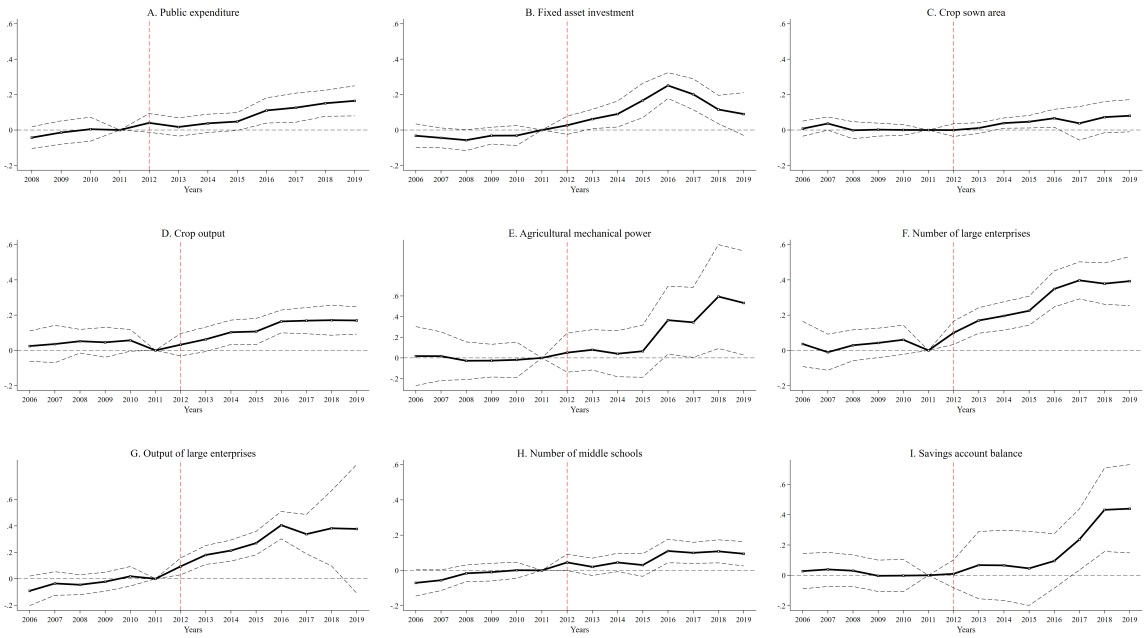


**Figure A.5:** Even study of the nine significant mechanism variables examined in Table 2

*Notes*: This figure presents the estimates of $\beta_k$ and the corresponding 95% confidence intervals estimated based on versions of the modified model 1 that use each of the nine significant mechanism variables examined in Table 2 as the dependent variable. The confidence intervals are computed from standard errors clustered at the county level. Panel A starts from 2008 due to missing data before that.

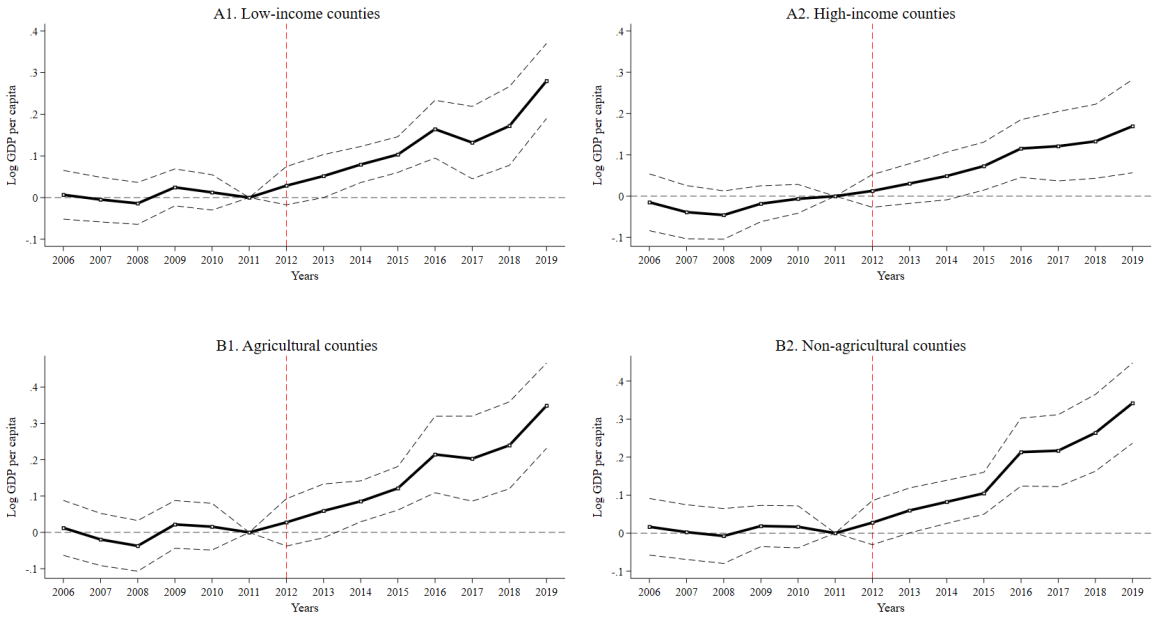


**Figure A.6:** Dynamic impact of the policy on log GDP per capita for different county groups

*Notes*: This figure replicates Panel A of Figure 5 using data from sub-samples. Panels A1 and A2 focus respectively on rich and poor counties defined by their 2011 GDP per capita, and Panels B1 and B2 focus respectively on agricultural and non-agricultural counties defined by their 2011 agricultural share in GDP. The definitions of these county groups are detailed in Footnote 16.

## 8.3 DID-RD estimates

**Table A.5:** Heterogeneity (RD-DID) (Dependent variable: Log GDP per capita)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | $Index_i$ | | | | | |
| | County-level GDP per capita in 2011 (thousand yuan) | County-level share of agricultural in GDP in 2011 | Number of counties in each CDA | Total population in each CDA (10 million) | Total GDP in each CDA (10 billion yuan) | Rate of mis-targeting of each CDA |
| $Treat_i \times Post_t$ | 0.1356*** | 0.1499*** | 0.1555*** | 0.1275*** | 0.1321*** | 0.1371*** |
| | [0.0251] | [0.0262] | [0.0252] | [0.0257] | [0.0257] | [0.0257] |
| $Treat_i \times Post_t \times Index_i$ | -0.0085*** | 0.1358 | 0.0035*** | 0.0495*** | 0.0036*** | -0.0036** |
| | [0.0027] | [0.1539] | [0.0009] | [0.0177] | [0.0012] | [0.0017] |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| County-fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 13,561 | 13,582 | 13,582 | 13,582 | 13,582 | 13,582 |
| R-squared | 0.939 | 0.939 | 0.940 | 0.939 | 0.939 | 0.939 |

*Notes*: This table replicates Table 3 by using the DID-RD estimation with the optimal bandwidth of 100km. Standard errors reported in square brackets are clustered at the county level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

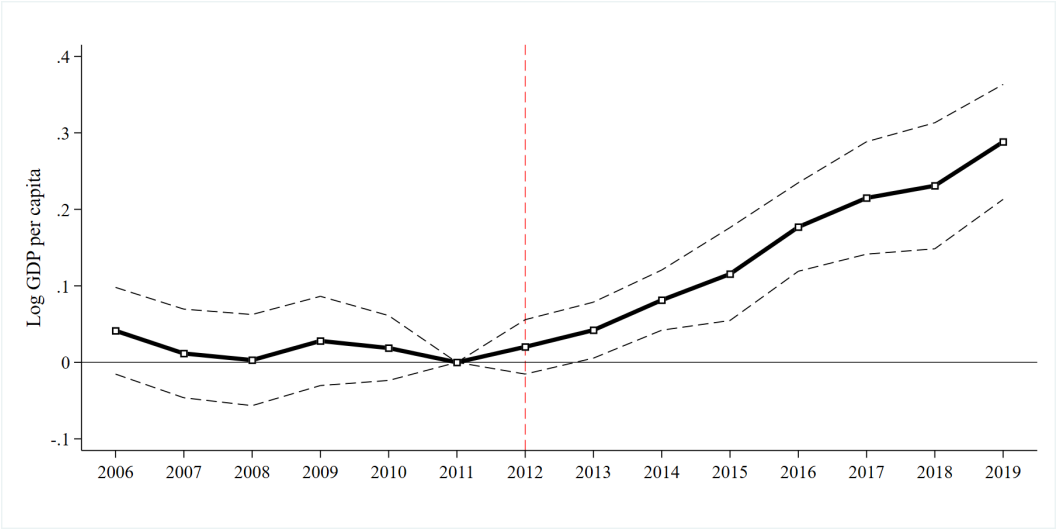


**Figure A.7:** Robustness of the DID-RD estimates

*Notes*: This figure presents the DID-RD estimates of the impact of the policy on log GDP per capita, based on a version of the model 3 that replaces the running variable $Dist_i$ by the third-order polynomial in the latitude and longitude of each county's centroid. The dashed vertical line shows the first year of the policy. The confidence intervals are computed from standard errors clustered at the county level.

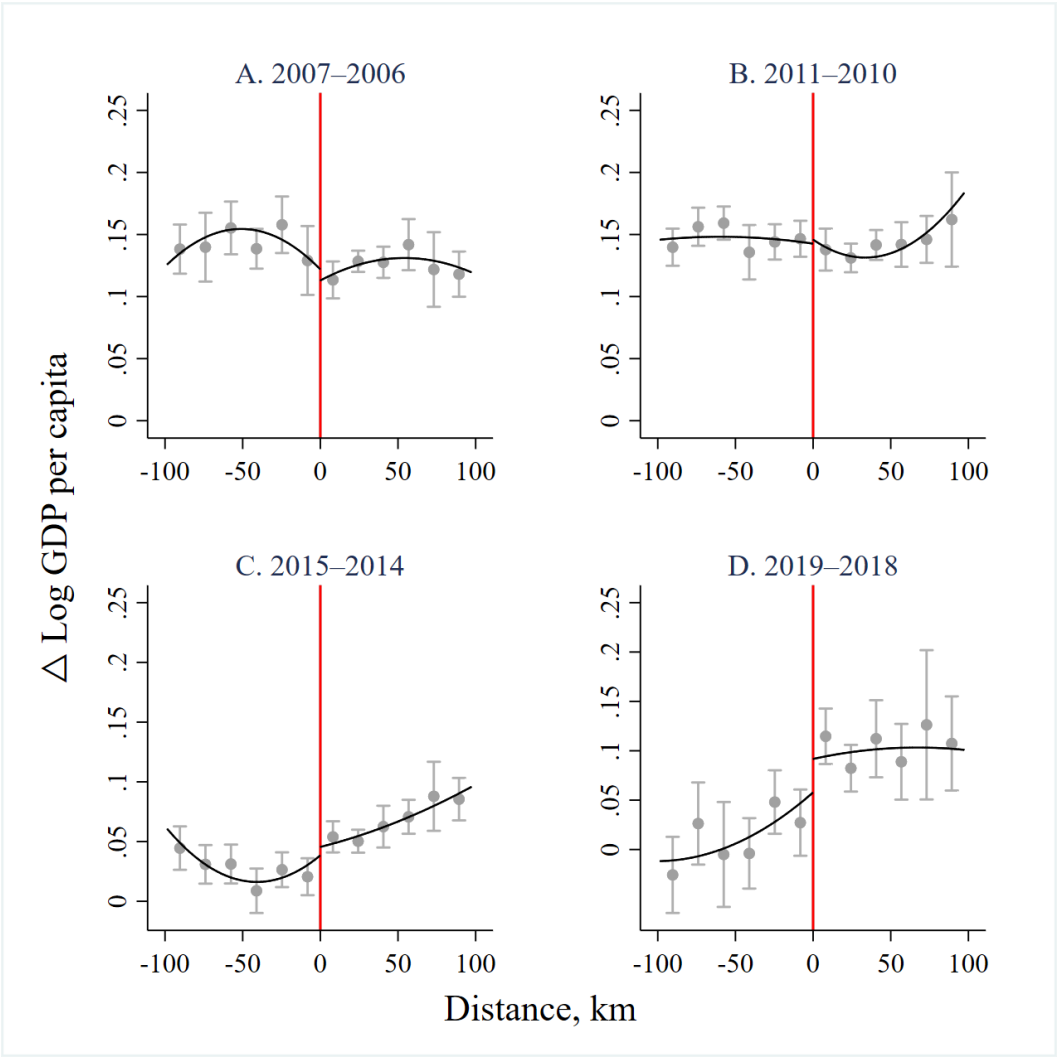


**Figure A.8:** Local continuity before and after the policy

*Notes*: This figure plots the first difference of log GDP per capita for representative years for counties within 100km of the border of each CDA. For example, Panel A plots the difference in log GDP per capita between 2007 and 2006.

**Table A.6:** Dynamic impact of the policy (DID-RD estimates)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | DID-RD estimates | | DID-RD estimates, control for spillovers | |
| | Log GDP per capita | Log agricultural GDP per capita | Log GDP per capita | Log agricultural GDP per capita |
| 2006 dummy×$Treat_i$ | 0.04 | 0.03 | 0.04 | 0.03 |
| | [0.04] | [0.05] | [0.04] | [0.05] |
| 2007 dummy×$Treat_i$ | 0.01 | 0.02 | 0.01 | 0.02 |
| | [0.04] | [0.05] | [0.04] | [0.05] |
| 2008 dummy×$Treat_i$ | -0.00 | 0.02 | -0.00 | 0.02 |
| | [0.04] | [0.05] | [0.04] | [0.05] |
| 2009 dummy×$Treat_i$ | 0.03 | 0.01 | 0.03 | 0.01 |
| | [0.03] | [0.05] | [0.03] | [0.05] |
| 2010 dummy×$Treat_i$ | 0.02 | 0.01 | 0.02 | 0.01 |
| | [0.03] | [0.06] | [0.03] | [0.05] |
| 2012 dummy×$Treat_i$ | 0.02 | 0.02 | 0.07* | 0.05 |
| | [0.04] | [0.05] | [0.04] | [0.05] |
| 2013 dummy×$Treat_i$ | 0.05* | 0.03 | 0.10*** | 0.06 |
| | [0.03] | [0.05] | [0.04] | [0.05] |
| 2014 dummy×$Treat_i$ | 0.09*** | 0.05 | 0.14*** | 0.08* |
| | [0.03] | [0.05] | [0.03] | [0.05] |
| 2015 dummy×$Treat_i$ | 0.14*** | 0.09** | 0.19*** | 0.12*** |
| | [0.03] | [0.04] | [0.04] | [0.05] |
| 2016 dummy×$Treat_i$ | 0.23*** | 0.17** | 0.28*** | 0.20*** |
| | [0.05] | [0.07] | [0.06] | [0.07] |
| 2017 dummy×$Treat_i$ | 0.25*** | 0.19** | 0.30*** | 0.22*** |
| | [0.06] | [0.07] | [0.06] | [0.08] |
| 2018 dummy×$Treat_i$ | 0.28*** | 0.22*** | 0.32*** | 0.25*** |
| | [0.06] | [0.08] | [0.06] | [0.08] |
| 2019 dummy×$Treat_i$ | 0.32*** | 0.21*** | 0.37*** | 0.24*** |
| | [0.06] | [0.08] | [0.06] | [0.08] |
| Ring [-10, 0)×$Post_t$ | | | 0.08* | 0.04 |
| | | | [0.05] | [0.04] |
| Ring [-20, -10)×$Post_t$ | | | 0.08** | 0.05 |
| | | | [0.04] | [0.04] |
| Ring [-30, -20)×$Post_t$ | | | 0.09** | 0.04 |
| | | | [0.04] | [0.03] |
| Observations | 13,610 | 13,617 | 13,610 | 13,617 |
| R-squared | 0.943 | 0.912 | 0.943 | 0.912 |

*Notes*: Columns 1 and 2 present the DID-RD estimates reported in Figures 6 and A.7. Columns 3 and 4 repeat columns 1 and 2 but additionally control for dummies capturing the spillover effects. All regression includes the year- and county-fixed effects and the two climatic control variables. Standard errors reported in square brackets are clustered at the county level. Significance levels are *** p < 0.01, ** p < 0.05, and * p < 0.1.