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Regional Poverty Targeting in China*

Albert Park
Department of Economics
University of Michigan

Sangui Wang
Institute of Agricultural Economics
Chinese Academy of Agricultural Sciences

Guobao Wu
Rural Development Institute
Chinese Academy of Social Sciences

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Abstract

We evaluate the effectiveness of regional targeting in China's large-scale poverty alleviation program begun in 1986 by analyzing a panel data set of all counties in China for the period 1981-95. Estimates of models of poor county designation and poverty fund allocation and newly defined *targeting gap* and *targeting error* measures show that political factors have affected targeting and that leakage has increased while coverage has improved. Only one of the three main programs is progressive. Growth model estimates find that poor county designation increased incomes per capita by 2.28 percent per year during 1985-92 and 0.91 percent during 1992-95. These results are relatively robust to redefining control groups using propensity-score matching methods.

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*Correspondence author is Albert Park, Department of Economics, University of Michigan, Ann Arbor, MI 48109-1220, telephone: 734-764-2363, fax: 734-764-2769, email: alpark@umich.edu.

1. Introduction

Recent research suggests that spatial factors may reduce household consumption of the poor independently of household characteristics such as education, family size, land-holding, assets, and susceptibility to economic shocks (Ravallion and Jalan, 1996; Ravallion and Wodon, 1999). Gallup and Sachs (1999) argue that geographic features strongly predict differences in level of economic development both among and within countries. In many countries, certain geographic regions have exceptionally high poverty incidence. The importance of location to poverty outcomes when labor and other factors are not fully mobile may justify targeting poor areas rather than poor individuals.[1] Ravallion and Jalan (1999) find evidence of geographic poverty traps in rural China, suggesting that the marginal product of own capital decreases with own capital but increases with respect to geographic capital.

Governments have responded with regionally targeted programs whose goal is to promote economic development through public investments (e.g., through budgetary grants, targeted loans, integrated rural development projects) rather than provide direct consumption subsidies.[2] Such programs must demonstrate success in effectively targeting poor areas and improving the well-being of rural households if they are to justify their cost. Unfortunately, "comprehensive evaluations of targeted interventions are sorely lacking" (Squire, 1995).[3] Such assessments are critical for informing public spending choices that seek to promote fundamental objectives of equity and efficiency.

In this paper, we examine the large-scale poverty alleviation program initiated by the Chinese government in 1986. As the largest regionally targeted anti-poverty program in the developing world, the Chinese case deserves attention both for its own sake and to glean insights into the merits and limitations of regionally targeted programs. We analyze a unique panel data set of all counties in China for the period 1981-95 to study the

[1] Regional targeting also could be justified when the goal is individual targeting if saved administrative costs compensate for the "roughness" of targeting (Besley and Kanbur, 1993).
[2] Elsewhere in Asia, Indonesia has programs targeted at villages, *kecamatan* (level above village), and provincial levels. Indian states have pursued different packages of interventions (Ravallion and Datt, 1996).
[3] Ravallion (1993) looks at the potential of regional targeting through budgetary grants in Indonesia, but does not assess impacts, and Jalan and Ravallion (1998) estimate the effect of China's poverty programs on

success of China's poverty program in targeting poor areas and increasing rural incomes. To evaluate targeting, we estimate models of the determinants of poor county designation and poverty fund allocation and calculate newly defined *targeting gap* and *targeting error* measures. To measure impact, we estimate a 4-period income growth model, in which identification is facilitated by the availability of data before and after "treatment" for both treatment and control groups. The results allow us to arrive at a rough estimate of the rate of return on poverty investments. We also test for income convergence and spillover effects in targeting. To address concerns that the sample includes rich counties that are not comparable to poor counties, and which may be subject to different time-varying unobservables, we test the robustness of our estimates to altering the control sample using propensity score matching methods recently popularized in the program evaluation literature (Heckman, Ichimura, and Todd, 1997; Dehejia and Wahba, 1998). We find relatively small differences in estimates using matched control groups.

We find that political factors have influenced the selection of poor counties, targeting has deteriorated over time, and leakage has increased while coverage has improved. Initial designations favored minority and revolutionary base areas, were not equitable across provinces, and were affected by lobbying efforts. Despite poverty reductions, the government added new poor counties but because of strong political resistance did not take away poor county status from counties that were no longer poor. For two of the three main poverty investment programs the amount of fund allocation to poor counties is not significantly correlated with income levels (one program is progressive). Finally, we find that poor county designation increased growth in rural income per capita by 2.28 percent per year during the period 1985-92 and 0.91 percent during the period 1992-95. These estimates are relatively robust to altering the comparison group of nonpoor counties using propensity score matching. The estimated rate of return on poverty investments was 15.5 percent in the former period and 11.6 percent in the latter. We also find evidence of spillover effects when neighboring counties have higher incomes but not if they have access to targeted programs. There is no evidence of a relationship between targeting success and investment return.

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expenditure growth (but do not look at targeting) using household data from 4 southern provinces in the late

The rest of the paper is organized as follows. After briefly introducing China's poverty alleviation programs, in section 2, we assess the targeting effectiveness of poor county designations. Section 3 examines fund allocation under the three programs among poor counties. In section 4, we estimate the impact of the programs and other factors on rural income growth and test for income and targeting spillover effects as well as a connection between targeting and impact. We also estimate impacts using propensity score matching methods. Section 5 concludes.

China's War on Poverty

In 1986, the Chinese government established the inter-ministerial Leading Group for Economic Development in Poor Areas (LGEDPA) to oversee an ambitious program to eliminate rural poverty. Government leaders were well aware that growing inequities accompanying economic reforms could lead to social and political instability. In 1993, the government announced the *baqi* (8-7) plan to lift the remaining 80 million rural poor out of poverty by the year 2000 (within 7 years).

China's three main targeted poverty investment programs are a subsidized loan program (*tiexi daikuan*) administered by the Leading Group's Poor Area Development Office and the Agricultural Development Bank, a public works program called Food-for-Work (*yigong daizhen*) run by the State Planning Commission, and a budgetary grant program (*fazhan zijin*) managed by the Ministry of Finance. During the period 1991-95, most subsidized loans went to industry (56 percent) and agriculture (35 percent). Food-for-Work funds went almost entirely to infrastructure (75 percent), and development capital was relatively evenly spread across different uses (agriculture—38 percent, infrastructure—26, industry—18, and education and health—12). Funds for these programs are allocated primarily to officially designated poor counties--currently 592 counties, or 28 percent of all county-level administrative districts in China. In 1996, the programs provided 11.6 billion yuan (or $1.4 billion), an amount equal to five percent of central government expenditures, 1.5 percent of total government expenditures (including local governments), and over 5 percent of rural household income in poor counties (Table

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1980s. See also van de Valle and Nead, eds. (1995).

1).[4] Funding increased sharply to 16.1 billion yuan in 1997. These amounts do not include funds provided to poor counties by local governments, international organizations, private donors, or through other government programs.[5] The goal of the programs has been not only to transfer resources to the poor, but also to promote economic development and income growth in order to lift the poor out of poverty permanently.

Since this war on poverty began in the mid-1980s, the number of rural residents officially classified as poor has fallen significantly, from 131 million in 1986 to 65 million in 1995 to 40 million in 1999. While these reductions are impressive, it is difficult to identify how much of the decrease was due to targeted programs and how much was due to broad-based rural income growth. Poverty reductions were even more dramatic in the early 1980s when China's rural economy grew rapidly following agricultural price increases and the introduction of the Household Responsibility System which returned production decision-making authority to households. The number of poor fell from 250 million in 1978 to 128 million in 1984, a reduction of historic magnitude. In contrast, poverty reductions during the late 1980s were much more modest as rural incomes stagnated, despite the creation of the Leading Group.[6] Greater poverty reductions resumed in the 1990s as rural income growth increased and the government put greater emphasis on anti-poverty efforts. Separating the effects of poverty programs and other factors in explaining economic outcomes in poor areas is a main goal of this

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[4] 1996 funding levels are representative of funding levels in other years (Table 1). In 1986, for example, funding equalled 4.2 (1.7) percent of central (total) government spending. Subsidies for loans are financed by the government budget, but loans are financed by the People's Bank of China through the Agricultural Development Bank (formerly the Agricultural Bank of China). Budgetary costs are 53 percent of funds assuming that the budgetary cost for loans or revolving funds is the difference between official loan interest rates and subsidized rates for 3 years. Pricing loans based on the real social opportunity cost of capital estimated at 10 percent (above inflation), the social cost of the program is estimated to be 68 percent of funds.

[5] Additional budgetary grants are discussed below. International poverty funds were at least 2.5 billion yuan in 1996 according to data from the Office of the Leading Group for Poor Area Development. Direct assistance to poor counties by 10 government bureaus (China had 44 bureaus at the time) was estimated at 3.3 billion yuan per year in the early 1990s (Xie, 1994). Other initiatives include partnership programs with rich provinces, as well as with local government agencies, and quasi-government nonprofit organizations such as Project Hope, which supports primary education. Some of these are described in *Ten Years' Implementation of China's Poverty Alleviation Plan* (1996).

[6] Estimates by the World Bank (1992) reveal similar trends as official statistics but suggest a greater slowing down of poverty reductions in the late 1980s.

paper. Because of lack of data on poverty incidence within counties, we focus not on poverty reductions per se, but on changes in rural incomes per capita in poor and nonpoor counties. Our focus on incomes overlooks potentially important non-income benefits, and our use of aggregate county data limits our ability to draw definitive welfare implications (see van de Valle (1998) for a review of measurement and identification issues in assessing welfare impacts of public spending).

2. Poor County Designation

Selection of Poor Counties

In 1986, a standard was necessary for determining which counties would receive the new poverty alleviation funds. The Leading Group initially adopted a mixed set of poverty lines to choose poor counties. The basic standard for selecting nationally designated poor counties was rural net income per capita below 150 yuan in 1985. However, a higher poverty line of 200 yuan was applied to counties in old revolutionary base areas (*laoqu*) and counties with large minority populations (*minzu xian*). For some counties in very important revolutionary base areas and for a few minority counties in Inner Mongolia, Xinjiang, and Qinghai, the poverty line was raised to as high as 300 yuan. Poor counties were chosen based on income per capita data for 1985 collected by the Ministry of Agriculture.

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 (Zhou and Gao, 1993). By 1988, the number of poor counties had reached 328.[7] Provinces designated additional counties as poor, supporting them with their own, usually limited funds.[8] By 1988, 370 counties had been designated as provincial poor counties. Three counties in Hainan Province were added to the list of

[7] In 1987, 13 counties in old revolutionary base areas and two other counties were added to the list. In 1988, 27 pastoral and semi-pastoral counties also were designated as national poor counties. Poverty standards for counties in pastoral regions were based on income data from 1984 to 1986. All pastoral counties whose average net income per capita was below 300 yuan and semi-pastoral counties whose average net income was below 200 yuan were designated as national poor counties. Also included as poor counties were those in the *sanxi* region (three prefectures in Gansu and Ningxia) which had been receiving special funds from the central government since the early 1980s.

national poor counties in 1989 when Hainan (an island) was separated from coastal Guangdong Province. Subsequently, there were no major changes in the list of poor counties until 1993. During this time, strong complaints were heard from poor counties that had not been designated as national poor counties, which finally led the Leading Group to adjust both the poverty lines and poor county designations in 1993. The Office of the Leading Group charged the State Statistical Bureau (SSB) to carry out new poverty calculations.

Based on the new poverty line, 326 counties reportedly qualified as poor. However, the government found it politically difficult to eliminate counties that already poor county designations, as county leaders vigorously fought to maintain access to targeted funds. Although the official poverty count had decreased from 125 million in 1985 to 80 million in 1993, the adjustment increased the number of nationally designated poor counties from 331 to 592 (Table 2).

Still, the 1993 poor county designations were considered an improvement because they were based on a poverty line determined by the SSB based on nutritional requirements and included many poor counties that had been neglected in the earlier designations. In a number of poorer provinces, many provincial poor counties became nationally designated poor counties. Yunnan and Guizhou, very poor provinces in southwest China, Inner Mongolia, and Hebei benefited most from the adjustments (Table 2). The proportion of the rural population in these four provinces residing in poor counties increased by more than 20 percent (and as high as 40 percent in Yunnan). Fujian, Guangdong, Shandong and Zhejiang—among China's richest provinces, were net losers.

Targeting Issues

Given the nature of the poor county selection process, a number of factors are likely to affect the targeting accuracy of China's poverty programs. First, political criteria were frequently mixed with poverty alleviation goals. In addition to stated preferential treatment for minority counties and old revolutionary base areas, political appeals by

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[8] The income standards for provincial poor county designation were less uniform and higher than that used for national poor county designation.

individual counties sometimes affected designations.[9] Once a county was designated, it became almost impossible politically to remove designations even when the county was no longer poor. Second, efforts to maintain balance among provinces in the number of designated poor counties may have hurt provinces in which the vast majority of counties were poor, especially in the initial designations. Third, initial designations were based on one year of data, so that designations as well as measures of targeting accuracy each year will be affected by random income variation. Fourth, national poverty lines do not allow for regional differences in the cost of living.[10] Chen and Ravallion (1996), for example, estimated that the cost of purchasing the SSB food bundle was 23 percent higher in Guangdong than in Guangxi in the late 1980s.

There are also inherent limitations associated with the administrative level of targeting which lead to incomplete coverage and leakage. Jalan and Ravallion (1998) report that about half of the poor in four southern provinces did not live in poor counties in the late 1980s. A 1988 national survey of over 10,000 rural households in 28 provinces found that only 37 percent of poor households were located in officially designated poor counties (Riskin, 1994). Although the official poverty count decreased from 125 million in 1985 to 50 million in 1997, with the new poor county designations in 1993, the rural population in national poor counties increased from 106 million to 199 million. Even if all of China's poor were located in poor counties, the great majority of households in poor counties are not poor. To make matters worse, China's poverty programs have been criticized for poor targeting at the sub-county level. Local officials have strong incentives to support revenue-generating industrial projects that do not necessarily maximize profits or reach the poor even though funds targeted at rural

[9] Authors interviews in Henan, Jiangxi, Guizhou, and Sichuan in 1996.

[10] There are several criticisms about how the national poverty line is determined. First, the food bundle excludes harmful consumption goods (alcohol, tobacco) even though poor households do, in fact, spend money on these items. Second, valuation of own produced goods at planned prices (before 1990) or weighted prices including planned prices (after 1990) in measuring incomes and constructing poverty lines underestimates their true value. This biases incomes downward more for the poor, whose consumption tends to depend more on own-produced goods. Third, the cost of living index used to adjust the poverty line for different years is a Paasche index, and so understates increases in the cost of living. Furthermore, the index is for average consumption, and so understates the poor population's share of grain in food expenditures and share of food in total expenditures. The direction of resulting bias depends on how fast grain and food prices rise relative to other prices. We thank Carl Riskin for pointing out the biases in the Paasche index.

households make a greater contribution to growth than industrial projects (Rozelle et al., 1998).. Bank managers have little incentive to pursue targeting goals in administering subsidized loans.

Empirical Analysis of the Determinants of Poor County Designation

Initial evidence on targeting can be found in the frequency distributions of poor and nonpoor counties across income levels.[11] In 1986, only half of the counties in the lowest income decile were designated as poor, even though there were even more counties designated as poor in the next income group (Figure 1). In 1993, many fewer counties in the lowest income groups were being excluded--better coverage, but more counties designated as poor were in the middle income groups--greater leakage (Figure 1).

We expect that status as a minority or revolutionary base county will have a significant effect on poor county designation. In 1990, 637 counties in China were minority counties (33 percent) and 195 were revolutionary base areas (10 percent). In our sample, 20 percent of minority counties and 44 percent of revolutionary base counties were designated as poor in 1986, accounting for 38 and 30 percent of all poor counties.[12] In 1993, the number of minority counties designated as poor more than doubled (to 46 percent of all minority counties) but the number of revolutionary base counties increased only slightly (to 48 percent).

We study the determination of poor county status by estimating probit functions for poor county designations in 1986 and 1993. Explanatory variables include log of income per capita, log of grain production per capita, and industrial share of total income in the year preceding the designations, status as a minority county or revolutionary base county, and provincial dummy variables. We use county-level economic data from the

[11] These distributions illustrate the incidence of targeting, a focus of many studies of targeting (e.g., Grosh (1995).

[12] We restrict the sample to counties that have not changed administrative boundaries and for which data is relatively complete. For the targeting gap calculations below, the sample includes 1837, or over 95 percent of counties in China in 1991, the year in which the number of counties was at its lowest.

Ministry of Agriculture, which were the basis of poor county designations in 1986.[13] All explanatory variables have estimated coefficients that are statistically significant. The fitted probabilities correctly predict the status of 92 percent of county designations in 1986 and 88 percent in 1993.

Based on the estimation results, we calculate marginal effects on the probability of poor county designation at the sample means for poor counties (Table 3). In 1986, a 1 percent increase in income per capita reduces the probability of being designated a poor county by 1.3 percent, a 1 percent increase in grain output per capita decreases the probability by 0.2 percent, and an increase in the industrial share of income of 1 percent reduces the probability by 0.7 percent. Designations are less responsive to per capita income and grain production in 1993 (1.1 and 0.1 percent) and slightly more responsive to industrial share of income (0.8 percent). Being a minority or revolutionary base county increases the probability of designation by 15 and 45 percent in 1986, and 17 and 18 percent in 1993.

Many provincial dummies have large and significant coefficients, suggesting that there was considerable discrimination against specific provinces. In the 1986 designations, poor provinces in Sichuan, Guizhou, Yunnan (southwest), Inner Mongolia, Henan, Hunan (central), and Gansu (northwest) were at a severe disadvantage, while counties in the wealthier provinces of Fujian, Shandong, Hubei, or Xinjiang were much more likely to be designated as poor. In 1993, despite a large number of newly designated counties in relatively disadvantaged provinces such as Yunnan and Guizhou, southwest provinces remained at a distinct disadvantage, along with Qinghai and Ningxia in the northwest and Anhui and Hunan in central China.[14]

Targeting Gaps

To evaluate overall targeting effectiveness, we define new measures which we refer to as *targeting gaps* and *targeting errors. Targeting gaps* describe mistargeting in

[13] The MOA data is known to show more poverty in China's southwest and less in the northwest in comparison to the SSB data (World Bank, 1992). Both SSB and MOA data are available for poor counties in 1994 and 1995. The two series have a rank correlation of 0.89 and 0.92 in the two years.

[14] Part of the measured bias against southwest provinces may be due to biases in the MOA versus SSB data. However, interviewed officials in Beijing confirmed that the number of poor counties in the poorest provinces was limited to preserve balance among provinces.

the full sample with respect to a reference poverty line, while *targeting error* describes mistargeting given a set number of targeted beneficiaries. Similar to poverty measures, gaps and errors can be aggregated using different weights.

We define two types of targeting gaps: the *targeting count gap* (TCG_t) and the *targeting income gap* (TIG_t). The targeting count gap is defined as

$$TCG_t = \frac{1}{N}\sum_{i=1}^{N}\{I_{it1}(P_{it} = 0, Y_{it} < Z_t) + I_{it2}(P_{it} = 1, Y_{it} > Z_t)\}. \tag{1}$$

Here, N is the total sample of counties, indexed by i. I_{it1} is an indicator variable for type I error (or incompleteness) that equals one if a county is not designated as poor (P_{it}=0) but its income per capita (Y_{it}) is below the poverty line (Z_t). I_{it2} is an indicator variable for type II error (or leakage) that equals one if a county is designated as poor (NP_{it}=1) but its income per capita is above the poverty line. TCG_t can be interpreted as the percentage of counties that are mistargeted, and is easily disaggregated into type I and type II error. These two types of errors are described as F-mistakes and E-mistakes in the similar measures developed by Cornia and Stewart (1995). Although we have aggregated the two into a single measure using equal weights, one can look at the two types of mistargeting separately or assign different social weights to type 1 and type 2 errors. Based on rough welfare calculations, Cornia and Stewart (1995) argue that type 1 error should have a substantially greater weight than type 2 errors even though it is the latter that is usually the focus of evaluations of targeted programs.

The targeting income gap is defined as

$$TIG_t = \frac{1}{N}\sum_{i=1}^{N}\{(Z_t - Y_{it})I_{it1} + (Y_{it} - Z_t)I_{it2}\}. \tag{2}$$

It is similar to the TCG except that mistargeting is weighted by the magnitude of mistargeting, measured as the difference between income and the poverty line. The TCG and TIG are analagous to the widely used poverty headcount and poverty gap measures, but are two-sided rather than one-sided. Just as for poverty measures, one can give greater weight to larger targeting income gaps by using higher order weighting terms (Foster, Greer, and Thorbecke, 1984). Targeting gaps have both a behavioral and welfare interpretation. Since policy makers make decisions on *county* designations, targeting gaps measure the accuracy of these decisions (behavioral). If the poverty count gap is weighted by county population, it measures the percentage of poor population missed or nonpoor population included (assuming populations within counties have the same income).

One problem with the targeting gap measures is that they are sensitive to the number of poor counties designated. If the number of designations is less than the number of truly poor counties, type I error is unavoidable, and if designations exceed the number of poor counties, type II error is unavoidable, even when targeting is perfect in that designations go to the poorest counties. Another way to assess targeting, then, is to compare outcomes with the perfect targeting case given the number of poor county designations. We define *targeting count error* (TCE) as the percentage of designations not given to counties that would be targeted under this definition of perfect targeting, or

$$\mathrm{TCE}_t = \frac{1}{D}\sum_{i=1}^{N} I_{it}(Y_{it} < Z_t^*, P_{it} = 0)\,. \tag{3}$$

Here, Z_t^* is the income level of the marginal, or threshold, county when targeting is perfect given the number of available designations (D). Similar to targeting gaps, we can weight the indicator functions by income differences with counties that were mistakenly targeted to calculate *targeting income error* (TIE_t) or by rank differences to calculate *targeting rank error* (TRE_t).[15]

We present yearly TCG and TCE measures for China's poor county designation in Table 4. Other targeting gap and targeting error measures tell a similar story. The TCG is sensitive to the chosen poverty line; as the line is increased type I error increases and

[15] Targeting income error formula is the same as for targeting income gap except the poverty line Z is the income of the threshold county and the summation is divided by D instead of N. Targeting rank error replaces income difference with income rank difference.

type II error decreases. We calculate the TCG for two different lines--the official poverty line and a relative poverty line equal to 60 percent of mean income per capita.

As measured by TCG or other targeting gap measures, targeting effectiveness has deteriorated steadily over time. From 1986 to 1995, the percentage of counties that were mis-targeted increased from 14 to 22 percent using the official poverty line and from 15 to 19 percent using the relative poverty line (or 12 to 19 and 14 to 17 percent using population-weighted measures). In 1986, failure to designate a poor county as poor was nearly twice as likely as designating a nonpoor county as poor (using either the official or relative poverty lines). But after gradual decline, type I error almost disappeared (incomplete coverage) using the official poverty line and fell substantially using the relative line following the new poor county designations in 1993. In that same year, type II error doubled, so that the overall TCG jumped noticeably.[16] Overall, incompleteness has fallen while leakage has increased. We calculate a targeting income gap (TIG) of 77 yuan in 1995 using the official poverty line (5 percent of total rural income), of which only a fraction (0.2) is type 1 error. Given that about one fifth of counties are mis-targeted, the average magnitude of "leakage" in mistargeted counties is about 350 yuan, or two thirds of the official poverty line.[17] Because of the low official line in 1995, mean income in all counties could be brought up to the poverty line with a transfer of only 0.25 percent of total rural income (compared to 3 percent in 1986), much less than total poverty spending.

Although the targeting count error (TCE) was substantial in the original designations (in fact, a majority of designations were mistargeted) and increased steadily over time, unlike the TCG, the TCE fell dramatically after new designations in 1993, even reaching levels below that of the original designations. Thus, the 1993 designations reduced targeting error, but through a strategy of expanded coverage beneficial to counties above the absolute or relative poverty thresholds.

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[16] The patterns are similar but even more striking using the targeting income gaps (not reported).

[17] Only part of the targeting gaps can be explained by preferential treatment towards minority and revolutionary base counties. In 1986, 25 percent of leakage (type II error) in the TCG (using the official poverty line) was due to minority counties and 35 percent to revolutionary base counties. By 1995, the comparable figures were 35 and 19 percent.

3. Fund Allocation

Using data on funding amounts by county for the years 1994-96, we examine the allocation of funds under the three programs.[18] Since the programs are administered by different agencies, we do not necessarily expect the determinants of fund allocation to be the same across programs. County funding amounts in the three programs have relatively low correlation coefficients.[19]

From a simple plot of average funding levels for the three programs during 1994-96 against income per capita, it is obvious that there is not a strong relationship between funding levels and income per capita (Figure 2).[20] The nonparametric estimate reveals a very weak inverse relationship. We test the extent to which average county funding amounts for the period 1994-96 can be explained by initial period characteristics (the same variables as in the poor county designation probits). For each program, we estimate specifications with and without provincial dummies (Table 5). We find that only development capital funds are clearly progressive with respect to income per capita. For both subsidized loans and Food-for-Work funds, we find slightly negative but statistically insignificant coefficients on income per capita without provincial dummies, and positive (and still insignificant) coefficients when provincial dummies are included. This suggests that within provinces, richer counties get more funds, whether because they have greater political influence or higher returns. In contrast, development capital is highly progressive, whether or not provincial dummies are included, consistent with the use of such funds to compensate for overall budgetary shortfalls that are correlated with low incomes. For loans and FFW, there is stronger evidence that fund allocations are inversely related to grain production per capita and industrial development, while neither of these variables enters significantly for development capital. That allocations respond more to grain production and industrialization suggests that these variables proxy better

[18] For subsidized loans, data is available only for loans outstanding at the end of the year rather than new loans, which reflects both new and earlier funding levels, the average duration of loans, and repayment levels. In general new loans are about one third of outstanding loans based on national and provincial data.
[19] Correlation coefficients are 0.18 for FFW and development capital, 0.35 for subsidized loans and FFW, and 0.50 for subsidized loans and development capital.
[20] We have added average outstanding loans directly to the average funding for FFW and development capital. This gives disproportionate weight to the subsidized loan component of poverty funds, and also

for infrastructure and funding needs that are the basis of allocation decisons. Finally, our results show that minority counties get strong preference in all 3 programs, and that revolutionary base areas are favored in the loan and development capital programs but not in the FFW program.

4. Program Impact

Patterns of Rural Income Growth

Rural income growth in China during the reform period has varied greatly over time and in poor versus nonpoor counties.[21] Dividing the reform era into three periods--1981-85, 1985-92, and 1992-95, we find that in the first period annual growth in income per capita in all counties was 24.5 percent in the first period, -0.7 percent in the second period, and 9.6 percent in the third period. In the second period (immediately following the establishment of the poverty alleviation program), income in poor counties grew faster than in nonpoor counties (2.1 percent versus -1.3 percent per year).[22] However, in the third period, poor county incomes grew relatively slowly--7.8 percent per year compared to 10.2 percent in nonpoor counties.

Empirical Specification

The growth in county i's rural income per capita (Y) from period t-τ to time t is modeled as a function of the county's status as a designated poor county made at the beginning of the period ($P_{it-\tau}$), initial income per capita ($Y_{it-\tau}$), other initial characteristics ($X_{it-\tau}$), county time-invariant characteristics (γ_i), and prefectural time-varying factors (λ_{pt}). The specification implicitly assumes that poor county designation is not endogenous to time-varying unobservables that differ within prefectures and are not correlated with initial characteristics. In the main specification, the sole X variable is grain production per capita, a commonly used poverty indicator in China. The error term consists of other

may introduce a bias towards progressivity if lower incomes correlate with lower repayment and thus higher outstanding loans.

[21] Growth rates for each county are calculated from spline regressions of log income on a time trend. The periods correspond both to obvious breaks in overall rural income trends in China as well as periods of different poor county designations.

[22] Tong et al (1994) also found that poor counties grew faster than nonpoor counties in the mid-1980s.

time-varying unobservables and measurement error that are assumed to be uncorrelated with the regressors:

$$\log Y_{it} - \log Y_{it-\tau} = \beta_{1d} P_{it-\tau} + \beta_2 \log Y_{it-\tau} + \beta_3 \log X_{it-\tau} + \gamma_i + \sum_P \lambda_{pt} + e_{it} \tag{4}$$

A panel is constructed from data for each county for four time periods: 1981-85, 1985-89, 1989-1992, and 1992-95. The first period predates the poverty program, the first poor county designations occurred during the second and third periods, and new designations were made during the fourth period. Information on growth rates before the poverty program began makes it possible to identify the effects of poor county status while also controlling, through county fixed effects, for unobservables that have persistent effects on growth. This also eliminates potential bias from the endogeneity of poor county designation to county unobservables that are time-invariant.

To implement the fixed effects, first we rewrite the last equation as follows:

$$y_{it} = \beta_{1d} P_{it-\tau} + (1+\beta_2) y_{it-\tau} + \beta_3 x_{it-\tau} + \gamma_i + \sum_P \lambda_{pt} + e_{it} \tag{5}$$

Small *y* and *x* denote logs, and the "~" superscript denotes differences from regional means, where regions can be defined as prefectures or provinces. In the sample, there are about 10 prefectures per province. We allow for the effect of the poverty program to be different for the period of original designations (1985-1992), captured by β_{11}, and the period of new designations (1992-95), captured by β_{12}.[23] Imposing these restrictions, controlling for region-time effects, and implementing county fixed effects by taking first differences[24] yields a system of 3 equations:

[23] We assume that impacts are the same within each designation period 1985-89 and 1989-92. Funding in real yuan did not differ greatly during this period (Table 1), nor did the specific features of the poverty alleviation programs. When we do allow for separate effects in the second and third sub-periods, the results are very similar.

[24] One could also implement fixed effects by subtracting means (within estimator), but this invalidates the chosen instrumental variables (below) whose exogeneity depends upon their being predetermined.

$$
\begin{aligned}
\tilde{y}_{i2} - \tilde{y}_{i1} &= \beta_{11}\tilde{P}_{i1} + (1+\beta_2)(\tilde{y}_{i1} - \tilde{y}_{i0}) + \beta_3(\tilde{x}_{i1} - \tilde{x}_{i0}) + (e_{i2} - e_{i1}) \\
\tilde{y}_{i3} - \tilde{y}_{i2} &= (1+\beta_2)(\tilde{y}_{i2} - \tilde{y}_{i1}) + \beta_3(\tilde{x}_{i2} - \tilde{x}_{i1}) + (e_{i3} - e_{i2}) \qquad (6) \\
\tilde{y}_{i4} - \tilde{y}_{i3} &= \beta_{12}\tilde{P}_{i3} - \beta_{11}\tilde{P}_{i2} + (1+\beta_2)(\tilde{y}_{i3} - \tilde{y}_{i2}) + \beta_3(\tilde{x}_{i3} - \tilde{x}_{i2}) + (e_{i4} - e_{i3})
\end{aligned}
$$

Here we replace t with explicit time subscripts (0=1981, 1=1985, 2=1989, 3=1992, 4=1995).[25] The coefficients on the poverty status variables should be interpreted as the effect of the poverty program on counties in the same prefecture in the same period with the same starting income and grain production levels and controlling for time-invariant unobservables.

This specification raises several estimation issues. First, the error terms are correlated across equations because of common error components as well as possible serial correlation, and are likely to have different variances because the dependent variables are specified for different periods of time (3 or 4 years). Second, the lagged income changes included as regressors are correlated with the error term because of common income level components with the dependent variable, creating an endogeneity problem.

To deal with these concerns, we estimate the three equations simultaneously using an iterative feasible 3SLS procedure, imposing appropriate cross-equation restrictions and using different instruments for the three equations.[26] The estimator can also be derived as a GMM estimator based on the exclusion restrictions. The instruments are lagged variables for income, grain production, and poverty status, and vary by equation because they are plausibly exogenous only when predetermined (Casselli, Esquivel, and Lefort, 1996). Thus, the instruments for equation one are values in period 0, for equation two values in periods 0 and 1, and for equation three values for periods 0, 1, and 2.

[25] Because the first two periods are four years and the last two are three years, we interpolate the three year growth rates to four years and make an adjustment to the third equation to account for the fact that the lagged difference is only three years. Reported estimates and standard errors are adjusted to reflect the effect on annual growth rates.

[26] The estimation procedure is programmed in GAUSS and the program is available from the authors. The initial coefficients are instrumented GLS estimates based on the assumption that the e_{it} are independent and have magnitudes proportional to the years of growth being explained.

This specification is similar to those used in the recent literature in empirical macroeconomics which tests for convergence using cross-country panel data (Barro, 1997; Casselli, Esquivel, and Lefort, 1996). As a byproduct, our estimates provide evidence on growth convergence among counties in China. Although similar in spirit, our specification differs from Jalan and Ravallion (1998) who study household consumption changes using 6 years of household panel data. We model county-level growth over multiple years as a function of initial conditions while they model annual household consumption as an autoregressive distributed lag process--AD(1,1), which allows for greater divergence from steady state growth, but includes more endogenous variables that must be instrumented with lagged values. Since our data aggregates thousands of households and spans multiple years (and spans the period of new designations after 1993), unusual divergence from steady state growth should be less of a problem.

Barro (1997) points out that differenced regressions intended to net out fixed effects may produce estimates that are more sensitive to measurement error and which do not exploit cross-sectional variation. Fixed effects also preclude testing the effects of some county characteristics of interest, such as minority or revolutionary base status. We thus estimate a specification without fixed effects (in levels rather than differences). However, to use lagged values as instruments requires us to drop the initial period (1981-85). This does enable us to include initial industrial share of income, for which data in 1981 is unavailable, as an additional regressor.

Additional Tests

Spillovers. We test the importance of two potentially important spillover effects from nearby counties that may affect our assessment of optimal targeting and impact—those due to their income level and poverty designation status. In a regression controlling for provincial, time-varying effects, we include variables for average initial income of other counties in the prefecture and the percentage of other counties in the prefecture designated as poor.

Targeting and Efficiency. It is possible that tradeoffs exist between targeting and efficiency. If the returns to investment (or poor county status) are lower in poorer counties, this could provide a rationale for provincial governments to intentionally mistarget. Alternatively, returns could be higher in poor counties that are more capital constrained. To test the relationship empirically, we allow program effects to vary by province, redefining the program effect in designation period d for province p (β_{1dp}) to be a function of provincial targeting success (T_{dp}), after controlling for province and time effects:

$$\beta_{1dp} = \beta_{1d} + \beta_{1p} + \beta_1 TRE_{dp} \tag{7}$$

We use provincial measures of the targeting rank error described earlier. Identification comes from relative changes in targeting success within provinces following new poverty designations in 1993. Improvement was greatest in poor provinces in northwest and southwest China that received the most new designations.

Impact of the Poverty Alleviation Program

Estimates of the impact of China's poverty program on rural income growth are presented in Table 8. Before turning to those results, we briefly describe results from simpler specifications intended to illustrate overall trends. An OLS regression of growth rates on time period dummies and program dummies reveals that poor counties grew 3.1 percent faster than nonpoor counties during 1985-92 and 2.5 percent less than nonpoor counties in 1992-95. If we add county fixed effects, the effect of being a poor county *increases* to 6.2 and zero percent in the two periods, although it is unlikely that all of this effect is attributable to the poverty programs rather than other factors affected poor and nonpoor counties differently. For example, institutional reforms fueled much of the growth in the early period (1981-85), likely with larger marginal returns in more productive areas. However, in the late 1980s while large grain-producers were hurt by lower marginal procurement prices, poor counties in remote regions diversified their cropping patterns and activities and benefitted from new market niches for products in which they had a relative comparative advantage (Tong et al, 1994). In the most recent period, differences in the development of rural industries likely played a greater role in rural income growth, favoring richer areas. More generally, measured effects on growth could be a byproduct of differences in regional growth patterns, if for instance provinces with more poor counties tended to grow slowly in the early period and faster in period 2.

Table 6 reports results for specifications that do a better job accounting for time-varying unobservables by controlling for prefecture-time effects and county initial conditions. In the sample, there are about 10 counties per prefecture. The preferred differenced 3SLS specification with prefecture-time controls finds that the poverty program increased rural income growth by 2.28 percent during 1985-92 and 0.91 percent during 1992-95. As seen in column two, without instrumenting the effects are somewhat smaller (1.80 and 0.77 percent). These effects are larger than those found by Ravallion and Jyotna (1998) who find that living in a national poor county increases consumption by 1.1 percent per year during 1985 to 1990 among households in officially designated poor counties in four southern provinces (Guizhou, Yunnan, Guangxi, and Guangdong). Without fixed effects (column 3), the effect of the poverty program is negative in both periods, although not statistically significant in the second period. One explanation for the different results is that counties with unobservables deleterious to growth are more likely to be designated as poor. Alternatively, the program's impact could be exaggerated in the differenced regressions if changes over time are benefitting poor counties, such as if poverty designations are going to counties with improved political connections which also facilitate growth, or if there is reporting bias associated with being a poor county.

Unfortunately, data does not permit us to separately estimate the extent to which specific programs affect income growth. We have data on county fund allocations only for the years 1994-96, and given the shortness of the panel, it is impossible to properly control for unobserved heterogeneity and time-varying factors. Despite these reservations, we estimate a model of third-period growth as a function of average funding levels during 1994-96, including provincial dummies and initial period economic variables (3), as well as minority and revolutionary base status. We find no significant effect of poverty alleviaton funds, except for a slight negative effect for subsidized loans.

We provide the first county-level estimates of income convergence in China using a national sample of counties, informing a disputed issue in the China growth literature (Ravallion and Jalan, 1999). The coefficient on lagged income in the preferred specification is –0.236, which suggests a fast rate of income convergence. A comparison with column 2 shows that without instrumenting, the coefficient is biased downward as expected. Consistent with a convergence story, higher grain production per capita also leads to slower growth, although the magnitude is much smaller. However, the level regression (column 3) shows that greater industrial development leads to faster growth (and divergence); a ten percent increase in industrial share of income increases income growth by 0.7 percent. Also, minority counties grow 4 percent slower and revolutionary base areas 2 percent slower than other counties.

Finally, we consider spillover effects and the relationship between targeting and investment return. We find positive income spillover effects that are small but precisely estimated (column 4). A 10 percent increase in the income per capita of neighboring counties increases income growth by 0.1 percent. This provides slight justification for giving less priority to poor counties located in more prosperous regions. The estimated effect of provincial targeting success on program impact is close to zero and not statistically significant (not reported), so that we find no support for a systematic relationship between targeting and investment return. It could be that the lack of targeting success is more a reflection of rent-seeking than efficiency considerations, or that the relationship is obscured by other factors.

Impact Estimates Using Propensity-Score Matching

In the growth model specification, the program variable is treated as exogenous. Although differencing allows us to sweep out the effect of time-invariant unobservables that may cause endogeneity, the possibility of bias from time-varying unobservables remains. Hundreds of counties in the sample are too rich to be considered for poor county designations, and factors affecting growth in such counties in a given period might differ systematically from those affecting growth in poor counties. An unrepresentative control group also may bias coefficient estimates of other regressors if true coefficients differ in rich and poor counties, leading to possibly bias in program impact estimates.

A direct solution to endogeneity bias is to find an instrument for the treatment variable. However, lacking a convincing time-varying instrumental variable, we instead use a propensity-score matching method to construct control groups for each treatment period that have observable characteristics comparable to the treated group. The identifying assumption is that time-varying unobservables not correlated with observables are unimportant, so that comparable control groups will yield reliable estimates of program impact (Heckman, Ichimura, and Todd, 1997). We adapt the matching algorithm of Dehejia and Wahba (1998), matching with replacement a control observation for each treated observation based on propensity scores which are fitted probabilities from probit estimates of poor county status.[27] Because nonpoor counties in the lowest propensity score range do not match well to poor counties in the same range in terms of the means of covariates (the two samples do not share the same support), and because the lack of nonpoor counties with extremely high propensity scores leads to excessive matching of some control counties to treated observations (39 matches for one county for the 1985-92 period and 154 matches in the 1992-95 period), we define a "trimmed" sample that excludes all counties with extremely low or high propensity scores, defined as propensity score ranges in which the percent of poor counties is less than 10 percent or greater than 90 percent. For both the matched and trimmed samples in each period, we calculate three measures of program impact—the difference in mean annual growth rates between treatment and control groups, a regression estimate of program impact during the period (an instrumented cross-sectional regression), and a 3SLS differenced estimate using data from all periods (specification 1 in Table 6), a parametric version of the difference-in-difference matching estimator proposed by Heckman, Ichimura, and Todd (1997).

These results, as well as descriptive statistics for covariates in the treatment and control groups, are presented in Table 7. For 1985-92, the matched and trimmed control groups are similar in their resemblance to the treated sample, but in 1992-95 the trimmed sample is a much closer match, mainly because of the lack of control observations for treated observations with very high propensity scores, which leads to excessive reliance on a single control observation (with 154 matches). This also explains why impact

[27] We estimate probits with interaction and squared terms (for continuous variables) and verify that this produces comparison groups in each propensity score strata (subrange) whose mean characteristics are comparable to the treated groups in the same strata.

estimates from level regression differs so much from the difference in mean growth rates for the matched sample, but much less so for the trimmed sample.

There are two important results in Table 7. First, earlier impact estimates from the preferred differenced growth model specification are relatively robust to the use of more carefully chosen control samples. The preferred estimates using the trimmed samples find impacts of 2.48 and 0.66 percent for 1985-92 and 1992-95, compared to 2.29 and 0.91 percent using the full samples. Second, even with matched control samples, the differenced specification produces impact estimates substantially higher than level regression estimates, implying that time-invariant unobservables bias impact estimates even when treated and control groups have identical observables. The panel data has allowed us to test directly the assumption of the propensity score methods, with a negative finding similar to that of Heckman, Ichimura, and Todd (1997) who study the impact of U.S. job training programs. The result reinforces the strengths of the identification strategy in the original growth model specification, which appears robust to time-varying unobservables that are correlated with observables.

Rate of Return

Based on our measurement of program impact on rural income growth, it is possible to estimate the rate of return on poverty investments. We assume that poor county designation increased rural income growth by 2.28 percent during 1985-1992 and 0.9 percent during 1992-95. In real terms, poverty spending during 1985-92 fell and then recovered to about its initial level, averaging 9.5 billion yuan per year (in 1995 yuan), equivalent to 89 yuan per person or 14 percent of rural income.[28] Based on the 2.28 percent impact on incomes, the poverty program on average increased rural income by 13.8 yuan per person per year. This suggests a rate of return of 15.5 percent, somewhat higher than the 12 percent estimated by Jalan and Ravallion (1998). For the 1992-95 period, the rate of return is still 11.6 percent despite increased spending and smaller program effects, because the approximate doubling of the program's coverage reduced spending per capita to 55 yuan.

[28] Calculations are based on the following assumptions: rural population in poor counties, 106.43 million in 1988 (Table 3); rural population growth rate, 0.7 percent per year, equal to the national average over this period; rural income per capita in poor counties, 568 yuan in 1985, growing by 2.1 percent per year.

Discussion

Our estimates of program impact are open to different interpretations. Critics will argue that performance was much worse than we describe, because we do not account for all expenditures—we exclude administrative costs of the programs (estimated to be 4-16 percent of program costs for 5 geographically targeted programs in Latin America (Grosh, 1995), matching or supplementary funds provided by local governments, relent poverty loans, international donor funds, and funds from a vast array of government and private initiatives. Some argue that the total of such spending is greater than official poverty alleviation funds (Xie, 1994). Thus, our estimates of positive impact on incomes could overstate the rate of return on poverty investments by more than 100 percent.

Second, indirect evidence of low repayment rates on subsidized loans (about 50 percent in the early 1990s) and suspected substitution effects make the relatively high rate of return surprising. Third, it is possible that some funds are being diverted to direct consumption which is showing up as income, leading us to overstate investment returns. This explanation implies that gains depend upon continued support. Fourth, differenced regressions remain subject to bias from time-varying unobservables that disproportionately benefit poor counties within the same prefecture. Finally, our results provide no evidence on the distribution of benefits within counties, so high impacts do not necessarily benefit the poor within poor counties.

Other factors, however, may bias our estimates downward. First, if targeted programs also benefit poor counties not designated as poor, then leakage may dilute the measured impact on targeted counties even though the absolute effects are large. This is also true if provincial governments substitute budgetary allocations away from counties supported by national poverty alleviation funds, or initiate programs targeted at poor counties not designated as poor. Also, if consumed funds are being consumed directly and not being reported as income, benefits may be greater than suggested by the impact on income.

To get a better sense of whether poverty funds follow or crowd out other investment funds, we include the log of average county government expenditures per capita for 1994-95 (the only year with available data) in the probits for poor county designation in 1993 and the regressions for poverty funding allocations for 1994-96. A one percent increase in budgetary expenditures per capita reduces the likelihood of designation by 0.05 percent (standard error 0.03). Thus, designated poor counties have fewer budgetary funds than non-designated counties *ceteris parabis*, pointing to slight selection or substitution effects that should lead to downward bias in program impacts. However, other types of investment funds may be allocated quite differently than budgetary funds. We also find that among poor counties greater allocations of all three types of funds are associated with higher budgetary expenditures.

Poorer relative performance in 1992-95 is consistent with our knowledge of aspects of program implementation. The pattern of spending on subsidized loans shifted away from agriculture (households) toward industry (firms and intermediary organizations), despite the greater return to the former (Rozelle et al., 1998). The budgetary crisis in poor counties became acute beginning in the early 1990s and worsened over time, increasing the incentive to divert investment funds to pay for recurrent expenditures (Park et al., 1996). On the other hand, benefits of Food-for-Work infrastructure (a program without significant funding until the early 1990s) may take more time to be realized, so that the lack of program impact for the most recent period may be premature.

5. Conclusion

Since 1986, the Chinese government has pursued one of the most ambitious efforts ever to eradicate rural poverty, investing billions of dollars in regionally targeted investment projects. Unfortunately, over time both the accuracy of targeting and the measured impact of the programs on rural income growth have deteriorated. Nonetheless, the Chinese government significantly increased funding for existing programs in 1997 in a bid to eliminate poverty by the end of the millenium.

This paper highlights some of the concerns that accompany regionally targeted programs. First, the political economy of targeting can strongly influence the accuracy of targeting. This was true for China’s initial poor county designations, which included explicit political criteria (minority, revolutionary base status), was not equitable across provinces, and was subject to lobbying efforts. Later, beneficiaries fought to protect their interests, leading to the decision to greatly expand coverage and increase targeting error. Second, allocation of funds among designated counties was not progressive, which can be interpreted either as targeting failure or a sensible tradeoff between targeting and other concerns. However, we did not find a significant relationship between targeting success and investment return, suggesting that lack of progressivity may be driven by political rather than economic factors.

Third, our assessment of program impacts finds modest positive effects on rural income growth, supporting the potential of targeted programs to contribute to economic development in poor counties. However, there are good reasons to view this result with caution. First, we do not know to what extent gains have benefited the poor, who probably account for less than 20 percent of the population in poor counties. Second, our estimate of the rate of return is subject to error because we are unsure of the true amount of investment being made in poor counties. Third, assessments of specific poverty programs, notably the subsidized loan program, have been highly critical and merit attention. Future research to shed more light on these questions will be of great value, as will work that better quantifies the tradeoffs in selecting the optimal administrative level for regional targeting.

The Chinese experience confirms the view that regional targeting may be a rather “blunt instrument” for reaching the poor (Ravallion and Lipton, 1995). Combined with the finding by Ravallion (1993) that Indonesia’s pattern of regional disbursements is poorly targeted, the evidence presented here suggests that political constraints are likely to undermine regionally targeted programs when the level of targeting is at the county level or higher. In China, there has been discussion of targeting townships rather than counties, and at least one province (Yunnan) has taken the initiative to target in this way.

We have assumed that perfect targeting is the goal of targeted programs, but it is worth pointing out that if tradeoffs exist between targeting and other social objectives, optimal targeting may not be perfect. Social weights on type I and type II error in targeting also may be different, so that our equally weighted targeting gap measures need not correspond directly with “successful” targeting.

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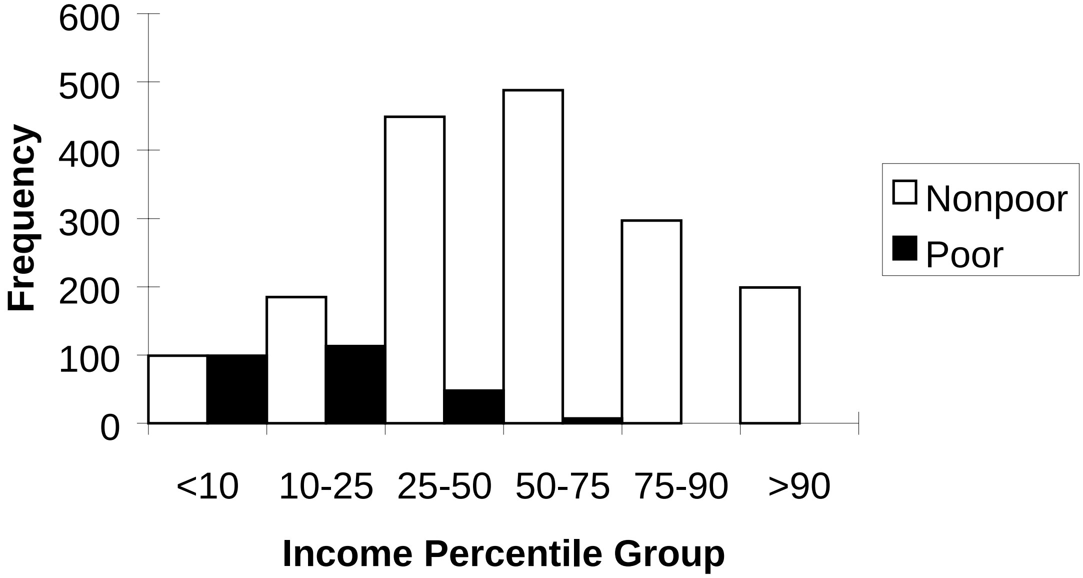
Figure 1
County Income Per Capita Distribution in Poor and Nonpoor Counties, 1986 and 1993

1986



1993

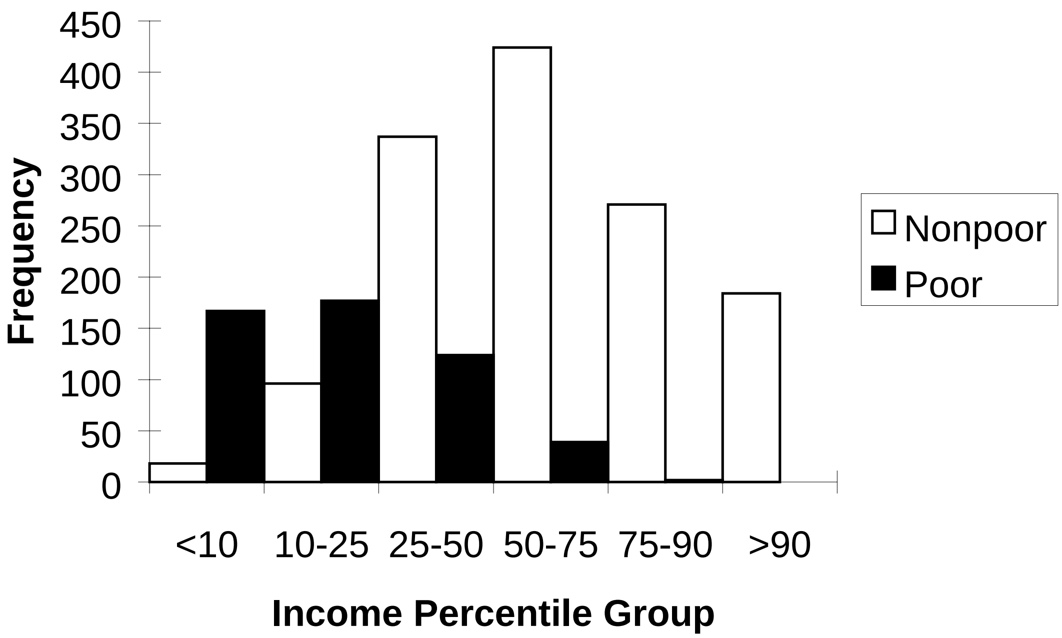


Figure 2
Poverty Alleviation Funds (PAF) Per Capita and Income Per Capita, County Means, 1994-1996

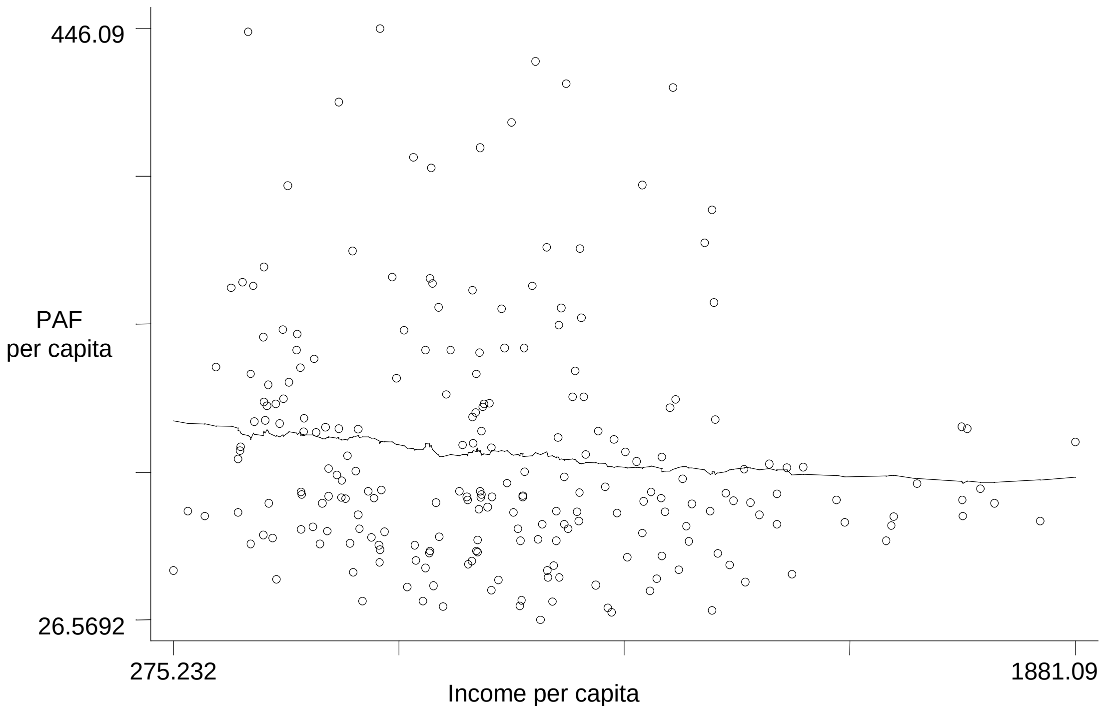


Table 1
China's Central Government Poverty Alleviation Funds, 1986-97
(billion yuan)

Year	**Loans Total**	PASL	LSBQ	SOE	PAL	Pastoral	**Budgetary Grants Total**	Development Capital	Education	Sanxi	Revolving Fund	**Food for Work Total**	**Nominal Total**	**Real Total** 1997 yuan
1986	**2.30**	1.00	1.00		0.30		**1.000**	0.800		0.20		**0.90**	4.20	11.78
1987	**2.30**	1.00	1.00		0.30		**1.000**	0.800		0.20		**0.90**	4.20	10.98
1988	**3.05**	1.00	1.00	0.70	0.30	0.05	**1.112**	0.800		0.20	0.112		4.16	9.18
1989	**3.05**	1.00	1.00	0.70	0.30	0.05	**1.112**	0.800		0.20	0.112	**0.10**	4.26	7.98
1990	**3.05**	1.00	1.00	0.70	0.30	0.05	**1.112**	0.800		0.20	0.112	**0.60**	4.76	8.74
1991	**3.55**	1.50	1.00	0.70	0.30	0.05	**1.112**	0.800		0.20	0.112	**1.80**	6.46	11.52
1992	**3.55**	1.50	1.00	0.70	0.30	0.05	**1.212**	0.900		0.20	0.112	**1.60**	6.36	10.76
1993	**3.55**	1.50	1.00	0.70	0.30	0.05	**1.272**	0.960		0.20	0.112	**3.00**	7.82	11.69
1994	**4.55**	2.50	1.00	0.70	0.30	0.05	**1.312**	1.000		0.20	0.112	**4.00**	9.86	12.11
1995	**4.55**	2.50	1.00	0.70	0.30	0.05	**2.027**	1.115	0.60	0.20	0.112	**4.00**	10.58	11.31
1996	**5.50**	3.45	1.00	0.70	0.30	0.05	**2.127**	1.215	0.60	0.20	0.112	**4.00**	11.63	11.72
1997	**8.50**	6.45	1.00	0.70	0.30	0.05	**3.627**	2.715	0.60	0.20	0.112	**4.00**	16.13	16.13
Total	**47.50**	24.40	12.00	7.00	3.60	0.50	**18.025**	12.705	1.80	2.40	1.120	**24.90**	**90.43**	**133.90**

Sources: OLGEDPA (1989), Piazza and Liang (1997), Jiang and Gao (1997), Li (1997)
PAL=poverty alleviation loans at regular interest through Agricultural Bank of China
LSBQ=subsidized loans for revolutionary base (*lao*), minority (*shao*), remote (*bian*), and poor (*qiong*) areas
PASL=poverty alleviation subsidized loans through the Agricultural Bank of China (Agricultural Development Bank since 1994)
SOE=subsidized loans for county state-owned enterprises in poor counties
*real values deflated by national retail price index

Table 2
National and Provincial Poor Counties, 1988 and 1993

Province	National Poor Counties 1988		Provincial Poor Counties 1988		National Poor Counties 1993			
	Number	Percent of Provincial Rural Pop	Number	Percent of Provincial Rural Pop	Number	Percent of Provincial Rural Pop	Rural Pop. in Poor Counties	Percent of Pop in Poor Counties
North								
Hebei	14	9.4	35	21.5	39	31.23	16.6	8.33
Henan	15	11.7	9	7.8	28	21.96	16.8	8.42
Shandong	9	9.9	5	4.4	10	9.42	6.8	3.39
Northeast								
Liaoning	3	6.9	8	13.4	9	15.41	3.5	1.73
Jilin			11	15.2	5	5.84	0.9	0.43
Heilongjiang			6	9.0	11	12.14	2.2	1.13
Northwest								
Inner Mongolia	16	23.9	24	34.8	31	47.86	6.8	3.43
Shanxi	14	13.8	21	11.6	35	26.15	5.9	2.96
Shaanxi	34	27.4	12	13.9	50	43.77	12	6
Ningxia	8	53.5			8	55.81	2	1
Gansu	31	47.5	12	16	41	62.1	11.9	5.96
Qinghai	10	36.3	10	48.7	14	43.49	1.4	0.69
Xinjiang	17	20.1	13	26.3	25	35.75	3	1.52
Yangtze River								
Zhejiang	3	2.3			3	2.29	0.8	0.41
Anhui	9	14.8	8	11.2	17	31.83	15.6	7.82
Jiangxi	17	23.4	39	44.6	18	25.1	7.9	3.98
Hubei	13	15.1	24	20.6	25	28.25	11.5	5.78
Hunan	8	5.4	20	17.7	10	11.53	6.1	3.07
South								
Fujian	14	19.1	2	1.1	8	8	2.1	1.04
Guangdong	4	4.5	27	20.6	3	1.44	0.8	0.4
Hainan					5	13.78	0.6	0.32
Southwest								
Guangxi	23	18	25	19.5	28	20.02	7.7	3.85
Sichuan	21	12.3	30	18	43	20.58	19.3	9.69
Guizhuo	19	29.6	12	12.5	48	57.48	16.8	8.42
Yunnan	26	20.5	15	11.9	73	61.05	20.1	10.1
Tibet					5	10.58	0.2	0.1
Total	**328**	**12.6**	**370**	**13.9**	**592**	**23.49**	**199.2**	**100**

Source: Calculated from data in Office of the Leading Group for Economic Development in Poor Areas, *Outlines of Economic Development in China's Poor Areas*, (Beijing : Agricultural Press), 1989, and State Statistical Bureau, *China Rural Economic Statistics by County, 1980-1987*, (Beijing: Statistical Press), 1989.

Table 3
Marginal Effects on Probability of Poor County Designation
(From Probits Evaluated at Poor County Means)

	1986	1993
Log(income per capita) (t-1)	-1.31	-1.13
	(0.0749)	(0.0526)
Log(grain output per capita) (t-1)	-0.216	-0.124
	(0.0509)	(0.0270)
Industrial share of income (t-1)	-0.705	-0.769
	(0.308)	(0.135)
Minority	0.146	0.166
	(0.0633)	(0.0377)
Revolutionary base	0.441	0.180
	(0.0411)	(0.0255)
Provincial dummies:		
North		
Henan	-0.240	-0.138
Shandong	0.392	-0.111
Northeast		
Liaoning	0.175	0.0882
Jilin		0.0309
Heilongjiang		0.0381
Northwest		
Inner Mongolia	-0.136	0.0140
Shanxi	0.282	-0.00751
Shaanxi	0.126	0.00762
Ningxia		-0.369
Gansu	-0.302	0.00431
Qinghai	0.343	-0.297
Xinjiang	0.363	-0.0626
Yangtze River		
Zhejiang	0.0834	-0.194
Anhui	0.244	-0.212
Jiangxi	-0.0426	-0.0474
Hubei	0.347	0.0533
Hunan	-0.182	-0.391
South		
Fujian	0.443	0.0613
Guangdong	0.143	-0.00769
Southwest		
Guangxi	0.0600	-0.129
Sichuan	-0.231	-0.46
Guizhou	-0.219	-0.341
Yunnan	-0.119	-0.320

Notes: Sample sizes are 1908 and 1953 and pseudo R-squared is 0.49 and 0.54. Marginals for minority and revolutionary base status as well as provincial effects are effect of change from 0 to 1. Provincial effects are with respect to Hebei. Marginal effects evaluated at full sample means in 1986 and 1993 are the following: income –0.129 and –0.704, grain –0.0212 and –0.0773, industrial share –0.0689 and –0.481, minority 0.181 and 0.130, and revolutionary base 0.143 and 0.216 (all statistically significant at the 1 percent level).

Table 4
Targeting Count Gap and Targeting Count Error, 1986 to 1995

Year	Targeting Count Gap Official Poverty Line				Targeting Count Gap Relative Poverty Line (60 Percent of Mean Inc. P. C.)				Target-ing Count Error
	Line	Type I	Type II	Total	Line	Type I	Type II	Total	
1986	213	0.094	0.050	0.144	598	0.099	0.050	0.149	0.524
1987	227	0.082	0.065	0.146	611	0.097	0.061	0.158	0.504
1988	236	0.044	0.101	0.144	586	0.086	0.073	0.159	0.574
1989	259	0.056	0.096	0.152	538	0.096	0.079	0.175	0.625
1990	300*	0.078	0.093	0.171	570	0.093	0.085	0.178	0.649
1991	304	0.058	0.101	0.158	590	0.093	0.084	0.177	0.629
1992	320	0.038	0.107	0.145	628	0.087	0.083	0.171	0.618
1993	350	0.002	0.225	0.227	655	0.028	0.150	0.178	0.280
1994	440	0.005	0.232	0.237	703	0.047	0.137	0.185	0.319
1995	530	0.004	0.218	0.222	793	0.065	0.120	0.185	0.334

*In 1990, self-consumed production valued at weighted purchase prices instead of planned prices for both income and poverty line.
Note: Calculations based on sample of 1837 counties with complete data for all years. No official poverty line was released for 1993.

Table 5
Determinants of Fund Allocations to Poor Counties, 1994-96

	Log(outstanding loans p.c.)		Log(Food for Work p.c.)		Log(development. capital p.c.)	
Log(1992 income p.c.)	-0.02	0.14	-0.14	0.18	-0.63	-0.41
	(0.11)	(0.13)	(0.15)	(0.17)	(0.22)	(0.28)
Log(1992 grain output p.c.)	-0.23	-0.23	-0.15	-0.29	-0.07	0.02
	(0.10)	(0.10)	(0.13)	(0.13)	(0.20)	(0.22)
1992 industrial income	-0.11	-0.75	-0.99	-0.24	-0.69	-0.27
share	(0.29)	(0.32)	(0.40)	(0.42)	(0.62)	(0.72)
minority	0.41	0.45	0.38	0.50	0.56	0.62
	(0.06)	(0.08)	(0.09)	(0.10)	(0.13)	(0.17)
revolutionary base	0.24	0.17	0.09	0.13	0.35	0.46
	(0.08)	(0.08)	(0.10)	(0.11)	(0.16)	(0.18)
provincial dummies	no	Yes	No	yes	no	Yes
N	541	541	526	526	501	501

Notes: Dependent variables are county averages for the years 1994-96. Independent variables are for the year 1992. Counties with no funding levels for FFW and development capital are excluded because there is ambiguity as to whether these are missing values or zeros. Results are not affected much in estimates of other specifications with and without zero values.

Table 6
Estimation Results: China Poverty Program Impact

	1	2	3	4
	3SLS	SUR	3SLS	3SLS
	Differences	Differences		Differences
Poor county (1986-92)	0.0228	0.0180	-0.0178	0.0236
	(0.00298)	(0.00299)	0.00371)	(0.00391)
Poor county (1993-95)	0.00906	0.00767	-0.00598	0.00951
	(0.00353)	(0.00350)	0.00388)	(0.00479)
Log(income per capita) (t-τ)	-0.236	-0.310	-0.288	-0.230
	(0.00475)	(0.00302)	0.00632)	(0.00628)
Log(grain output per capita) (t-τ)	-0.0176	-0.0120	-0.00594	-0.0168
	(0.00261)	(0.00261)	0.00287)	(0.00356)
Industrial share of income (t-τ)			0.0717	
			(0.0120)	
Minority dummy			-0.0401	
			(0.0106)	
Revolutionary base dummy			-0.0198	
			(0.00726)	
Average income in other				0.0118
counties in prefecture(t-τ)				0.00445
Percent of other counties in pref.				0.00275
designated poor (1986-92)				0.00574
Percent of other counties in pref.				0.000303
designated poor (1993-95)				0.00621
Province-time controls				Y
Prefecture-time controls	Y	Y	Y	
Number of equations	3	3	3	3
N	1676	1676	1637	1676

Notes: For specifications 1, 2, and 4, dependent variables are differences in log income, for specification 3, dependent variables are log income for periods 2-4. Province- and prefecture-time controls are accomplished by subracting regional means from all variables before differencing. In iterative 3SLS estimation, lagged differences in log income or lagged log income are treated as endogenous. Instruments for each equation are all predetermined variables in levels, including further lagged income, lagged grain production, and poor county status. Counties lacking complete data are excluded. Sample size for specification 3 is smaller because of missing data on industrial share of income.

Table 7
Estimates of Program Impact and Sample Characteristics Using Control Groups Selected by Propensity-Score Matching

	N	Mean Prop. Score	Max Freq. Of Control	Mean Log (Inc. p.c.)	Mean Log (Grain p.c.)	Mean Ind. Income Share	Percent Rev. Base	Percent Minority	Diff. In Mean Annual Growth	Reg. Coef. (IV, Level)	Reg. Coef. (3SLS, Diff)
1985-92											
Poor-all	253	0.602		6.28	-1.34	0.101	0.324	0.340			
Nonpoor-all	1403	0.073	1	6.82	-1.05	0.163	0.073	0.207	0.023	0.0111	0.0229
Nonpoor-match	101	0.598	39	6.28	-1.23	0.091	0.360	0.419	-0.000	0.0035	0.0227
Poor-trimmed match	177	0.546		6.31	-1.34	0.103	0.243	0.367			
Nonpoor-trimmed match	82	0.545	11	6.34	-1.33	0.094	0.282	0.492	0.005	0.0084	0.0248
1992-95											
Poor-all	473	0.716		6.45	-1.19	0.148	0.180	0.357			
Nonpoor-all	1183	0.118	1	6.98	-0.90	0.264	0.082	0.160	-0.027	-0.0075	0.0091
Nonpoor-match	144	0.704	154	6.49	-0.97	0.152	0.074	0.180	-0.119	-0.0061	0.0099
Poor-trimmed match	209	0.483		6.64	-1.04	0.154	0.177	0.234			
Nonpoor-trimmed match	120	0.483	7	6.72	-1.07	0.167	0.158	0.268	-0.015	-0.0155	0.0066

Notes: Matches are nonpoor counties with closest propensity score, sampled with replacement. Trimmed matches exclude observations in propensity score ranges in which poor counties comprise less than 10 or more than 90 percent of observations, or with propensity scores less than 0.10 (both periods) or greater than 0.90 (0.80) for 1985-92 (1992-95). Means and regressions weight control observations by the number of matches. Level regressions regress growth rate of income per capita on poor county status, log income per capita, log grain production per capita, industrial income share, revolutionary base dummy, and minority county dummy, all expressed as differences from prefectural mean; income per capita is instrumented by lagged covariates from 1980 (1985-92 period) and 1980 and 1985 (1992-95 period). 3SLS differenced estimates are from the 4 period, 3 equation system described in (xx).