Evaluating China’s poverty alleviation program: A regression discontinuity approach

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Abstract

This paper evaluates the impact of the 8–7 Plan, the second wave of China’s poverty alleviation program, on rural income growth at the county level over the program’s disbursement period, from 1994 to 2000. Program participation was largely determined by whether a county’s pre-program income fell below a given poverty line; hence, a regression discontinuity approach is employed to estimate the causal effects of the program. Using a panel data set, we find that the 8–7 Plan resulted in an approximately 38-percent increase in rural income for counties that were treated between 1994 and 2000. Our empirical results also suggest the important role of initial endowments in the path toward economic development.

1. Introduction

Evidence from cross-country studies suggests that sustained economic growth has typically been effective in reducing poverty (Ravallion and Chen, 1997; Dollar and Kraay, 2002). However, broad-based growth is not always the panacea for poverty (Morduch, 2000), possibly because 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 over the lagging poor, public efforts in many countries have assumed the form of poor area development programs to fight poverty. Despite the theoretical underpinnings of pursued strategies, whether these programs worked as intended remains unknown.

In this paper, we implement a regression discontinuity (RD) approach to deal with the issue of nonrandom program placement. One distinctive feature of the 8–7 Plan is that poverty assignment was based on an identifiable indicator, specifically whether the pre-program income fell below a given poverty line. Hence, a regression discontinuity approach is employed to estimate the causal effects of the program. Using a panel data set, we find that the 8–7 Plan resulted in an approximately 38-percent increase in rural income for counties that were treated between 1994 and 2000. Our empirical results also suggest the important role of initial endowments in the path toward economic development.

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program rural income per capita was below the poverty line. As such, the causal impacts of the program can be gauged by comparing the counties just below the dividing line to the counties just above it. Empirically, we use the discontinuity of the program assignment at the eligibility threshold to construct an instrumental variable for actual treatment status.

Employing a panel data set of roughly 1700 Chinese counties over 20 years, our two-stage least squares estimates reveal that the 8-7 Plan produced a sizeable positive impact on rural income growth. We estimate that the program has increased per capita income by approximately 38% (or 0.96 standard deviation) during the program period of 1994 to 2000. According to our estimates, the rate of return on poverty investments was 42% during the program's disbursement period. Our estimate is considerably larger than a conventional difference-in-differences (DD) estimation, which tends to be biased. Downward bias in the “conventional” estimates indirectly reveals the significant role of initial conditions in subsequent economic development.

Importantly, estimates of the program effects from our design are robust across a variety of specifications. These estimates are insensitive to the inclusion of various control variables and different functional form choices. Moreover, estimates are insensitive to the exclusion of observations far away from the cutoff point. Finally, we demonstrate that our results are unlikely to be driven by discontinuities in pre-program county characteristics or endogenous sorting around the threshold.

To explore whether the program has a persistent effect, we examine income gains three years after the program ended. Estimates of program effect in a “longer run” are smaller in magnitude. This finding indicates that the program may have had only sizeable short-run effects that began to decay just a few years after its implementation.

Our paper is closely associated with prior literature studying China’s poverty programs. The 8-7 Plan was pre-dated by the first wave of China’s poverty program, which is highly similar in content but smaller in scale. Modest literature evaluating first-wave interventions suggests that the program was successful in raising income and consumption of people residing in poor counties (Jalan and Ravallion, 1998; Rozelle et al., 1998; Park et al., 2002). As an aside, Park et al. (2002) likewise examine the performance of the 8-7 Plan after its first full year of implementation by employing the DD approach. As an interim evaluation, the DD approach observes that the program increased per capita income growth by 0.91% during the 1992 to 1995 period. Despite the importance of China’s 8-7 Plan, the work of Park et al. (2002) remains the only study to date that assesses the effectiveness of the program. Given that the new wave of the program was not effective until 1994, an estimate of the longer-term impact is needed to assess the program’s overall efficacy.

Our empirical findings have important policy implications. First, as the most populous developing country, China’s success against poverty has played a major role in the marked decline in the global poverty rate since the early 1980s (Chen and Ravallion, 2010). Our empirical findings indicate that China’s publicly funded poverty alleviation programs had very important contributions to this trend. Second, important lessons for the rest of the developing world can be gleaned from China’s poverty reduction efforts. Our analysis reveals that in a setting where poverty is closely related to geography, gains from area-specific interventions to reduce poverty can be substantial, at least within its disbursement period. Third, our results echo earlier research that surmised the limited long-term effectiveness of this type of poor-area programs (Chen et al., 2009).

Our paper has a few caveats. First, we are unable to identify the exact mechanisms of how the program works. The program simultaneously has three different types of interventions. However, the current data set does not permit us to identify their effects separately. Second, the estimated return may still be biased, and the direction of bias is unclear. On one hand, our results may overstate the rate of return on poverty investments for non-economometric reasons, such as unobserved administrative costs and subsidized loan defaults. On the other hand, our results may also underestimate the rate of return if the 8-7 Plan crowds out other government spending.

The remainder of the current research proceeds as follows. Section 2 provides background information on poverty in China and 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 provides the conclusion.

2. Background

2.1. Poverty in China

In China, poverty is closely linked to geography (Ravallion and Jalan, 1999). There is a marked difference between rural and urban areas, with existing evidence pointing toward poverty being mainly a rural phenomenon (World Bank, 2000). Moreover, greater differences in the incidence of rural poverty exist between prosperous coastal areas and less developed and most poverty-prone inland areas. Fig. 1 shows the geographic distribution of rural poverty incidence in 1991 at the provincial level. A province’s shading indicates its rural poverty incidence, in which darker shading represents higher incidence level. The graph clearly shows the geographic concentration of poverty in inland areas.

Two rounds of poverty reduction have been recorded since the mid-1980s: the first round during 1986 to 1993, and the second round during 1994 to 2000. Official statistics indicate that the poor population declined during the 1990s. Fig. 2 plots trends from 1990 to 2000 in the entire nation’s official poverty headcount. Poverty reduction was impressive over the given period, with both the size of poor population and incidence of poverty experiencing a dramatic decline. The population identified as poor decreased from 85 million in 1990 to 32 million in 2000. As a share of rural population, poverty headcount fell from 9.5% in 1990 to 3.4% in 2000.

2.2. First-round poverty alleviation program

In response to concerns over the lagging poor, the Chinese central government launched an ambitious anti-poverty program in the mid-1980s. The interministerial Leading Group for Poverty Reduction (the Leading Group hereafter) was founded in 1986 to supervise and coordinate the implementation of the entire program. Two features distinguish the new program from 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 with the greatest need. Second, the newly adopted measures emphasized the promotion of 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.

Priority ranking of counties is based on a statistic known as the rural net income per capita. Every year, each county-level statistical agency randomly selects approximately 100 rural households, and asks these

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1 Our identification strategy is similar to an evaluation of Head Start program in the United States (Ludwig and Miller, 2007). Using the discontinuity in program funding across counties, Ludwig and Miller (2007) showed that the program has led to a substantial decline in child mortality and improved educational attainment.

2 Needless to say, the geographically targeted program is unlikely to benefit the poor in areas not covered, and there may be leakage to the non-poor in designated poor areas (Park et al., 2002).

3 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).
samples to keep records of all their revenues and expenditures. Subsequently, data are collected and aggregated to calculate county-level rural net income per capita. This measure is one of the most important official poverty statistics used by the Chinese government in welfare assessment in rural areas and related policy making (Park and Wang, 2001).

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. In practice, however, explicit criteria for designation were not strictly enforced. Although the first-round designation captured a sizeable fraction of the poor population, considerable criticism surrounded the approach for program placement, which was heavily compromised by politics (Park et al., 2002). In certain provinces, inclusion of politically favored counties even crowded out counties below the mandated poverty line (World Bank, 2000). Furthermore, researchers questioned the validity of poverty lines used for designation.

2.3. Second-round program: 8-7 Plan

In response to criticisms, the Leading Group adopted a renewed poverty line, according to which major revision was applied to the National Poor Counties list in 1993. In principle, poor counties are those with rural net income per capita below 400 yuan in 1992. However, faced with pressure from previously designated counties, the central government decided to raise the poverty line to 700 for counties labeled as "poor" before 1993. In total, 592 counties were designated as National Poor Counties, almost a third of all counties in China.

In Fig. 3, regions shaded in red show the designated counties, clustering mostly in inland and mountainous areas. Compared to the previous wave of designation, the new list certainly performed better.

4. An additional 73 counties were added in the three years that ensued. For a detailed description of first-round designation, see Park et al. (2002).

5. It is worth noting here that the 1994 designation employed the 1992 data to assign programs, and poverty lines were not made public until data collection was completed. This setting renders any precise sorting around the eligibility cutoff unlikely.
in covering the poorest population. Government estimates suggest that over 72% of the rural poor were residing in newly designated counties. This second wave of designating poor counties in 1993 and the subsequent development assistance are likewise known as the 8-7 Plan.\textsuperscript{6}

Implementation of the entire project was supervised by the Leading Group, headed by a top-ranking official. Aside from its massive spending, the central government attempted to meet its poverty reduction goals by leveraging its unique personnel control system.\textsuperscript{7} In a meeting in 1996, the central government stated clearly that it would reward or punish local government officials according to their performance in poverty alleviation. In particular, local officials would be demoted should they fail to accomplish poverty reduction objectives set by the central government.

The 8-7 Plan has three major interventions for poverty reduction. First, targeted counties received credit assistance. The government invested RMB 27.8 billion (1994 price) in the subsidized loan program during the seven-year period of 1994 to 2000. Every year, funds for loans were disbursed by the People’s Bank of China (central bank) to provincial Agricultural Banks of China (ABCs), which in turn made allocations to their lower-level branches, following the scheme established by the Leading Group. Final decisions to lend were made jointly by county-level offices of the Leading Group and the ABC. The subsidized annual interest rate was 2.88%. Before 1996, the bulk of loans was channeled to rural enterprises rather than rural households; priority of allocation switched back to households in 1996.

Second, the Ministry of Finance provided budgetary grants for investment in poor counties. The funds, primarily managed through the fiscal system, totaled RMB 12.5 billion (1994 price) over the seven-year period. Upon approval by the Leading Group, the Ministry of Finance made allocations to provincial departments of finance. Provincial departments of finance then transmitted the funds to county finance bureaus, which administered the final disbursements in cooperation with the respective sectoral county government departments. Majority of funds came in the form of Poor Area Development Funds, which were used for productive construction projects. Other budgetary grants were provided through earmarked grants for basic education, health care, and soon.

Third, public employed projects (i.e., Food-for-Work) were established in designated areas. The program sought to raise the long-term development capacity of poor areas by supporting land improvement and the construction of basic infrastructure, such as roads and drinking water systems. The program likewise provided

\textsuperscript{6} The program was called “8-7 Plan” because its main objective was to raise the majority of the remaining 80 million poor above the government’s poverty line within seven years.

\textsuperscript{7} For a review of China’s personnel control system and empirical evidence of its incentive role in local economic development, see Li and Zhou (2005).
short-term assistance to the poor by creating more jobs. The central government distributed coupons to relevant local planning commissions, which then used these to pay for physical inputs and labor under the program. The central and provincial governments made decisions on the types of investment, county governments selected sites, and village committees were responsible for the allocation of project investment and labor mobilization.

Although designated poor counties differ in the level of development, all were treated with roughly the same intensity. In the following analysis, we assume homogeneous treatment intensity and attempt to estimate the collective impact of the whole bundle of interventions received by the poor counties.

3. Empirical strategy

In this section, we lay out the econometric models employed to estimate the program impact. With no random assignment, we can assess the program effect using the DD framework, which examines whether the income increases more among National Poor Counties than among undesignated counties:

\[
\Delta \log y_i = \log y_{92}^0 - \log y_{94}^0 = \alpha + \beta_1 \text{NP94}_i + \epsilon_{i90} - \epsilon_{i94}
\]

where \( \Delta \log y_i \) is the change in log rural net income per capita in county \( i \) from 1994 to 2000; \( \text{NP94}_i \) is a binary indicator equal to one if county \( i \) was designated as poor in 1994; \( \beta_1 \) is the parameter of interest, which measures the program impact; and \( \epsilon_i \) represents unobserved county-level factors of county \( i \) in year \( t \).

Consistent estimation of \( \beta \) using least squares requires \( \text{E} [\text{NP94} \cdot (\epsilon_{i90} - \epsilon_{i94})] = 0 \). If the omitted transitory factors that affect income growth are simultaneously correlated with the National Poor County status, then the DD approach will yield an inconsistent estimate of the true parameter. In fact, poor and non-poor counties differ dramatically in terms of local “initial conditions” such as geography. 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 are likely to benefit more from the fast-growing economy than poor counties. In this situation, a simple comparison of income gains in poor and non-poor counties can lead to an underestimation of the program effect.8

The DD estimator can exaggerate the program’s effectiveness as well. Being designated as poor is partially driven by the transitory shock \( \epsilon_i \). Suppose there is a certain transitory shock \( \epsilon_i' \) that is serially correlated. If the shock lingers on for two years but wears off almost completely in eight years, a subsequent rise in \( \Delta \log y_i \) among those treated is expected, even in the absence of a true program effect.9 In addition, the estimation of \( \beta \) is confounded by unobserved factors, such as change in central leaders’ preferences toward certain local leaders or areas, which simultaneously affects the selection of poor counties and local income growth (Park et al., 2002). All 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 poverty programs in China creates a discrete relationship between a county’s pre-program performance \( \text{(criteria variable)} \) and its probability of being treated \( \text{(or propensity score)} \). This condition provides an opportunity to estimate the causal effects of poverty alleviation program with the RD design (Hahn et al., 2001). The simplest version of an RD design is known as the sharp design, where a deterministic relationship exists between the treatment status and the selection variable (Lee, 2008). However, in many cases, the relationship may 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 may be unobserved by researchers. In this case, assignment to treatment depends on the selection variable in a stochastic manner, which is referred to as a fuzzy design.

Conceptualizing the RD approach in an instrumental variable (IV) framework is useful in case of a fuzzy design (Angrist and Lavy, 1999; van der Klauw, 2002). In essence, we use the initial eligibility of the program, \( \text{Eligible}_i = 1(y_{92}' - \gamma) \), as an instrument for actual National Poor County status, \( \text{NP94}_i \), \( \gamma \) is a constant function that is equal to unity when the embraced statement is true, and \( \pi \) is the publicized threshold of National Poor County (NP94) eligibility. Parametric first-stage and reduced-form equations are as follows:

\[
\text{NP94}_i = \pi_0 + \pi_1 \text{Eligible}_i + f(y_{92}') + u_i
\]

and

\[
\Delta \log y_i = \gamma_0 + \gamma_1 \text{Eligible}_i + g(y_{92}') + v_i
\]

where \( f(\cdot) \) and \( g(\cdot) \) are smooth functions of the selection variable. In practice, we model \( f(\cdot) \) and \( g(\cdot) \) as lower-order polynomials with different slopes on two sides of the cutoff.10 \( \pi_1 \) captures discontinuous change in the propensity score at the cutoff; \( \gamma_1 \) measures the relative difference in outcome variable for counties above and below the cutoff. In an exactly identified case like ours, the IV estimator, \( \beta_{IV} \), is simply the ratio \( \gamma_1/\pi_1 \). As there are two different types of counties, which face two distinct cutoffs as described in the Background section, we include county-type fixed-effects in our empirical model as well.

The key assumption for identification is that no discontinuity exists in counterfactual outcomes at the critical cutoff. Moreover, as detailed information on the implementation of individual counties is unavailable, we have to assume that the program intensity is the same for every county, and that the program impacts are homogeneous.

4. Data and descriptive evidence

In this paper, we use a county-level data set on 1946 Chinese counties over 15 years (1981 to 1995) collected by the Ministry of Agriculture (MOA).11 The data set contains important social and economic variables of these counties, particularly rural net income per capita, the variable used for program assignment, and welfare assessment. We supplement the data set with economic variables for the years 2000 and 2003 from various issues of the Yearbooks of Agricultural Development Bank of China. Price deflators at the provincial level are obtained from the China Compendium of Statistics 1949–2004. Our major sample has approximately 1700 observations because of the missing information on county-level income per capita for a few counties in certain years. Descriptive statistics of the full sample are provided in Table 1, and those for poor and non-poor counties are presented in Table 2. Approximately 28% of counties were designated as National Poor Counties in 1994, based on 1992 rural net income per capita (current price).

Statistics reveal that the National Poor Counties are indeed considerably poorer than non-poor counties before the program was introduced; however, they have now outgrown non-poor counties. In 1994, average

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8 In a similar argument, Ravallion and Jalan (1999) point out that “geographic externalities” are a particularly significant source of bias in conventional evaluations of poor-area programs.

9 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).

10 Simple two-stage least squares (2SLS) implicitly assume that both functions are approximated by polynomials of the same order.

11 During the period under consideration, there were roughly 2300 county-level regions, excluding districts within prefecture-level cities. The MOA is an executive state agency within the central government. This ministry has been using its own sampling framework and survey instruments to collect household data since 1980. Considering that the MOA is of a much higher rank than the county governments, influencing the household survey is difficult for local officials.
per capita income of the non-poor counties was 84% higher than that of the National Poor Counties. By 2003, however, the average per capita income was only 51% higher, suggesting that poor counties grew faster than non-poor ones during the program period. However, we cannot simply interpret the difference in growth rate as the causal effect of the program, as treatment status was not randomly assigned. For the National Poor Counties, by 2003, the average per capita income of the non-poor counties was 84% higher than that of the National Poor Counties. By 2003, however, the average per capita income was only 51% higher, suggesting that poor counties grew faster than non-poor ones during the program period. However, we cannot simply interpret the difference in growth rate as the causal effect of the program, as treatment status was not randomly assigned. Prior to presenting the formal regression results, we first provide a number of graphical evidence of program designation. Fig. 4 plots the first-stage relation between a county's 1992 per capita income relative to the cutoff and its probability of being treated. Each data point (open circle) represents the fraction of counties designated as poor within 20-yuan intervals of the 1992 income per capita relative to cutoff. The figure presents dramatic evidence that there indeed exists a stark change in the probability of being treated around the cutoff. Visually, probability of treatment is approximately 0.60 higher among the initially eligible counties.

### Table 1
Summary statistics.

<table>
<thead>
<tr>
<th>Variables</th>
<th>Observations</th>
<th>Mean</th>
<th>Standard deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>NP94 (National Poor County in 1994)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1992 rural net income per capita</td>
<td>1462</td>
<td>0.0799</td>
<td>0.0662</td>
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<tr>
<td>(current price)</td>
<td></td>
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<tr>
<td>1994 rural net income per capita</td>
<td>1462</td>
<td>0.0894</td>
<td>0.0757</td>
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<tr>
<td>(1994 price)</td>
<td></td>
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<tr>
<td>2000 rural net income per capita</td>
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<td>(1994 price)</td>
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<tr>
<td>2003 rural net income per capita</td>
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<td>(1994 price)</td>
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<tr>
<td>Change in log income per capita 94–00</td>
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<td></td>
<td></td>
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<tr>
<td>National Poor County before 1994</td>
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<tr>
<td>Minority county</td>
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<td></td>
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<tr>
<td>Revolutionary base</td>
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<tr>
<td>Fraction with high school or more</td>
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<tr>
<td>Fraction with college or more</td>
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Notes: The major data set is collected by the Ministry of Agriculture. NP94 is an indicator variable that is equal to one if the county was designated as a National Poor County in 1994.

### Table 2
County-level covariates by poor county status.

<table>
<thead>
<tr>
<th>Variables</th>
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<th>Standard deviation</th>
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</thead>
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Notes: The major data set is collected by the Ministry of Agriculture.
ineligible (e.g., income relative to cutoff is 5 yuan). With a valid regression discontinuity design, the two groups on average should be similar in every respect, although they have different propensity scores. Therefore, the break of the fitted line at the cutoff is an estimate of parameter \( \pi_1 \) in Eq. (2) (without any adjustment of covariates), equal to roughly 0.15. The 8–7 Plan gives the National Poor Counties an explicit incentive to strategically over-state their post-treatment income. If income in the MOA data is reported by the local governments, our results may actually reflect misreporting rather than real growth. To address this concern, we use an alternative data set, the National Poverty Monitoring Survey (NPMS), which is independently collected by the National Bureau of Statistics (NBS) to check the quality of the MOA data. The NBS is a national statistical agency that reports directly to the state council and is therefore independent of the MOA. Each year, the NPMS randomly selects 60 to 150 households in each poor county. The sampled households are asked to keep accounts of their monthly or quarterly incomes and expenses. The raw data are directly submitted by the Rural Household Survey teams of the NBS to the headquarters. The NPMS has its own measure of rural income per capita in 2000 for the poor counties. By comparing income measures from the MOA and NPMS for the poor counties, we find that the two data sets are indeed independent. Furthermore, no systematic overreporting of post-treatment income for the poor counties in the MOA data is observed.

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.

5.1. DD estimates

We first present the simple DD estimates (Eq. (1)) of the program effect in Table 3 as a useful benchmark. The dependent variable is the change in log income per capita from 1994 to 2000. Column (1) provides the unadjusted correlation between the dependent variable and the designation status. Coefficient on treatment dummy is positive and statistically significant at the one-percent level, suggesting that the program is helping poor counties. Over the decade, designated poor counties had an 18-percent gain in rural per capita income.

To examine whether 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. After adjusting for covariates, the magnitude of the estimated program effect decreases slightly but remains statistically significant. Column (3) further includes provincial fixed-effects in the regression, allowing only within-province comparisons of counties. This exercise only leads to a marginal change in the size of the estimate. The implied program effect is a 15-percent gain in per capita income.

Finally, we estimate a more flexible DD specification that includes per capita income from 1990, 1991, and 1992. The results are shown in Column (4) of Table 3. With the additional pre-treatment income measures included, the coefficient of NP94 becomes very small and statistically insignificant.

5.2. RD estimates

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.

First-stage regressions (Eq. (2)) reported in Panel A of Table 4 suggest that initial eligibility, specifically per capita income relative to the poverty line, is a very strong predictor of program designation. Column (1) demonstrates results from the most parsimonious specification, where eligibility is the only regressor other than provincial and county-type dummy variables. Coefficient on eligibility is highly significant and large in magnitude, and the \( R^2 \) exceeds 0.60. Consistent with graphical evidence, eligible counties, specifically those below the official poverty line, have a significantly higher probability of being designated. The size of this estimate remains substantial (greater than 0.35) when a quadratic of the running variable and other controls are included (Columns (2)–(4)). These results indicate that a 1992 per capita income relative to the official poverty line is a strong predictor of the actual treatment status.

Estimates of the reduced-form model (Eq. (3)) suggest that program eligibility has a large impact on income. As shown in all four columns of Panel B, eligibility coefficients are always positive and significant, at least at the one-percent level. From a model with the richest set of controls in Column (4), the estimated impact of eligibility is 14.4%, equivalent to a 0.36 standard deviation of change in log income.

The IV estimates presented in Panel C show a large program impact on rural income. The IV estimates are merely the ratio of 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 running variable, the NP94 coefficient shrinks by approximately half of its size. Adding a quadratic of the running variable slightly increases the size of the estimate, as shown in Column (3). The coefficient is large in magnitude and significant at the one-percent level. Finally, the estimate is largely insensitive to include other county-level covariates (Column (4)). The estimated coefficient of 0.384 implies that during the 1994 to 2000 period, the program raised rural income per capita by 38.4% (0.96 standard deviation). 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 intercept shift at the threshold should not rely solely on particular functional forms of the running variable, or on data points in the

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12 We will test this assumption indirectly in the next section.

13 We combine the two RD thresholds into a single RD in our main specifications to obtain reasonable statistical power. In Table 9, we present results for the two RD thresholds separately. Both the first-stage and the reduced-form (and therefore the 2SLS) estimates are indeed different across the two groups. The 700-yuan threshold sample produces no meaningful estimates due to a very limited sample size. The 2SLS estimate for the 400-yuan threshold is around 0.6, whereas the combined 400- and 700-yuan threshold yields an estimate of around 0.4.
To determine if the program has a lasting effect on income growth, we use the change in log income per capita from 1994 to 2003 as the outcome variable. Remember that the program under consideration was not phased out until 2000. Instead of assessing the program’s impact immediately after its implementation, we are now allowing for a three-year lag when constructing the growth measure. If the program does have a persistent effect, estimates using this new outcome variable should be at least as large as those from the previous analysis for a shorter period.

5.4. Specification checks

For our empirical strategy to be valid, poor and non-poor counties around the eligibility threshold should have similar pre-program trends in income growth. For a first robustness check, we use the change in log income per capita from 1992 to 1994 as outcome of interest (recall that the program was not introduced until 1994). Without any differential trends between the treated and untreated close to the cutoff, the expectation is that the 2SLS estimates using eligibility as instrument for actual treatment should yield estimates statistically insignificant from zero. The IV estimates of the program effects on pre-program income growth are reported in Table 7. The structure of the table is similar to that of Tables 5 and 6, which displays estimates from various specifications using different functional forms.

Table 6

<table>
<thead>
<tr>
<th>Full sample</th>
<th>±1000 yuan</th>
<th>±500 yuan</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>NP94</td>
<td>0.384***</td>
<td>0.441*</td>
</tr>
<tr>
<td>(0.142)</td>
<td>(0.262)</td>
<td>(0.192)</td>
</tr>
<tr>
<td>Polynomial terms</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>Sample size</td>
<td>1610</td>
<td>1610</td>
</tr>
</tbody>
</table>

Notes: The dependent variable is the 94–00 change in log income per capita. NP94 is an indicator variable that is equal to one if the county was designated as a National Poor County in 1994. Eligible is an indicator variable that is equal to one if the county’s 1994 rural net income per capita was below the poverty line. County-level controls include minority county indicator, revolutionary base indicator, log population, log sown area per capita in 1990 and other labor market measures calculated using the 1990 Population Census. All regressions control for county-type and provincial dummies. Huber–White standard errors are in parentheses. * denotes statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 5

2SLS results for 94–00 change in log income per capita, using eligibility for NP94 as instrument.

<table>
<thead>
<tr>
<th>Full sample</th>
<th>±1000 yuan</th>
<th>±500 yuan</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>NP94</td>
<td>0.280**</td>
<td>0.193</td>
</tr>
<tr>
<td>(0.129)</td>
<td>(0.233)</td>
<td>(0.178)</td>
</tr>
<tr>
<td>Polynomial terms</td>
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<td>3</td>
</tr>
<tr>
<td>Sample size</td>
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<td>1607</td>
</tr>
</tbody>
</table>

Notes: The dependent variable is the 94–00 change in log income per capita. NP94 is an indicator variable that is equal to one if the county was designated as a National Poor County in 1994. Eligible is an indicator variable that is equal to one if the county’s 1994 rural net income per capita was below the poverty line. County-level controls include minority county indicator, revolutionary base indicator, log population, log sown area per capita in 1990 and other labor market measures calculated using the 1990 Population Census. All regressions control for county-type and provincial dummies. Huber–White standard errors are in parentheses. * denotes statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

extreme ends of 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 purpose of comparison, Column (1) reproduces the results from Column (4) (Panel C in Table 4). In the next column, a cubic 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) and (6) repeat the exercise with a “±500 yuan” window. The estimated effects are comparable in magnitude, but no longer statistically significant. Generally speaking, our estimates are largely insensitive to the choice of functional forms and samples.

Following Lemieux and Milligan (2008), we estimate a linear spline model for a successively narrower interval around the cutoff. Table 10 shows RD estimates with varying window widths for the first-stage, reduced-form and 2SLS regressions. All specifications control for the county-type dummy and the running variable with different slopes on each side of the discontinuity. Broadly speaking, the results are fairly robust for window widths no smaller than ±300 yuan. As we narrow the window widths further below ±300 yuan, the point estimates are more variable, but they remain positive and are generally consistent with the RD estimates based on larger samples.

5.3. Lasting effect

To determine if the program has a lasting effect on income growth, we use the change in log income per capita from 1994 to 2003 as the outcome variable. Remember that the program under consideration was not phased out until 2000. Instead of assessing the program’s impact immediately after its implementation, we are now allowing for a three-year lag when constructing the growth measure. If the program does have a persistent effect, estimates using this new outcome variable should be at least as large as those from the previous analysis for a shorter period.

Table 6 repeats the exercise presented in Table 5, this time with longer-run growth measure as outcome of interest. Overall, the 2SLS produces positive but unstable estimates for different specifications. Compared to the results from the last table, estimated program impacts on the income growth from 1994 to 2003 are slightly smaller in magnitude. These results, taken in conjunction with the “shorter-run” estimates from the previous table, indicate that the 8–7 Plan may have merely created a spurt in income that began to decay after only three years.

<table>
<thead>
<tr>
<th>Full sample</th>
<th>±1000 yuan</th>
<th>±500 yuan</th>
</tr>
</thead>
<tbody>
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Notes: The dependent variable is the 94–00 change in log income per capita. NP94 is an indicator variable that is equal to one if the county was designated as a National Poor County in 1994. Eligible is an indicator variable that is equal to one if the county’s 1994 rural net income per capita was below the poverty line. County-level controls include minority county indicator, revolutionary base indicator, log population, log sown area per capita in 1990 and other labor market measures calculated using the 1990 Population Census. All regressions control for county-type and provincial dummies. Huber–White standard errors are in parentheses. * denotes statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

In a local average treatment effect setting such as ours, the estimated effects apply to a population of compliers around the cutoff: those counties that are treated when they satisfy the cutoff rule, but would not otherwise be treated. Following Angrist and Pischke (2005), and Almond and Doyle (2011), we calculate the implied means of the complier characteristics. The compliers are quite similar to the population of counties. Complier counties tend to have lower average educational attainment and more minorities. Other characteristics show much smaller differences.
forms and samples within increasingly narrow intervals near the cutoff point. Overall, 2SLS estimates are very small in magnitude, and none are statistically significant at conventional levels, suggesting no differential trends around the cutoff.

The identifying assumption underlying the RD design is that prior to treatment, only the propensity scores of the counties 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 check for discontinuities in the predetermined county-level characteristics at the cutoff that may confound our estimates. Table 8 gives the estimated discontinuities for pre-determined county-level characteristics. In addition to the 1990 baseline characteristics contained in our original data set, we also generate several other county level labor market variables using the 1990 Population Census. Each entry is a discontinuity estimate for a different pre-determined characteristic, calculated in the same manner as for our baseline specification (a quadratic in the running variable with different slopes above and below the cutoff), but using no auxiliary controls. We find little evidence of any discontinuity in these pre-determined county characteristics. For a subset of the pre-determined variables, results are also shown in Fig. 6, which plots average county characteristics against the running variable.

The continuity assumption can be violated in the presence of precise sorting across the cutoffs. In 1994, the administrator used 1992 data to allocate treatment, and exact poverty lines were not released until after data were collected. Fig. 7 presents a histogram of the number of counties at each of the 10-yuan bins of 1992 rural income per capita. It appears that there is no discernible jump in the density at the poverty line, which suggests no evidence of nonrandom sorting across the cutoffs.

5.5. Rate of return

Our estimates of the program’s effect on rural income can be used to calculate the rate of return on public investments. To be conservative, we use the estimate from our preferred specification that has the richest set of controls, as reported in the last column of Table 4. Our estimates indicate that the program increased per capita income gains by 38.4% from 1994 to 2000. In real terms, central government poverty reduction funding during the same period averaged 19.1 billion yuan per year (in 1994 yuan), equivalent to 95.7 yuan per person per year. This comprises the central government’s outlays on credit, budgetary grants, and public funding during the same period averaged 19.1 billion yuan per year (in 1994 yuan), equivalent to 95.7 yuan per person per year. This comprises the central government’s outlays on credit, budgetary grants, and public
Based on the 38.4-percent impact on incomes, the poverty program on average increased rural income by 40.6 yuan per person per year. This finding suggests a 42-percent rate of return.

6. Conclusion

This study employs a panel data set to examine the performance of the 8-7 Plan, a poverty alleviation program introduced in the early 1990s 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 change in the county's per capita income. Based on a regression discontinuity approach, our analysis reveals that the program produced 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 by approximately 38% during the 1994 to 2000 period. This estimated program effect is considerably larger than that obtained from a conventional DD method, which indirectly reveals the important role of initial endowments in the path toward economic development.

To examine whether the program has a lasting effect on promoting income growth, we likewise estimate the program's impact on change in log rural per capita income from 1994 to 2003. The estimates of program effect in this longer time horizon are slightly smaller in size, and less significant for a number of model specifications than the "short-run" estimates. This finding suggests that the 8-7 Plan may only have generated a spurt in rural income that began to decay shortly after the program was phased out.

Admittedly, even with a valid regression discontinuity design, biases in the estimates can arise from interference because of spillover effects through local public finance (Chen et al., 2009). The national program

\[ \text{Fig. 6. Similarity of counties' pre-treatment characteristics around the eligibility cutoff.} \]

Notes: Panels refer to (from top left to bottom right) the following county attributes: minority county indicator, revolutionary base indicator, fraction with high school or more, fraction with college or more in 1990. The continuous line represents the predicted value from a second order polynomial in the running variable, and a dummy for counties above the cutoff. Each point represents the average outcome within 25-yuan intervals of the 1992 income per capita relative to cutoff.

\[ \text{Fig. 7. Histogram of the 1992 rural net income per capita.} \]

Notes: "400" and "700" indicate the two cutoff values used for 1994 designation. The histogram has a bin width of 10 yuan.

15 Calculations are based on statistics provided by Wang (2005).
would crowd out provincial government spending in the National Poor Counties, and increase spending in non-participating counties. In the presence of such spillover effects, our estimates would underestimate the program’s true impact. Unfortunately, more detailed local public finance statistics are currently unavailable for us to test for such spillover effects. Learning more about this issue remains an interesting topic for future research.

Constrained by data availability, assessment of the 8-7 Plan focuses on average income growth instead of poverty reduction. The use of county-level data prevents us from depicting the program’s distributional implications. More research is needed to examine these dimensions of the program effect.

References