Evaluating China's Poverty Alleviation Program: A Regression Discontinuity Approach*

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Abstract

This paper evaluates the impact of 8-7 Plan, the second wave of China's poverty alleviation program, on rural income growth at the county level over the period 1994-2004. Program participation was largely determined by whether a county's pre-program income fell below certain poverty lines. The discreteness of the assignment rule is exploited to obtain convincing estimates of the program effect. Using a panel dataset, we find that the 8-7 Plan resulted in a substantial gain in rural income for the treated counties. The empirical findings also indirectly reveal the important role of initial endowments in economic development.

JEL Classification: H43; H54; I38; O53

1 Introduction

Evidence from cross-country studies suggest that sustained economic growth has typically been poverty reducing (Ravallion and Chen, 1997; Dollar and Kraay, 2002). However, broad-based growth is not always the panacea for curing poverty (Morduch, 2000). Due perhaps to inferior initial conditions, the impoverished people residing in certain regions are unable to fully share the gains from aggregate high growth (Ravallion and Jalan, 1999). As a response to concerns about the lagging poor, public efforts have been taken in many countries in the form of poor area development programs to fight poverty and reduce inequality. Despite the theoretical underpinnings of the strategies pursued, whether these programs worked as intended is mainly an empirical issue.

This paper evaluates the large-scale poverty alleviation program instituted by the Chinese government in 1994. Known as the 8-7 Plan, it aspired to promote local economic development through targeted public investments. The program is impressive in terms of scale and its public outlay. In a bid to lift the majority of the remaining 80 million poor out of poverty by 2000, the program covered a total of 592 counties, or 28 percent of all county-level administrative units in China. Over the course of its seven-year operation, the program cost RMB 1240 billion (USD 14.9 billion equivalent), an annual 5-7 percent of China's central government expenditures. Two prominent features of the program also deserve a mention. First, program assignment was based on an indicator-based targeting scheme. Second, the interventions, which were administered at the county level, intended to promote permanent income growth by supporting productive investment rather than subsidizing consumption.

The 8-7 plan was pre-dated by the first wave of China's poverty program, which is very similar in content but smaller in scale. A modest literature evaluating the first wave interventions suggests that the program was successful in raising income and consumption of people living in the poor counties (Jalan and Ravallion, 1998; Rozelle et al., 1998; Park et al., 2002). As aside, Park et al.(2002) also examines the performance of the 8-7 Plan after its first full year of implementation. It finds that the program increased per capita income growth by 0.91 percent during the period 1992-1995. Albeit an interim evaluation, it is the only study that we are aware of that assesses the effectiveness of the 8-7 Plan. Nevertheless, given the fact that the new wave of program did not come into effect until 1994, an estimate of the program impact of a longer term is needed to judge its overall efficacy.

Esimating program effectiveness is often complicated by the nonrandomness of program placement. This problem is particular relevant when public interventions are targeted based on certain individual characteristics. In our context, if treated counties benefited more from the aggregate economic growth than untreated counties due to geographic differences, a "naive" comparison of gain incomes can lead to an underestimation of the program effect. On the contrary, upward bias could rise when political connections were important. The program effect could also be overstated if those selected into the program had experienced adverse shocks prior to eligibility, relative to those that were not selected. To deal with the endogeneity problem, we implement a regression discontinuity approach that is facilitated by the program's discrete assignment rule. In particular, program placement was largely determined by whether a county's pre-program per capita income fell below certain poverty lines. As such, causal impacts of the program can be gauged by comparing counties clustered just below the dividing line to counties just above.

We make use of the discontinuity of program assignment at the eligibility threshold to construct an instrumental variable for actual treatment status. Employing a panel data set of roughly 1300 Chinese counties over 20 years, our two-stage least squares results shows that the 8-7 Plan had a positive impact on rural income growth. This finding is robust to functional form choices and various sets of control variables. According to our estimates, the program increased 1994-2004 per capita income gains by about 0.45 standard deviation.

The remainder of the paper proceeds as follows. Section 2 provides some background information on the poverty alleviation program. Section 3 illustrates our empirical strategy and Section 4 describes the data used. Section 5 reports the empirical results and Section 6 concludes.

2 Background on the Program

In response to concerns about the lagging poor, the Chinese central government launched an ambitious anti-poverty program in the mid-1980's. An interministeral Leading Group for Poverty Reduction (henthforth the Leading Group) was founded in 1986 to supervise and coordinate the implementation of the entire program. Two features of the new program distinguish itself from China's prior poverty reduction efforts. First, program assignment was based on a system of county-level targeting. In recognition of the remarkably uneven distribution of poverty across the country, the planner decided to use a targeting device to disburse limited funds to areas of the greatest need. Second, the newly adopted measures introduced a new emphasis on promoting economic development. Unlike prior welfare and relief programs, the transfer was not intended for direct consumption. Rather, resources were primarily channeled toward economic development and revenue-generating activities, which we elaborate below.

A priority ranking of counties is based on a statistic called rural net income per capita.[1] Each year, every county-level statistical agency randomly chooses around 100 rural households as the survey sample. The sample households are told to keep records of their revenues and expenditures in either nominal or real terms. Finally, those data were collected and aggregated, based on which the statistic was computed. This measure is one of the most important official poverty statistics that the Chinese government often relies on in welfare assessment in rural areas and related policy making.

In 1986, the Leading Group initially identified 258 counties as National Poor Counties according to a mixed set of poverty lines. In general, counties with rural net income per capita below 150 yuan were designated as poor. For political considerations, the poverty line was raised to 200 for minority counties and 300 for "revolutionary base" counties. However in practice, the explicit criteria for designation were not strictly enforced.[2]

Although the first-round designation had captured a sizeable fraction of the poor popu-

[1] This approach is known as an indicator-based targeting, or statistical targeting, which relies on certain key indicators to administer interventions (Besley and Kanbur, 1993).

[2] Another 73 counties were added in the three years that followed. For a detailed description of the first-round designation, see Park et al. (2002).

lation, there had been considerable criticism raised against the approach for program placement. The goal of poverty alleviation was heavily compromised by politics (Park et al., 2002). In some provinces, the inclusion of politically favored counties even crowded out counties below the mandated poverty line (World Bank, 2001). Further, the validity of the poverty lines used for designation was questioned by researchers. Partially in response to these drawbacks, the Leading group adopted a renewed poverty line, according to which major revision was made to its list of National Poor Counties in 1993. In principle, the standard for choosing poor counties was rural net income per capita below 400 yuan in 1992. However, faced with the pressure from previously designated counties, the central government decided to raised the poverty line to 700 for the counties label as "poor" before 1993.[3] The new adjustment raised the number of National Poor Counties to 592, almost a third of all counties in China. The red shaded regions in Figure 1 show the designated counties, clustering mostly in inland and mountainous areas. Compared with the previous wave of designation, the new list certainly did a better job covering the poorest population. The government estimates suggest that over 72 percent of the rural poor were residing in the newly designated counties. This second wave of designating poor counties in 1993 and subsequent development assistance are also known as the 8-7 Plan.

The package of interventions in the 8-7 Plan was comprised of three major components. First, the targeted counties received credit assistance from the Agricultural Bank of China. The subsidized loans were first disbursed to rural enterprises and then also directly towards rural households later on. Second, budgetary funds were allocated to these counties by the Ministry of Finance. Third, public employed projects (Food-for-Work projects) were established in the designated areas. The projects sought to support the construction of basic infrastructure, such as roads, drinking water systems, and agricultural infrastructure. The central government distributed coupons to relevant local planning commissions, which used them to fund these projects. The implementation of the entire project was supervised by the Leading Group. All counties designated as poor got treated with roughly the same

[3]It is worth pointing out here that the 1994 designation employed the 1992 data to assign programs and the poverty lines were not made public until the data collecting was completed. This setting makes any precise sorting around the eligibility cutoff very difficult.

intensity. In the analysis that follows, we assume homogeneous treatment intensity and try to estimate the collective impact of the whole set of interventions received by the Poor Counties.

3 Empirical Strategy

In this section, we discuss the econometric models employed to estimate the program impact. Absent a random assignment, one could assess the program effect under a difference-in-differences (DD) framework, i.e. to see whether the income increases more among National Poor Counties than among undesignated counties,

$$\Delta y_i = y_i^{04} - y_i^{94} = \alpha + \beta \cdot \texttt{NP94}_i + \epsilon_i^{04} - \epsilon_i^{94} \tag{1}$$

where Δy_i is the change in rural net income per capita in county i from 1994 to 2004 (hereafter the "gain income"); $\texttt{NP94}_i$ is a binary indicator equal to one if county i was designated as poor in 1994; and ϵ_i^t represents unobserved county-level factors of county i in year t. β is the parameter of interest, which measures the program impact.

Consistent estimation of β using least squares requires $E[\texttt{NP94}_i \cdot (\epsilon_i^{04} - \epsilon_i^{94})] = 0$. If the omitted transitory factors that affect income growth are at the same time correlated with National Poor County status, then the DD approach will yield an inconsistent estimate of the true parameter. Poor and non-poor counties differ dramatically in terms of local "initial conditions" such as geography. If geography, for instance, has different effects on income in different years, it cannot be treated as a fixed effect and is not removed by first-differencing. In particular, non-poor counties mostly lie in coastal and less hilly areas, whereas poor counties tend to be concentrated in remote upland areas. If geographic disparities are important, non-poor counties certainly can benefit more from the fast-growing economy as a whole than poor counties. In this sense, a simple comparison of gain incomes in poor and non-poor counties can lead to an underestimation of the program effect.[4]

The DD estimator could also exaggerate the program's effectiveness. Being designated as poor is partially driven by the transitory shock ϵ_i^{92}. Suppose there is some transitory

[4]In a very similar argument, Ravallion and Jalan (1999) points out that "geographic externalities" is a particularly significant source of bias in conventional evaluations of poor-area programs.

shock ϵ_i^t that is serially correlated. If the shock lingers on for at least two years but wears off almost completely in twelve years, a subsequent rise in y_i^{t+12} among the treated is expected even in the absence of a true program effect.[5] In addition, estimation of β is confounded by unobserved factors such as the change in central leaders' preferences toward certain local leaders/areas, which simultaneously affects the selection of poor counties and local income growth (Park et al., 2002). All the problems described above render the DD estimator unattractive for our purpose.

By tying the decision of designation to certain poverty lines, the assignment rule for the poverty programs in China creates a discrete relationship between a county's pre-program performance (the criteria variable) and its probability of being treated (aka. propensity score). This provides an excellent opportunity to estimate the causal effects of the poverty alleviation program with a Regression Discontinuity (RD) design (Hahn et al., 2001). The simplest version of a RD design is known as the Sharp Design, when there is a perfect relationship between the treatment status and the selection variable. However in many cases, this relationship might be imperfect when the observed selection criterion is not strictly followed. The administrator may rely on other factors to assign treatment, but these additional variables might be unobserved by the researcher. In this case the assignment to treatment depends on the selection variable in a stochastic manner. This setup corresponds with a Fuzzy Design.

It is useful to conceptualize the RD approach in an instrumental variable (IV) framework in case of a Fuzzy Design (Angrist and Lavy, 1999; van der Klaauw, 2002). In essence, we use the initial eligibility of the program, $\texttt{Eligible}_i = 1(y_i^{92} < \bar{y})$, as an instrument for actual National Poor County status, $\texttt{NP94}_i$. Here $1(\cdot)$ is an indicator function which equals unity when the embraced statement is true; $\bar{y}$ is the publicized threshold of National Poor County (NP94) eligibility. The parametric first-stage and reduced-form equations are

$$\texttt{NP94}_i = \pi_0 + \pi_1 \cdot \texttt{Eligible}_i + f(y_i^{92}) + u_i \tag{2}$$

[5]This problem is analogous to that observed in the analysis of training programs, in which participants are selected into treatment after experiencing a decline in pre-program earnings (Ashenfelter, 1978).

and

$$\Delta y_i = \gamma_0 + \gamma_1 \cdot \texttt{Eligible}_i + g(y_i^{92}) + v_i \tag{3}$$

where $f(\cdot)$ and $g(\cdot)$ are smooth funtions of the selection variable. π_1 captures the discontinuous change in the propensity score at the cutoff; γ_1 measures the relative difference in the outcome variable for counties above and below the cutoff. In an exactly identified case like ours, the IV estimator, β_{IV}, is simply the ratio γ_1/π_1. The key assumption for identification here is that there is no discontinuity in counterfactual outcomes at the critical cutoff. In practice, we model $f(\cdot)$ and $g(\cdot)$ as lower-order polynomials and simple Two-Stage Least Squares (2SLS) implicitly assumes that both functions are approximated by polynomials of the same order. Since two different types of counties face two distinct cutoffs as described in the background section, we also include county-type fix-effects in our empirical model.

4 Data

In this paper, we use a panel dataset that covers 1947 Chinese counties in 30 provinces over 20 years (1981-1995). County is the unit of observation. The data contains both economic variables and information on population and geography for each county. The majority of the data was collected by the Ministry of Agriculture. The economic variables in the year of 2004 come from various issues of Provincial Statistical Yearbooks and are deflated into 1994 prices. Price deflators are collected from the China Compendium of Statistics 1949-2004. Due to missing information on the county-level income per capita for a few provinces in certain years, our major sample has approximately 1300 observations. Descriptive statistics of the key variables are provided in Table 1. To facilitate the comparison of baseline characteristics between the poor counties and non-poor counties, we also provide descriptive statistics by Poor County status in Table 2.

5 Results

In this section, we systematically test whether the poverty alleviation program has a positive impact on rural income. Unless otherwise noted, we use the Eicker-White robust standard errors for inference.

As a useful benchmark for our analysis, we first present simple difference-in-differences estimates of the program effect in Table 3. The dependent variable is the gain income, or the change in the rural net income per capita 1994-2004.[6] Column 1 gives the unadjusted correlation between the dependent variable and the designation status. The coefficient on the treatment dummy has a counterintuitive negative sign and is statistically significant at the five percent level. It suggests that the program is *hurting* the poor counties: over the decade, counties with designation had about a 115 yuan relative loss in rural net income per capita. To see if the NP94 indicator has picked up any cross-county variation along other dimensions, column 2 introduces into the regression a set of pre-program county-level variables, namely the population, sown area per capita, grain production per capita and industrial income per capita in 1994. Interestingly, after the adjustment for covariates, the sign of the estimated program effect reverses. Given the standard error, however, we are unable to reject the null hypothesis that there is no program effect. Column 3 further includes province fixed-effects in the regression, allowing only with-in province comparisons of counties. The size and precision of the point estimate grow substantially. The implied program effect is a gain income close to 150 yuan. Overall, the results produced by a DD strategy are very sensitive to the choice of control variables, suggesting that omitted factors may play an important role.

As discussed earlier, the first differencing approach is unable to control for time-varying factors that affect both designation status and income. To address this concern, we next implement the instrumental variable approach described in section 3 to estimate the program effects. We use the indicator of initial eligibility to instrument for the actual designation status in 1994.

Prior to presenting the formal regression results, we first provide some graphical evidence. Figure 3 plots the first-stage relation between a county's 1992 per capita income relative to the cutoff and its probability of being treated. The solid line comes from the estimation of a nonparametric regression that uses a (unweighted) uniform kernel density

[6] Gain incomes are in 1994 prices unless otherwise noted

smoother. There are indeed stark changes in the probability of being treated around the cutoff. The dashed lines in the same figure come from predicted values of a bivariate regression of NP94 on eligibility. It shows that the probability of treatment is 0.70 higher among the initially eligible counties.

Figure 2 plots each county's change in income per capita, the outcome of interest, against its income per capita relative to the eligibility cutoff in 1992, the running variable. Each solid circle represents the local average gain income within 5 yuan intervals of the running variable. The continuous line is the predicted outcome from a regression that includes a third order polynomial in the running variable, and a dummy for observations above the cutoff. The dashed lines are pointwise 95 percent confidence intervals from the regression. Overall, there is a slightly positive association between the outcome variable and the running variable, which suggests that poor counties are lagging behind in terms of income growth. More importantly, there is a clearly discernible gap in the predicted outcome at the threshold. Compare the counties which were barely eligible (for example, the income relative to cutoff was -5 yuan), with counties which were barely ineligible (for example, the income relative to cutoff was 5 yuan). With a valid regression discontinuity design, the two groups should on average be similar in every respect except having different propensity scores. Therefore the break of the fitted line at the cutoff is an estimate of parameter π_1 in equation (2) (without any adjustment of covariates), equal to roughly 200 yuan.

Table 4 (Panel A) reports the results from estimating equation (2), the first-stage relation between actual treatment, the dependent variable, and initial eligibility. Column (1) shows the results from our most parsimonious specification, where eligibility is the only regressor other than provincial and county-type dummies. The coefficient on eligibility is highly significant and large in magnitude, and the R^2 exceeds 0.6. Consistent with the graphical evidence, the eligible counties, i.e., those below the official poverty line, have a significantly higher probability of being designated. The size of this estimate remains substantial (greater than 0.4) when a cubic of the 1992 income per capita and other controls are included (columns 2-4). In short, there are indeed sharp changes in the probability of treatment near the eligibility threshold.

Panel B contains the results from estimating equation (3), the reduced-form model. We regress gain income on the indicator for program eligibility. Column (1)-(4) give the results of regressions using the same specifications as in Panel A. The eligibility coefficients are always positive and significant at least at the 10-percent level. Moreover, the fit of the regressions is considerably good. From a model with the richest set of controls in column (4), the estimated impact of eligibility is 145 yuan, equivalent to a 0.2 standard deviation of the gain income.

Panel C presents the instrumental variable results for a variety of specifications. These estimates are just the ratio of the reduced-form coefficients (in Panel A) and first-stage estimates (in Panel B). The estimate in column (1) suggests a large effect when the selection variable is not controlled for. As column (2) includes the per capita income in 1992, the NP94 coefficient shrinks by more than a third. Adding a cubic in the per capita income somehow increases the size of the estimate, as shown in column (3). The coefficient is large in magnitude and significant at the 5-percent level, and it implies that in 1994-2004, designated counties had about a 360 yuan increase in income per capita (a 0.45 standard deviation). Further, this estimate is insensitive to including other county-level covariates (column 4). Overall, the IV estimates for the program effect are always significant and relatively stable across specifications.

For our parametric estimates to be credible, identification of the intercept shift at the threshold should not rely solely on particular functional forms, or on data points in the extreme ends of the distribution. To address this concern, we add higher order polynomials and limit our sample within increasingly narrow intervals around the cutoff. Table 5 reports the results from these exercises. For the sake of comparison, column (1) simply reproduces the results from column (4), Panel C in Table 4. In the next column, a quartic term is included and the entire sample is used for estimation. The parameter estimate is very similar to that from the previous specification. In the following two columns, we focus on counties within 1000 yuan of their respective cutoffs and experiment with different functional forms. Our estimates appear robust to these changes. Columns (5)-(6) repeat the exercise with a "±500 yuan" window. Again, the estimated effects are statistically significant and comparable in magnitude. Broadly speaking, our estimates are largely insensitive to the choice of functional forms and samples.

The identifying assumption underlying the RD design is that before treatment, only counties' propensity scores change discretely at the critical cutoffs. All other county-level factors, observable or unobservable, should evolve smoothly with the running variable, especially at the cutoffs. As a partial test of this assumption, we can check for discontinuities in the predetermined county characteristics that might confound our estimates. Table 6 contains tests of smoothness of a set of county-level covariates through the eligibility cutoff. Each column gives a discontinuity estimate for a different variable, using the eligibility indicator, a quartic in 1992 income per capita and a county type dummy as explanatory variables. None of the point estimates are statistically distinguishable from zero, suggesting no clear breaks around the cutoff.

The continuity assumption could also be violated in the presence of precise sorting around the eligibility threshold (McCrary, 2007). According to the institutional setup, manipulation of this sort should not be expected. In 1994, the administrator used 1992 data to allocate treatment and the exact poverty lines were not released until after the data were collected. Figure 3 presents a histogram of the number of counties at each of the 50 bins of 1992 rural income per capita, suggesting no evidence of nonrandom sorting across the cutoffs.

6 Conclusion

This study employs a panel data set to examine the performance of 8-7 Plan, a poverty alleviation program introduced in the early 1990's by the Chinese government. The program's placement rule causes a discontinuity in a county's probability of treatment as a function of its pre-program per capita income. This feature is exploited to identify the causal effects of the program on the change in the county's per capita income. Based on a regression discontinuity approach, our analysis shows that the program had a significant positive effect on income growth. This finding is robust to different model specifications. According to our estimates, the program increased per capita income gains by around 0.45 standard deviation in the 1994-2004 period. This estimated program effect is much larger than that obtained from a conventional difference-in-differences method, which indirectly reveals the important role of initial endowments in economic development.

Constrained by data availability, our assessment of the 8-7 Plan focuses on average income growth instead of poverty reduction. The use of county-level data also prevents us from depicting the program's distributional implications. For a more comprehensive evaluation of the success of the 8-7 Plan, more research is needed to look at these dimensions of the program effect.

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Table 1: Summary statistics

Variables	Observations	Mean	Standard Deviation	Min	Max
NP94	1947	0.28	0.45	0	1
1992 rural net income per capita	1947	647.47	293.56	119	2482
1994 rural net income per capita	1797	1022.24	500.33	221	3866
2004 rural net income per capita (1994 price)	1453	2143.94	1035.55	467	6497
Gain income per capita 94-04	1305	1126.65	800.98	-1891	5098
National Poor County before 1994	1946	0.16	0.37	0	1
1994 population	1806	469218.7	330401.1	7229	2079895
1994 sown area per capita (Mu)	1789	2.231	0.94	0.15	8.24
1994 Grain production per capita (Kg)	1790	424.24	198.68	2.21	2029.54
1994 Industrial income per capita	1780	1522.37	3189.00	0.55	43970.03

Table 2: County-level covariates by Poor County status

Variables	Observations	Mean	Standard Deviation
National Poor Counties			
1992 rural net income per capita	549	409.36	122.81
1994 rural net income per capita	491	634.20	212.46
2004 rural net income per capita (1994 price)	395	1655.93	763.88
Gain income per capita 94-04	338	1041.76	681.01
Population in 1994	502	367316	278826.1
1994 sown area per capita (Mu)	498	2.47	1.16
1994 Grain production per capita (Kg)	498	342.63	137.81
1994 Industrial income per capita	495	308.28	408.03
Non-National Poor Counties			
1992 rural net income per capita	1398	740.98	288.26
1994 rural net income per capita	1306	1168.12	499.62
2004 rural net income per capita (1994 price)	1058	2326.14	1064.61
Gain income per capita 94-04	967	1156.32	837.17
Population in 1994	1304	508448.1	340278
1994 sown area per capita (Mu)	1291	2.14	0.82
1994 Grain production per capita (Kg)	1292	455.70	209.36
1994 Industrial income per capita	1285	1990.05	3638.55

Table 3: Difference-in-differences results for 1994-2004 gain income per capita

	Dependent variable: Gain income per capita 94-04		
	(1)	(2)	(3)
NP94	-114.57**	9.40	146.45***
	(45.77)	(55.26)	(52.15)
Controls			
County-level factors	N	Y	Y
Provincial dummies	N	N	Y
R^2	0.004	0.09	0.38
Sample size	1305	1268	1268

Notes: Huber-White standard errors are in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: First-stage, reduced-form, and 2SLS results for 1994-2004 gain income per capita, using eligibility for NP94 as an instrument

	(1)	(2)	(3)	(4)
Panel A: 1st stage (NP94)				
Eligible	0.70***	0.61***	0.42***	0.43***
	(0.04)	(0.04)	(0.05)	(0.05)
R^2	0.61	0.63	0.66	0.66
Panel B: reduced-form (Gain income per capita 94-04)				
Eligible	290.38***	163.01**	153.58**	145.28*
	(57.14)	(64.53)	(74.16)	(75.39)
R^2	0.41	0.42	0.42	0.40
Panel C: 2SLS (Gain income per capita 94-04)				
NP94	415.66***	268.94**	362.36**	339.58*
	(86.47)	(109.58)	(182.79)	(185.64)
Notes on all panels				
y^{92}	N	Y	Y	Y
Cubic in y^{92}	N	N	Y	Y
Other county-level controls	N	N	N	Y
Sample size	1305	1305	1305	1268

Notes: Huber-White standard errors are in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%
All regressions control for county-type and provincial dummies.

Table 5: 2SLS results for 1994-2004 gain income per capita, using eligibility for NP94 as an instrument

	Full sample		±1000 yuan		±500 yuan	
	(1)	(2)	(3)	(4)	(5)	(6)
NP94	339.58* (183.65)	365.04** (185.52)	366.30** (187.33)	363.49* (193.46)	398.90** (203.71)	542.51** (229.54)
Polynomial terms in y^{92}	3	4	3	4	3	4
Sample size	1268		1243		1069	

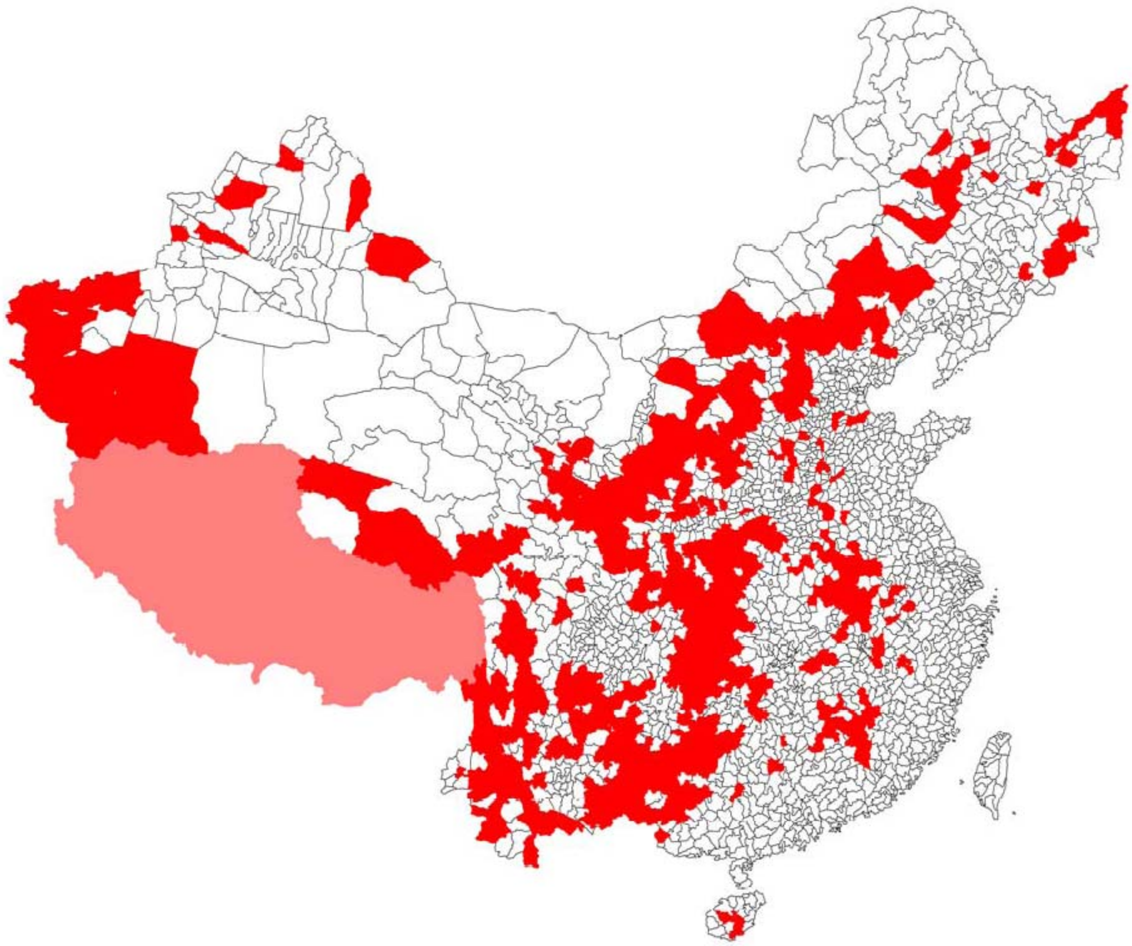
Notes: Huber-White standard errors are in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%
All regressions include county-level controls and provincial dummies.

Table 6: Continuity of pre-determined county-level characteristics

	Dependent variables (1994 values)			
	(1)	(2)	(3)	(4)
	Population	Sown area per capita	Grain production per capita	Industrial income per capita
Eligible	56566.37	-0.17	12.69	16.48
	(43380.92)	(0.12)	(17.28)	(100.79)

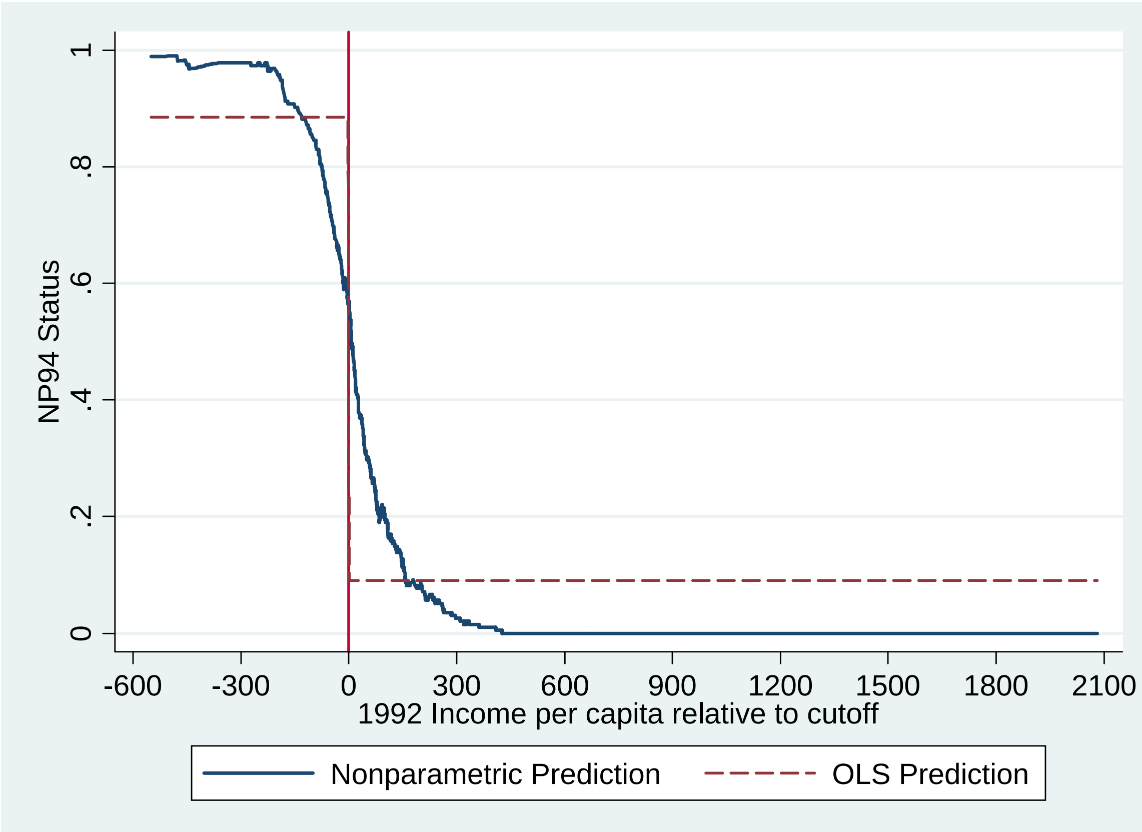
Notes: Huber-White standard errors are in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 1: Map of National Poor Counties (designated in1994)



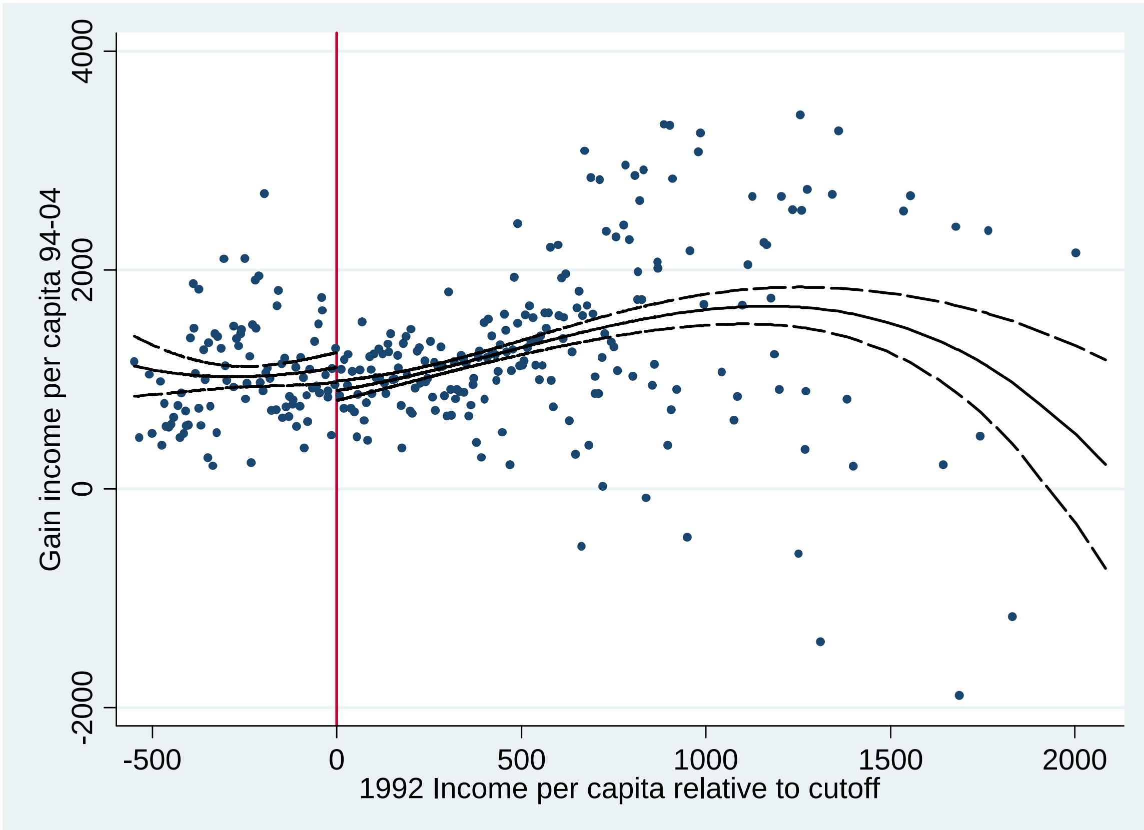
Source: Heilig et al. (2006)

Figure 1: Program placement



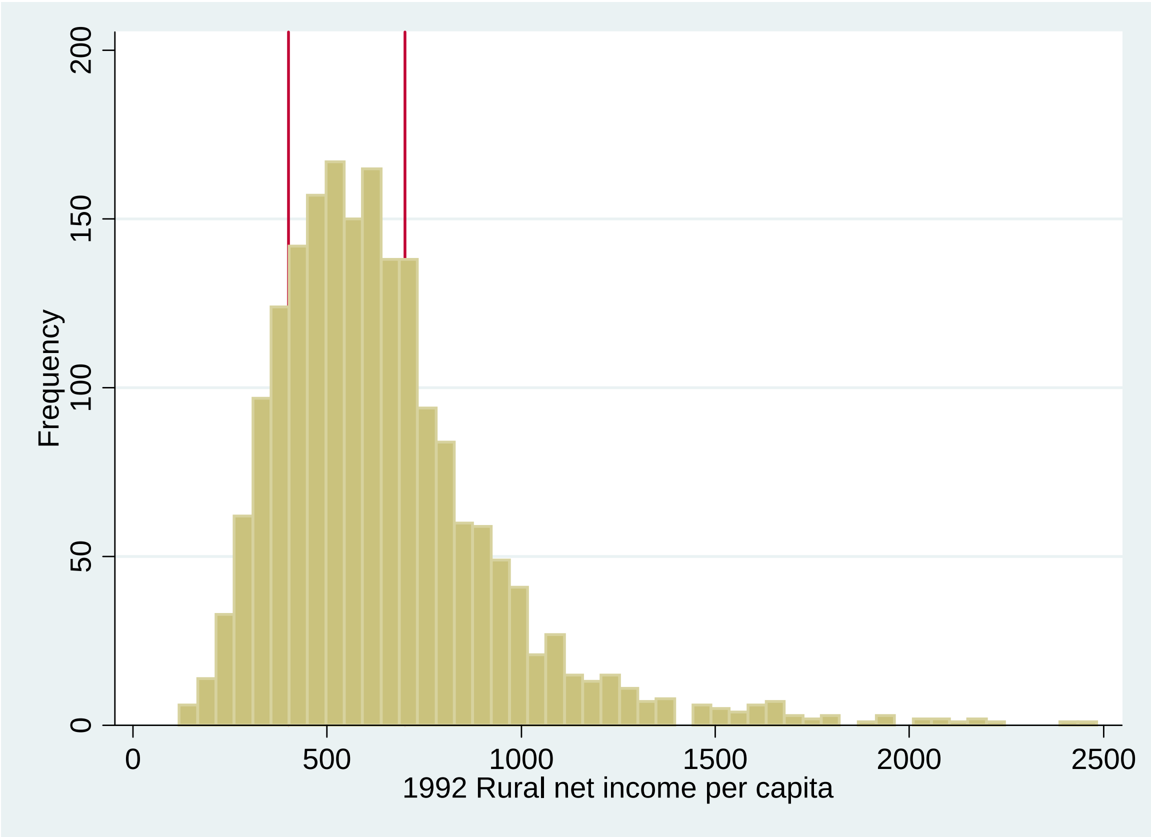
Notes: The solid lines plots unweighted smoothed values of the proportion of counties designated as poor in 1994 (with a bandwidth of 0.10). The dashed line is the OLS prediction from regressing NP94 on Eligible.

Figure 2: Change in income per capita by 1992 income per capita relative to cutoff



Notes: Each solid circle represents the local average gain income within 5 yuan intervals of the 1992 income per capita relative to cutoff. The continuous line is the predicted outcome from a regression that includes a third order polynomial in the running variable, and a dummy for observations above the cutoff. The dashed lines are pointwise 95 percent confidence intervals from the regression.

Figure 3: Histogram of the 1992 rural net income per capita



Notes: The red lines indicate the poverty lines used for 1994 designation.