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AND POVERTY ALLEVIATION: AN
EVALUATION OF CHINA'S POOR
VILLAGE INVESTMENT PROGRAM**

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ABSTRACT

Community-based Development and Poverty Alleviation: An Evaluation of China's Poor Village Investment Program

We conduct the first systematic evaluation of the world's largest community-based development program--China's flagship poverty alleviation program began in 2001 which finances public investments in designated poor villages based on participatory village planning. We use matching methods and a panel household and village data set with national coverage to compare changes from 2001 to 2004 in designated poor villages that began plan investments and in designated poor villages that had yet to begin plan investments. We find that the program significantly increased both government- and village-financed investments. While the program did not increase the income or consumption of poorer households, it did increase the income and consumption of richer households by 6.1 to 9.2 percent. We also find suggestive evidence that governance matters in the distribution of program benefits. Relative gains were greater for richer households in villages with more educated leaders, and higher quality village committees delivered greater benefits to both richer and poorer households.

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1. Introduction

Community-based development, defined in a recent review article as “an umbrella term for projects that actively include beneficiaries in their design and management” (Mansuri and Rao, 2004, p.1), has recently emerged as a popular model for development assistance.¹ For instance, depending on whether one uses a narrow (broad) definition, World Bank lending to community-based development projects increased from \$325 million (\$2 billion) in 1996 to \$3 billion (\$7 billion) in 2003 (Mansuri and Rao, 2004). This trend is one manifestation of the growing recognition that sound governance and local accountability may be critical for the success of public projects (see, for example, Easterly, 2002; World Bank, 1999).

Community-based development can be viewed as a form of decentralization that devolves authority to the most local community level. However, as pointed out by Bardhan and Mookherjee (2005, 2002, and 2000), the impact of decentralization depends critically on differences in the susceptibility to capture by elites or other interest groups of different levels of government. This is especially true when targeting the poor is a specific goal of development projects, which is often the case in developing countries. Concerns over capture are magnified in developing country settings where information and monitoring systems as well as local democracy or other forms of local accountability are often weak (Conning and Kevane, 2002). Heterogeneity among households within a community can also make it difficult to come to a consensus on how to prioritize public projects, reducing participation in civic activity, public goods provision, and targeting

¹ Mansuri and Rao (2004) distinguish this from community-driven development in which communities are fully in control of projects.

effectiveness (Alesina and La Ferrara, 2000; La Ferrara, 2002; Galasso and Ravallion, 2005; Banerjee and Somanathan, 2007; Araujo et al, 2008).

The limited available empirical evidence on the targeting success of decentralized programs has produced mixed results (see Mansuri and Rao (2004) and Bardhan and Mookherjee (2002) for reviews of this literature). Some papers have found that local communities are better able to target transfers to the poor in comparison to government agencies or that decentralization improves the responsiveness of public expenditures to the needs or demands of community members.² However, other research finds that community-based development projects are co-opted by local elites or do not effectively target the poor within communities.³ The World Bank's own assessment of the experience of popular Social Fund projects implemented in many different countries around the globe concluded that the projects were not particularly successful in targeting the poor (World Bank, 2002; Tandler, 2002).

Previous evaluations of the impact of community-based development projects suffer from key shortcomings. Many are based on one-time surveys that can track changes over time only by asking retrospective questions. Frequently, evaluations rely

² Galasso and Ravallion (2005) find that decentralized allocation by villages to households within the village was more pro-poor than government allocations of the subsidies across villages. Besley et al (2005) find that the poor are more likely to participate in public meetings about poverty transfers and to receive transfers conditional on participating. Alderman (2002) finds no evidence of local capture in a decentralized targeted social assistance program in Albania. Faguet (2004) shows that changes in investment patterns responded strongly to objective indicators of need after decentralization of spending authority to municipalities in Bolivia. Foster and Rosenzweig (2003) find that local democratization leads to greater responsiveness of public investments to the demands of village populations. Heller et al (2007) portray decentralization reforms as successfully increasing citizen participation in decision-making and the satisfaction of citizens in fund allocations in Kerala, India. Pradhan and Rawlings (2002) find that a number of social fund investments effectively targeted poor households.

³ Platteau (2004) describes local elites taking control of social fund expenditures in west Africa. Rao and Ibanez (2005) find that social fund projects in Jamaica poorly matched local preferences, especially of the poor, although most villagers expressed satisfaction with the projects. Paxson and Schady (2002) and Chase (2002) find that targeting within communities in Peru and Armenia were not well-targeted to the poor.

on subjective assessments which are susceptible to reporting biases and focus on processes or concepts that are difficult to quantify, rather than measurable welfare outcomes. None of the studies to date utilize household-level panel data to measure the impact of the program on incomes, consumption, and poverty. There thus remains a glaring scarcity of hard quantitative evidence on the impacts of decentralization experiments and community-based development projects (Bardhan and Mookherjee, 2002).

In this paper, we conduct the first systematic evaluation of the world's largest community-based development program--China's flagship poverty alleviation program begun in 2001 which finances public investments in designated poor villages based on participatory village planning. Prior to 2001, China's poverty investment programs had targeted poor counties since 1986, with no participatory component. By the end of 2001, 148,000 villages, or 21 percent of all rural villages in China, were officially designated as poor villages by the national government, entitling them to targeted investment funds financed by multiple government agencies. About 140 million persons, or 15 percent of China's rural population, live in officially designated poor villages. By 2004, the central government was allocating 32.7 billion yuan (about \$4 billion) annually to official poverty investment programs, equal to over 5 percent of the central government budget (Table 1).⁴ Under the new program, each designated village completes a public investment plan which is supposed to follow a procedure in which villagers vote to select projects. The government pledged to complete investments based on these plans by the end of the decade. The Chinese program is distinctive for being one of the few examples

⁴ This total counts the full value of subsidized loans including principal. The value of the subsidies is about 5 percent of loan value (loan interest rates are 2.88 percent), but subsidized poverty loans have low repayment rate (typically about 50 percent).

of community-based development initiated by the government of a developing country rather than international donors.

The Chinese case also is of interest because decentralization in rural China has been widely praised as a positive force in promoting economic development. Experimentation with the return to household farming in the late 1970s first occurred in very poor collectives in Anhui Province, leading to nationwide decollectivization in the early 1980s. Later, fiscal decentralization was credited for creating positive incentives for local governments to rapidly develop township and village enterprises. China later passed a village election law in 1987 to codify procedures to elect village officials every three years in order to increase local accountability. However, there are also accounts of corrupt behavior by local officials and excessive taxation of rural farmers, especially in poor areas (Bernstein and Lu, 2003).

This paper takes advantage of a unique household and village panel data set with national coverage for the years 2001 and 2004, a period which spans the introduction of China's poor village investment program. The research design focuses on a comparison of changes in officially designated poor villages that already began investments based on village planning from 2002 to 2004 with changes in designated poor villages that had yet to begin village investments by 2004 but for which there was an "intention to treat". We employ a nearest neighbor matching method which allows for bias adjustment and heterogeneous treatment effects to calculate difference-in-difference estimates of the impact of the program on program villages (the treatment effect on the treated).

We examine four research questions. First, to what extent did the program increase investments in public infrastructure? Second, what were the impacts of the

program on household income and consumption growth, and poverty? Third, how did the investment program affect the propensity of rural households to migrate? Finally, to what extent did governance factors, such as elite capture and the quality of village government, mediate the impact of the program?

The rest of the paper is organized as follows. In section 2, we introduce the data. In section 3, we describe the institutional features of the program and describe and analyze its implementation. Section 4 describes the empirical strategy for quantifying program impacts. Sections 5-8 motivate and present empirical results for the four research questions. Section 9 concludes.

2. Data

The household data we analyze are a subset of the National Bureau of Statistics (NBS) annual rural household survey, a nationally representative survey of over 60,000 rural households. Households living in approximately one third of all counties in China are included in the annual survey. As part of a World Bank poverty assessment project for China, NBS made available data for the years 2001 and 2004 for all of their sample households living in nationally designated poor counties (which account for about 25 percent of all counties in China) and sample households living in one third of sampled non-poor counties. It turns out that 97 percent of the 2001 and 2004 samples are surveyed in both 2001 and 2004, making it possible to construct a two-year panel for nearly all sample households. The household variables include net income, consumption expenditures, education of the household head, household size, number of laborers, number of out-migrants, and cultivated land area. Income and expenditure measures are

based on self-recorded diaries kept by sample households throughout the year, and so are more accurate than many household surveys based on one-time interviews.

A special-purpose village survey also was conducted in early 2005 in all of the 3036 villages which contain the sample households made available for the poverty assessment. NBS surveys 10 households per sample village. The village questionnaire included detailed questions about all village investments by type of project each year from 2001 to 2004, regardless of whether they were financed by official poverty programs. Investments were disaggregated by financing source, including government investments, village investments, and the amount of village corvee labor provided for investment projects. In designated poor villages, detailed questions also were asked about the timing of the implementation of the poor village investment program.

To evaluate the impacts of the investment program, we restrict attention to designated poor villages and households living in designated poor villages. This reduces our sample to 666 designated poor villages and the 5500 households surveyed in those villages in both 2001 and 2004.

3. Implementation of China's poor village investment program

China's poor village investment program began in 2001, under the direction of the Leading Group Office for Poor Area Development (LGOPAD) under China's State Council. The Leading Group is a super-ministerial body whose members comprise top officials from different ministries involved with poverty alleviation work. The Office of the Leading Group has overseen regionally targeted government poverty investment programs since 1986 and also has played a key role in implementation a series of World

Bank poverty reduction projects. Prior to 2001, both government and World Bank poverty projects targeted officially designated poor counties. Evaluations have found that both types of projects successfully increased rural incomes but that the government's poor county designations did not target the poor effectively (Jalan and Ravallion, 1998; World Bank, 2001; Park, Wang, and Wu, 2002; Ravallion and Chen, 2005).

The new program targeting villages was in part a response to prior criticisms of the targeting effectiveness of poor county designations. The decision to base investments on participatory village plans reflected concerns that investment projects were not sufficiently focused on the needs of China's poor, and was influenced by active engagement by international donors, especially the Asian Development Bank, which provided technical assistance for the design of the new program.

According to official guidelines, poor villages were selected according to a weighted poverty index based on eight indicators.⁵ However, local governments could alter the weights or the indicators based on local circumstances, and often were constrained by the limited administrative data available at the village level. Given this discretion, the multitude of indicators used, and the information constraints, it is perhaps not surprising that there was substantial mistargeting of villages when evaluated on the basis of income or expenditure per capita (World Bank, 2009). The regional breakdown of designated poor villages is presented in Table 2. Nearly half of all poor villages are in western China even though the West accounts for only 28 percent of China's villages. The percentage of villages designated as poor is 41 percent in the northwest, 32 percent

⁵ The eight indicators were: grain production per capita, cash income per capita, percent of low quality houses, percent of households with poor access to potable water, percent of natural villages with reliable access to electricity, percent of natural villages with all-weather road access to the county seat, percent of women with long-term health problems, percent of eligible children not attending school.

in the southwest, 26 percent in the northeast, 22 percent in the central region, and 8 percent in coastal areas.

China's official poverty alleviation programs have emphasized the financing of public investment projects in poor, rural areas. In addition, various government agencies administer an array of other policies and programs that may benefit the poor,⁶ and international and domestic donors and NGOs also have undertaken poverty reduction projects. Established in 1986, the three main government poverty investment programs are administered by different government agencies, and have historically emphasized different types of investments. The subsidized loan program provides cheap credit to households and enterprises for income-generating projects and is administered by the Agricultural Bank of China, with interest subsidies paid by the Ministry of Finance. The Food-for-Work (FFW) program finances rural infrastructure projects, usually by paying for material costs while villages contribute corvee labor, and is administered by the National Development and Reform Commission (formerly the State Planning Commission) at various administrative levels. The program initially emphasized road and drinking water projects, but over time the scope of projects has widened. Finally, the Ministry of Finance provides earmarked budgetary grants to local governments to finance a wide range of public investment projects in poor areas. Table 1 describes central government spending on the three main poverty investment programs since the start of village planning in 2001.

The poor village investment program was intended to integrate and coordinate investments under the three official poverty investment programs. However, consistent

⁶ For example, the Ministry of Civil Affairs provides disaster relief, and assistance for the elderly indigent and the disabled, the Ministry of Education provides scholarships for poor children, and the Minority Affairs Committee funds projects in poor minority areas.

with previous experience, coordination among government agencies has proven difficult because each agency is reluctant to relinquish authority over the resources they control and the LGOPAD has limited ability to influence other government agencies. As a result, the extent to which investment funds from the three programs are integrated into village plans depends on local bureaucratic politics. Field interviews suggest that subsidized loans are rarely made in conjunction with village plans, FFW projects are sometimes coordinated with village plans, and budgetary grants closely follow village plans (Wang, 2005). The lack of inter-agency coordination is one reason why village investment plans often have budgets that far exceed the amount of actual investments that are financed. It also suggests that actual village investments undertaken could reflect not just the needs of villagers but also the mandates of government agencies.

According to official training materials (Gao, Wang, and Huang, 2001), the following principles should guide the village planning process: projects helping the poor should be favored; participation of households and different groups (e.g., women) should be emphasized; plans should integrate resources from different sources; plans should be for a 3-5 year time horizon and reflect local conditions and causes of poverty; and plans should be developed following standardized procedures set by the county government. Specific procedures are recommended for completing village investment plans. First, an analysis of the causes of poverty in the village should be conducted and possible solutions identified, based on analysis of village-level data and participatory workshops with 10 to 20 villagers. Next, with the support of technical experts, a SWOT (strength, weakness, opportunity, and threat) analysis and then a feasibility study should be completed for potential projects. Projects are selected by a plenary session of the entire

village, with the views of poor households given added weight. Once projects are chosen, a more detailed assessment is completed describing project beneficiaries, project requirements, project implementation (schedule, budget, labor allocations), and a plan for monitoring and evaluation. The final product is the village planning document, which serves as a detailed blueprint for the investment projects to be undertaken.⁷

Field research in different regions revealed that actual practice rarely adheres to all of the guidelines. Plans are often designed by a group including village committee members, small group (hamlet) leaders, Communist Party representatives, and household representatives, most often under the supervision of township government officials who receive training from county LGOPAD staff. Qualitative research conducted in 12 villages in six provinces found that many households were unaware or did not participate in village planning, and that problems which adversely affected project outcomes included insufficient funding, selection of inappropriate projects, and lack of infrastructure maintenance (World Bank, 2009).

How many poor villages actually began investments based on village planning by 2004? Figure 1 describes the share of poor villages completing plans and the share of villages starting investments based on village plans in each year from 2002 to 2004. By the end of 2004, 55 percent of poor villages (366 sample villages) had completed plans and 37 percent of poor villages (244 sample villages) had begun investments based on the plans.⁸ A main reason that most villages had yet to begin planned investments three

⁷ Village planning documents should have eight sections: 1) introduction; 2) current situation; 3) guidelines and objectives; 4) project identification; 5) total budget; 6) supporting system; 7) implementation plan; 8) organization and management.

⁸ The Leading Group Office for Poor Area Development reports that a higher percentage of poor villages (83 percent) had completed village plans but a lower percentage of poor villages (32 percent) had begun investments based on the plans by the end of 2004.

years after the program had begun is that county governments generally concentrated annual program allocations in a subset of villages. The decision to fund village plans sequentially rather than simultaneously was due to practical concerns such as economies of scale in investments and the fixed costs associated with supervising the design and implementation of plans in each village. The village data confirm that that increase in “treated” villages over time was due to a gradual expansion of investments in new villages within counties rather than expansion across counties.

We focus on a comparison of designated poor villages with and without planned investments. This design avoids selection biases associated with poor village designation, but remains subject to selection biases associated with the timing of village plan financing. Our prior is that the former biases are likely to be far more problematic than the latter. Poor village designation is a politicized process in which substantial resources are at stake. Moreover, the highly visible designations are frequently used as the basis for targeting for other projects and policies initiated by local governments, social organizations, or international donors. If that is the case, using non-poor villages as controls could lead us to confound the effects of the investment program with the impacts of other targeted programs and policies.

Nonetheless, we remain concerned about the endogeneity of the timing of investment financing. Which poor villages tend to get plan-based investments first? Comparing mean characteristics of villages with and without plan investments and restricting attention to differences that are statistically significantly different from zero, we find that villages that began investments by 2004 were better off villages. They have more investment per capita prior to the program, have greater agricultural productivity,

more households with telephones, shorter distance to market, are less likely to be in the mountains, and have greater income per capita (Appendix 1). In Table 3, we present estimates of multivariate models of the determinants of whether a designated poor village began planned investments. The first two models are a logit model and a linear regression model including provincial fixed effects. The third model adds county fixed effects to the linear regression to examine how counties decide on which villages to fund first. Not very many variables enter significantly or consistently across specifications, consistent with a lack of substantial selection bias. Controlling for provincial fixed effects, the factors that predict having started planned investments include being in mountainous terrain, being closer to a market, having more telephones, and not having paved roads. With county fixed effects, villages closer to towns, not having roads, and having paved roads are more likely to start plan investments by 2004. Overall, starting investments earlier is not strongly associated with many variables and the variables that are influential are split between positive and negative factors, making it difficult to generalize about whether to expect positive or negative selection bias.

4. Empirical strategy

To estimate the treatment effect on the treated, we assume that selection is based on observables, so that systematic differences in outcomes between treated and control observations with the same values of the covariates can be attributed to the treatment. A number of regression-based and matching estimators have been proposed to estimate treatment effects under this unconfoundedness, or exogeneity assumption. Matching methods have the advantage of relying less on distributional or parametric assumptions,

which minimizes bias, but possibly at the expense of less precision (Imbens, 2004; Abadie and Imbens, 2006a). They do not rely on linearity or other functional form assumptions relating outcomes to a set of covariates, and are less subject to bias caused by a lack of common support.

Although the exogeneity assumption may seem strong, specific features of the data and estimation algorithm can significantly improve the reliability of matching estimators, producing results that have been shown to be very close to those based on a randomized design (Smith and Todd, 2005; Abadie and Imbens, 2006a). Smith and Todd (2005) counsel that geographic mismatch between matched observations should be avoided, common survey questions and survey methods should be used for treatment and control groups, and difference-in-differences matching is preferred to cross-sectional matching, especially in the presence of the first two problems. Abadie and Imbens (2006a) propose a method to eliminate bias caused by imprecise matching of covariates between treatment and control observations using nearest neighbor matching with a fixed number of matches. Another important advantage of their estimator, which we use for the matching estimates presented in this paper, is that they derive an explicit analytical formula for the asymptotic variance of the estimated treatment effect, so that standard errors are asymptotically ($N^{1/2}$ -) consistent.⁹

For each treatment observation i , we can write the following expression for our estimate of outcome Y_i absent treatment ($\tilde{Y}_i(0)$) and if treatment occurred ($\tilde{Y}_i(1)$):

⁹They show in other work that commonly used bootstrapped standard errors are inappropriate for matching methods with a fixed number of matches (Abadie and Imbens, 2008), and that their estimator performs well even in fairly small samples (Abadie and Imbens, 2006b).

$$\tilde{Y}_i(0) = \frac{1}{M} \sum_{j=\tilde{S}_M(i)} (Y_j + \hat{\mu}_0(X_i) - \hat{\mu}_0(X_j)) \quad (1)$$

$$\tilde{Y}_i(1) = Y_i. \quad (2)$$

The function $\hat{\mu}_0$ is a fitted linear regression function of Y_j on covariates X_j using the control observations. It is used to adjust the counterfactual estimates to account for differences in the matching variables for each treatment observation (X_i) and its matched control observations (X_j). This correction leads to an unbiased estimate of the average treatment effect on the treated, which can be expressed as:

$$\tau = \frac{1}{N_1} \sum_{W_i=1} (Y_i - \tilde{Y}_i(0)), \quad (3)$$

where N_1 is the number of treated observations and $W_i=1$ signifies that the observation is in the treatment group (Abadie and Imbens, 2006a).

In making specific methodological choices, our approach is to minimize potential bias whenever possible, potentially at the cost of lost efficiency. To minimize geographic mismatch, we enforce exact matching by province. Data for both treatment and control villages and households come from the same survey administered by the NBS. The panel data enable difference-in-difference comparisons. Each treatment observation is matched to 3 control observations with replacement, which is few enough to enable exact matching by province for nearly all observations but enough to significantly reduce the asymptotic efficiency loss (Abadie and Imbens, 2006a).

Matching is based on a set of covariates which are time-invariant or were measured before the start of the program. The weighting matrix uses the Mahalanobis metric, which is the inverse of the sample variance/covariance matrix of the matching

variables. We choose the following set of 17 matching variables for village level matching:

- 1) mean years of schooling of household heads
- 2) log of total investment per capita in 2001
- 3) log of government investment per capita in 2001
- 4) indicator variable for whether in designated poor county
- 5) log of county population
- 6) county agricultural productivity index, based on predicted grain yield from regression on geographic variables: slope, elevation, rainfall, and temperature
- 7) percent of village hamlets with telephone access in 2001
- 8) percent of village hamlets with access to safe drinking water in 2001
- 9) percent of village hamlets with road access in 2001
- 10) percent of village hamlets with paved road access in 2001
- 11) kilometers to nearest market in 2001
- 12) indicator variable for mountainous region
- 13) indicator variable for whether an old revolutionary base area
- 14) kilometers to nearest township
- 15) kilometers to nearest county
- 16) mean log(income per capita) in 2001 (from household data)
- 17) village poverty headcount rate in 2001 (from household data)

The relatively large number of matching variables, in addition to perfect matching by province, does raise concerns about lack of common support, since few control villages are likely to share similar values for all covariates. However, bias adjustment using these variables helps control for such differences in the covariates. To provide a sense of the comparability between treatment and control observations, in Appendix Table 1, we provide output from a balancing test based on matching using logit-based propensity scores.¹⁰ The table describes the mean values of all covariates for treatment and control observations before and after matching. We use the propensity scores as a diagnostic tool to restrict the sample used in each matching estimation to those with common support (at least one control observation has an equally high propensity score). A plot of the

¹⁰ This is not the matching used for estimation (which employs a Mahalanobis weighting matrix), but is more readily calculated using the STATA `psmatch2` command.

propensity score distribution for the full set of treatments and possible controls is provided in Appendix Figure 1. The figure also shades treatment observations that lack common support, which are excluded in the estimation.

The nearest neighbor matching estimator with panel data may still be subject to bias if unobservables associated with selectivity of villages starting plan investments earlier are correlated with changes occurring in villages over time. For example, unobserved leadership ability could lead villages to start investments earlier and also use investments more effectively. However, to the extent that such leadership ability also produces higher rates of investment or higher village income per capita before the program began, it will be controlled for by the matching variables.

5. To what extent did the program increase public investments in targeted villages?

It might seem obvious, almost tautological, that the program would increase public investments in targeted villages. However, there are several interesting aspects of this question. First, given the concerns that some funding agencies are not coordinating their poverty investments with village plans, it is worthwhile to verify that the targeted villages are, in fact, receiving greater investment funding. Local governments also could decide to reallocate non-poverty investment funds away from poor villages and so dilute the impact of the program. It is also of interest to examine whether government financing of village projects is a substitute or complement to villages' own financing of projects. In Chinese villages, village committees selected by villagers have some discretion to raise and spend funds independently of the government. There is a natural incentive for a village to substitute government financing for its own, which could enable it to spend

more funds in other areas, such as for social assistance. On the other hand, government-funded projects often require matching funds from villages, either in the form of village financing or village corvee labor. Government-financed projects could also raise the returns to complementary investments.

In the full (not matched) sample of designated poor villages, program villages that began plan investments by 2004 saw annual investment per capita increase from 70 yuan before the program to 121 yuan after it began compared to an increase from 54 yuan to 73 yuan in designated villages yet to start plan investments.¹¹ The share of villages with zero monetary investments fell from 33 percent before the program began to 11 percent afterwards, compared to a decline from 37 to 24 percent in villages yet to begin plan investments. Government investment per capita increased by 38 yuan in villages that began plan investments compared to 22 yuan in villages that did not. Interestingly, while village-financed investment per capita increased by about 4 yuan (from 9 to 13 yuan) in villages that began plan investments, it declined by 3 yuan (from 19 to 16 yuan) in villages that did not.¹² This suggests that the program crowded in locally financed investment.

Table 4 presents the estimated impacts of the investment program on the amount of village investments using nearest neighbor matching. Total investment includes the value of corvee labor days based on the mean daily male labor wage rate in 2004 as reported in the village questionnaire. Total monetary investment is inclusive of

¹¹ Annual averages before and after plan investments began are calculated based on the actual year of initial treatment for villages that began plan investments. On average, treatment is for two years. For villages without planned investment, 2001 and 2002 are defined to be before and 2003 and 2004 are defined to be after.

¹² These changes are even more pronounced if we look at changes in total investment amount in treated and untreated villages; mean village investment increased from 10.9 to 15.8 thousand yuan in treated villages compared to a decline from 23.6 to 16.6 thousand yuan in untreated villages.

government and village investment. The results confirm that the beginning of village investments based on the plan was associated with a very substantial increase in the amount of total investment per capita—by 131 percent on average.¹³ Both government- and village-financed investments increased significantly, by 110 percent and 50 percent, respectively. The value of village corvee labor also increased by 29 percent in treated villages. These impacts are all positive and statistically significant, leading us to conclude that government financing was a complement rather than a substitute for village financing in practice. Complementarity could be due to required local matching contributions for government-financed projects or to positive interactions between the returns to government and village project spending.

Table 4 also reveals some interesting contrasts in village-level financing of investments in western versus non-western regions. When splitting the sample, the estimated percentage increase in total investment, total monetary investment, and total government investment were very similar for villages in the West and non-West. This is driven by the prominence of government investment in total investment. However, there is a sharp contrast in the change in village monetary investments and village corvee labor in the West and non-West. In Western villages, there is no noticeable increase in village monetary investment (9.2 percent, not statistically significant) but a significant increase in village corvee labor investments (51 percent, highly statistically significant). In non-Western villages, just the opposite occurs. There is a significant increase in village monetary investment (119 percent), similar in magnitude to the increase in government-financed investment, but a much smaller increase in the value of corvee labor days (19

¹³ Percentage increases in investment should not be interpreted literally because for villages with initial investment of less than one yuan per capita (including many zeros), log investment per capita was set equal to zero.

percent, not statistically significant). Thus Western villages appear to match government investment with labor contributions while non-Western villages match with local financing. This is consistent with the lack of local revenue availability in the poor Western region.

6. What were the impacts of the program on household income, consumption, and poverty?

A main goal of China's poor village investment program is to alleviate poverty, so quantifying the impacts of the program on household income and consumption levels is a central evaluation task. However, a few caveats deserve mention at the outset regarding ways in which our evaluation of household impacts is likely to underestimate the true benefits of the program. First, investments under the program began in 2002; by the end of 2004 the mean duration of program investments in villages that began investments was only 2 years. This means that most "treatment" villages had not completed plan investments by 2004. It is also reasonable to expect lags in the effects of public investments as households gradually learn how best to utilize access to new public goods.¹⁴ Second, important benefits of the program are unlikely to be fully captured by household income or consumption measures, such as health benefits of clean drinking water or higher quality of education and health services. Still, many of the public investments do aim to raise productivity and improve market access and information (i.e.,

¹⁴ Lagged effects also are a potential identification concern because the lack of data prior to 2001 precludes us from directly controlling for pre-existing trends. This means that estimated treatment effects could reflect lagged effects of greater or better quality investments in treated villages prior to the program. However, to the extent that such effects are related to the level of 2001 total investment per capita or government investment per capita, they are controlled for through their inclusion among the matching variables.

roads, irrigation, land improvement, communications) making it meaningful to investigate the extent to which the program increases the income and consumption of richer and poorer households.

How did village planning affect the well-being of rural households in program villages? Table 5 reports the estimated impacts on income and consumption growth. We report results for all households, poorer households, and richer households. To maximize the sample size for within-village comparisons of rich and poor, we use median income to divide the sample into richer and poorer households. Even so, some villages have only rich or only poor households. Of the 609 villages in the matched sample, 569 have at least one poor household, 505 have at least one rich household, and 465 villages have both rich and poor households. We report results both for the maximum sample sizes and for the restricted sample of villages with both rich and poor households. A comparison of results for rich and poor households using the restricted sample is analogous to controlling for village fixed effects. In addition, we may consider villages with both rich and poor households to be more heterogeneous with respect to poverty, and so a comparison of estimates for the restricted and maximum samples can provide clues to how program impacts may vary with this dimension of village heterogeneity.

The results are quite striking. There are no statistically significant effects on mean income or consumption growth for the full sample of villages. However, whether we use the maximum or restricted samples, we find significantly positive effects on the income and consumption of richer households, but small and even negative (and statistically insignificant) effects on poorer households. For richer households, the program increases both household income per capita and expenditures per capita by 6.1

(9.2) percent using the maximum (restricted) sample. The more positive impacts for the restricted sample suggest that the program created larger benefits for richer households in villages that also had poorer households, which is consistent with rich households capturing benefits for themselves. Not surprisingly, when we estimate the impact of the program on changes in village poverty headcount rates, we find that they are close to zero and statistically insignificant.

We also estimated several variants of the above specification (not reported). First, we looked at program impacts separately for relative income groups defined as being below or above the median income per capita in the village. It was still the case that the program increased incomes more for the relatively rich than for the relatively poor, but the differences were much more attenuated. This suggests that absolute income thresholds matter, either because they prevent households from participating in or taking advantage of investment opportunities or because they correspond with diverging interests or preferences. We also used China's official low income line rather than median income in the sample to divide rich and poor. Using that standard, the average poverty headcount rate in the sample of designated poor villages was 32.6 percent in 2001. The results do not yield statistically significant impacts; consumption growth remains higher for the rich, but income growth is similar for both rich and poor. The weaker results may be due to the reduced sample of poor households associated with a lower line, or the existence of important threshold effects at income levels above the official low income line. Finally, we estimated program impacts separately by region. We find larger effects of the program on the income and consumption of richer households in non-western villages.

7. How did the investment program affect the propensity to migrate?

Given the rapid speed of structural change occurring in China, especially in coastal regions, increasing labor mobility of the poor and including them in China's rapid industrialization and urbanization may be an important vehicle for poverty reduction. At the same time, recent research suggests that households with poor endowments are less likely to migrate (Du, Park, and Wang, 2005; World Bank, 2009). Investments that raise agricultural productivity are expected to reduce the relative returns to migration, but in subsistence-oriented households, higher agricultural productivity could also release surplus labor for use elsewhere. Investments in roads and education raise both agricultural and nonagricultural productivity while reducing the costs of migration, so that the effects on migration propensity are ambiguous in theory. Some government officials are concerned about congestion costs in cities caused by rapid increases rural-urban migration. For all of these reasons, it is of interest to understand whether poverty investments are inhibiting or facilitating labor migration out of poor rural areas.

Turning to the results on migration propensity presented in Table 5, we find that the program has a statistically significant negative effect on the migration likelihood of rich households, reducing household migration probability by 3.1 percent using either the maximum or restricted samples. The program has a much smaller and statistically insignificant effect on the migration probability of the poor. This may be because the rich disproportionately capture local economic opportunities, raising their opportunity cost of migration, or because the poor are more likely than the rich to migrate with better roads or communication, offsetting higher local returns.

8. To what extent did governance factors mediate the impact of the program?

Few question the importance of institutions and governance for the effective implementation of development projects. The presence and characteristics of local leadership can strongly influence project outcomes (Besley et al., 2004; Khwaja, 2009). There are two ways in which governance may matter. First, governance may be influenced by the program, for instance if the exercise of going through the village planning process makes villagers feel more engaged and enthusiastic about participating in civic activities. Or, the infusion of new investment funds could raise the stakes of leadership and so attract more individuals to run for village office or participate in village decision-making, whether for selfish or unselfish reasons. Second, good governance or leadership may influence the effect of the program on outcomes of interest, perhaps through better project design or better maintenance of infrastructure.

The village survey included a detailed set of questions about village leaders, the village committee (the main decision-making body in the village), and the frequency of and attendance at meetings of the village committee and village assemblies (meetings of all villagers). We conducted a principal components analysis of eight governance variables, which resulted in three principal factors, each with an intuitive interpretation. In the empirical analysis we focus on two governance variables.¹⁵ The first is the education level of village leaders, which heavily weights three variables about equally—years of schooling of the village party secretary, years of schooling of the village mayor,

¹⁵ The third principal factor heavily weights the frequency of and attendance rate at village-wide meetings. This factor was not found to be significantly associated with program impacts. One interpretation of this result is that village-wide assemblies are related in a fairly mechanical way to the size (population and area) of the village. Large villages rarely organize such assemblies.

and the share of village committee members with a middle school education or above. In China village mayors are elected every three years, as are village committee members. Party secretaries are usually appointed by township officials at the same time as village leader elections. Education of village leaders is an appealing variable for testing whether elites in the village act on the behalf of the rich, the poor, or both.

We label the second village governance variable “the quality of the village committee.” This factor heavily weights the number of members of the village committee and the frequency of meetings of the village committee. Both of these variables are likely to be positively associated with the level of interest among villagers in participating in governance activities, the amount of activities requiring organization and discussion, and the extent of consultation and consensus-building that goes into village decision-making. Because of political sensitivity and the difficulty of finding reliable indicators of the quality of elections, we did not ask questions about recent village elections.

We first treat the governance variables as outcome variables and test whether the program had an impact on village governance. The results are reported in Table 6. We find that the program had a significant positive effect on the education level of village leaders (village leaders have 0.622 more years of schooling in treated villages), but no significant effect on the quality of the village committee.

A key challenge in analyzing the relationship between governance and program impacts is that we do not have governance measures that predate the implementation of the village planning program. As a result, we cannot directly control for governance outcomes prior to the program; our measured impacts thus could reflect selectivity into

treatment, rather than impacts of the program. This is less of a concern for the second governance variable, for which we do not estimate a significant treatment effect even though any endogeneity bias would most likely be upward (for example, if undertaking more plan investments require more meetings). For the education of village leaders, the potential problem is that villages with educated leaders are more likely to be treated (i.e., begin investments earlier). This will not be a problem if the matching variables adequately proxy for possible confounding factors that may arise if we fail to control directly for village leader education prior to the program. In support of the effectiveness of the existing matching variables, we point out that they include the average years of schooling of household heads, 2001 log investment per capita, 2001 mean log income per capita, and the 2001 village poverty headcount rate. Thus, the effect of the education of village leaders is not likely to proxy for the overall education level of villagers, and the matching variables effectively control for any differences in unobserved initial governance inasmuch as they influence initial village investment or the level and distribution of income. However, we cannot rule out the possibility of selection bias influencing the results, although it would have to affect richer and poorer households differently to explain the within-village comparisons of richer and poorer households. The results are certainly consistent with the proposition that elites (the more educated) will become more actively engaged in the political process if external projects raise the returns or stakes to their participation.

We are more concerned about whether governance characteristics influence whether the program helps the poor, a question which is more relevant for assessing whether the program is achieving its self-proclaimed objectives. In addition, by

comparing how governance affects program impacts on the rich and poor, we can also focus on within-village comparisons that are not subject to bias from unobserved village-level factors, including selection bias associated with initial governance or endogenous average treatment effects of the program on village governance. The analogy in a linear regression specification would be to look at the triple interaction of the treatment variable, the governance variable, and initial poverty status while controlling for village fixed effects and initial poverty status.

We implement this idea by restricting the sample as before to villages with both rich and poor households, and then splitting this restricted sample evenly into high governance villages and low governance villages. Using these subsamples, we then estimate four treatment effects—for poorer households in low governance villages, poorer households in high governance villages, richer households in low governance villages, and richer households in high governance villages. It is then straightforward to calculate the differences and difference-in-differences of these impact estimates to examine whether better governance helps the poor, whether it helps the rich, and whether it helps the rich more than the poor.

The results are presented in Table 7. We focus on changes in income per capita and changes in consumption per capita. We find that when leaders are more educated, the benefits of the program, whether measured by income or consumption, decline for the poor and increase for the rich. For income, there is a large difference in program impacts for richer households when leaders are more educated versus less educated, and an even greater difference in impacts on poorer and richer households when village leaders are less educated (this latter difference is statistically significant). For consumption, we find

that the impact on poorer households is actually negative (and statistically significant) when leaders are more educated, which could be due to their being asked to pay more ad hoc fees to support matching investments for new projects or other purposes. The difference in treatment effects for poorer households when village leaders are more educated versus less educated is statistically significant, as is the difference in the program impacts on richer versus poor households when leaders are more educated. More importantly, the preferred difference-in-differences estimate comparing the relative effect of more educated leaders versus less educated on richer versus poorer households is large for both income and consumption, and statistically significant for consumption.¹⁶ These results support the idea that educated leaders are more likely to help the rich than the poor.

The influence of the quality of the village committee is strongly positive for both poor and rich households, whether one looks at income or consumption. The difference in income gains from the program in villages with high versus low quality village committees is over 31 percent for income and over 13 percent for consumption. The difference-in-differences estimate of the effect of village committee quality on the program benefits to richer versus poorer households is small and not statistically significant.

9. Conclusions

Taken together, our evaluation of China's poor village investment program finds little evidence that participatory decision-making has helped the poorest of the poor benefit more from China's targeted investment program or played a major role in rural

¹⁶ We conduct a simple t-test for whether coefficients differ using the estimated standard errors.

poverty reduction during its first three years of operation. We do find evidence that governance factors matter in the distribution of program benefits. More educated leaders are likely to favor richer households over poorer households, and high quality village committees deliver greater benefits whether for the richer or for the poorer. Whether the poor lack the capacity to take advantage of public investments or are being systematically excluded from participating in community-based development projects remains an open question. The results suggest that the program may benefit from measures to increase monitoring effectiveness and accountability, perhaps by mandating individually targeted benefits that are easy to track. There also may be scope for increasing program impacts by improving village governance factors that mediate program impacts. Our results demonstrate that decentralization and community-based development are not panaceas, but must be understood within the specific context of local governance and institutions.

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Figure 1
Poor village completion of plans and start of investments based on plans,
2001 to 2004

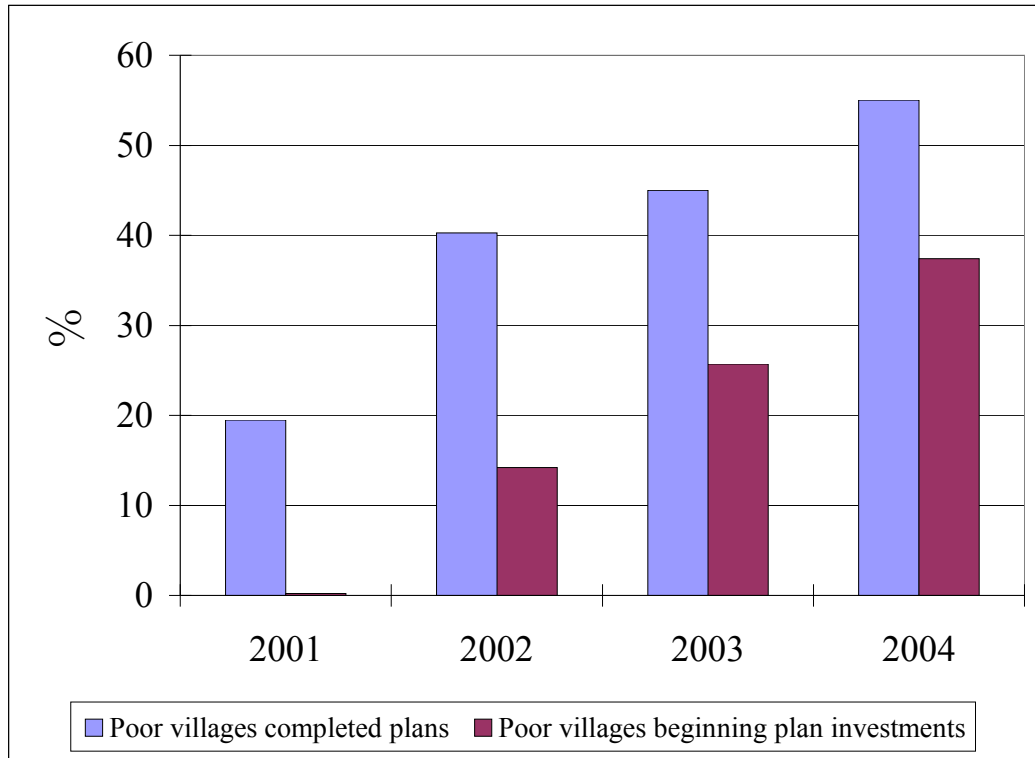


Table 1
Central government funding for poverty alleviation programs, 2001 to 2004
(billion Yuan)

Year	Subsidized loans	Food for work	Budgetary funds	Total
2001	18.5	6.0	6.0	30.5
2002	18.5	6.0	6.6	31.1
2003	18.5	6.0	7.4	31.9
2004	18.5	6.0	8.2	32.7
Total	74.0	24.0	28.2	126.2

Source: LGOPAD and MOF

Table 2
Number of designated poor villages, by region

Region	Total no. of villages	No. of designated poor villages	% of villages designated poor	Share of poor villages (%)
Coastal	249,723	20,698	8.3	14.0
Northeast	35,540	9,182	25.8	6.2
Central	225,964	48,950	21.7	33.0
Southwest	132,879	42,647	32.1	28.8
Northwest	65,151	26,654	40.9	18.0
Total	709,257	148,131	20.9	100.0

Source: LGOPAD

Table 3
Determinants of starting village plan investments by 2004

	logit		OLS		OLS	
	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
Mean yrs of schooling	0.083	0.064	0.014	0.012	0.007	0.013
Ln('01 tot. inv. p.c.)	0.073	0.070	0.013	0.014	0.007	0.016
Ln('01 gov. inv. p.c.)	0.008	0.083	0.002	0.016	-0.011	0.018
Poor county	0.151	0.384	0.036	0.078		
Ln(county pop.)	-0.321	0.218	-0.057	0.040		
Ln(county ag prod.)	0.486	0.836	0.123	0.169		
'01 share w/telephone	*0.007	0.003	**0.001	0.001	0.001	0.001
'01 share w/water	-0.002	0.003	0.000	0.000	0.000	0.001
'01 share w/road	-0.001	0.003	0.000	0.001	*-0.001	0.001
'01 share w/pave rd.	*-0.007	0.004	-0.001	0.001	*0.002	0.001
Distance to market	*-0.025	0.014	*-0.004	0.002	-0.001	0.003
Mountainous	***1.063	0.326	**0.203	0.061	-0.028	0.090
Rev. base area	-0.334	0.290	-0.069	0.057	0.212	0.169
Distance to town	-0.013	0.015	-0.002	0.003	** -0.008	0.003
Distance to county	0.007	0.011	0.001	0.002	0.000	0.002
Ln('01 inc. p.c.)	0.331	0.319	0.059	0.057	-0.006	0.061
'01 poverty rate	0.348	0.641	0.059	0.123	-0.014	0.136
Province fixed effects		X		X		
County fixed effects						X
N		625		625		625

Table 4
Effect of village investment program on growth in village investment per capita,
by financing source (village nearest neighbor matching estimates)

Financing source	All China	West	Non-west
Total investment	***1.31 (0.264)	***1.52 (0.288)	***1.50 (0.554)
Total monetary investment	***1.30 (0.265)	***1.46 (0.286)	***1.46 (0.548)
Govt monetary investment	***1.10 (0.185)	***1.15 (0.279)	***1.17 (0.308)
Village monetary investment	**0.503 (0.210)	0.092 (0.145)	**1.19 (0.519)
Value of corvee labor days	**0.286 (0.131)	***0.505 (0.057)	0.186 (0.210)
N	609	386	223

Notes: ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels. All variables defined as log differences.

Table 5
Effect of village investment program on household income p.c., consumption p.c.,
and share of labor that migrates
(village nearest neighbor matching estimates)

	$\Delta \ln(\text{inc. pc})$	$\Delta \ln(\text{cons. pc})$	$\Delta \text{migration-share}$
All villages			
All	0.007 (0.029) 609	-0.008 (0.027) 609	-0.012 (0.015) 609
Poor	-0.029 (0.074) 569	0.006 (0.037) 569	-0.017 (0.018) 569
Rich	*0.061 (0.033) 505	*0.061 (0.034) 505	*-0.031 (0.017) 505
Villages with both poor and rich households:			
All	0.017 (0.036) 465	0.047 (0.039) 465	-0.025 (0.017) 465
Poor	-0.063 (0.083) 465	0.006 (0.043) 465	0.010 (0.020) 465
Rich	**0.092 (0.038) 465	***0.092 (0.035) 465	*-0.031 (0.018) 465

Table 6
Impact of village investment program on village governance in 2004
(village matching estimates)

Governance outcome	All China
Education of leaders	***0.622 (0.105)
Quality of village committee	0.043 (0.096)
N	604

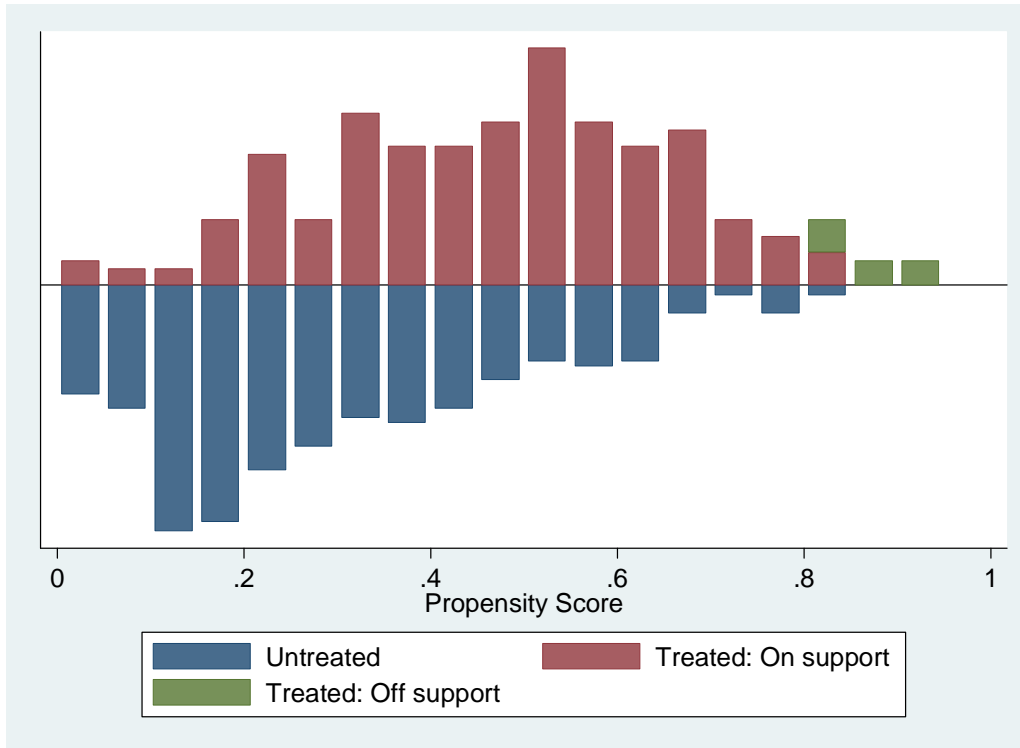
Note: ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels.

Table 7
Governance and Program Impact on the Rich and Poor within Villages
(Village Matching Estimates)

		Education of village leaders			Quality of village committee		
		Low	High	Diff.	Low	High	Diff.
mean							
$\Delta \ln(\text{inc. pc})$	Poor	**0.119 (0.055)	0.077 (0.070)	-0.042	-0.137 (0.087)	***0.319 (0.115)	***0.456
	Rich	** -0.156 (0.071)	-0.014 (0.054)	0.142	0.030 (0.059)	***0.355 (0.064)	***0.325
	Diff.	***-0.275	-0.091	0.184	0.167	0.036	-0.131
Mean							
$\Delta \ln(\text{con. pc})$	Poor	0.066 (0.081)	***-0.134 (0.041)	** -0.200	** -0.131 (0.057)	**0.131 (0.062)	***0.262
	Rich	-0.090 (0.080)	-0.037 (0.041)	0.053	0.025 (0.048)	***0.179 (0.056)	**0.154
	Diff.	-0.156	*0.097	**0.253	**0.156	0.048	-0.108

Notes: Coefficients are program impacts. ***, **, and * signify statistical significance at the 1, 5, and 10 percent levels. Education of village leaders is principal component that gives high weights to education of party secretary, education of village head, and share of village committee members with middle school education or above. Quality of village committee is principal component that gives high weights to the number of village committee members and the frequency of village committee meetings. Villages are divided into even-sized groups of low and high. Relative poor and rich households are those below and above the 2001 median income per capita. The sample is restricted to villages with both poor and rich households, and ranges in size from 203 to 230 villages.

Appendix Figure 1
Distribution of propensity scores (logit) for treatment and control poor villages



Appendix Table 1
Balancing test for village matching, based on propensity score matching

Variable	Sample	Mean		%bias	%reduct		t-test	
		Treated	Control		bias	t	p> t	
Mean yrs of schooling	Unmatched	7.487	7.2445	12.5			1.48	0.140
	Matched	7.4728	7.5331	-3.1	75.1		-0.33	0.742
Ln('01 tot. inv. p.c.)	Unmatched	2.0245	1.5974	20.2			2.44	0.015
	Matched	1.9674	1.9548	0.6	97		0.06	0.951
Ln('01 gov. inv. p.c.)	Unmatched	1.1185	0.85503	14.8			1.8	0.072
	Matched	1.0917	1.021	4	73.2		0.41	0.682
Poor county	Unmatched	0.88987	0.90306	-4.3			-0.52	0.602
	Matched	0.8894	0.86083	9.4	-116.6		0.9	0.370
Ln(county pop.)	Unmatched	12.885	12.838	7.2			0.85	0.394
	Matched	12.88	12.872	1.2	83.5		0.13	0.899
Ln(county ag prod.)	Unmatched	5.6607	5.576	45.1			5.37	0.000
	Matched	5.656	5.6668	-5.8	87.2		-0.61	0.541
'01 share w/telephone	Unmatched	53.765	39.744	33.7			4.02	0.000
	Matched	53.043	50.056	7.2	78.7		0.74	0.463
'01 share w/water	Unmatched	62.571	60.098	5.8			0.7	0.487
	Matched	62.413	63.013	-1.4	75.7		-0.15	0.882
'01 share w/road	Unmatched	75.034	75.557	-1.5			-0.18	0.856
	Matched	75.969	78.208	-6.5	-328.8		-0.69	0.488
'01 share w/pave rd.	Unmatched	9.8626	10.694	-3.1			-0.37	0.712
	Matched	10.317	11.137	-3.1	1.4		-0.31	0.754
Distance to market	Unmatched	6.4084	8.4148	-23.8			-2.74	0.006
	Matched	6.4687	6.2305	2.8	88.1		0.35	0.728
Mountainous	Unmatched	0.5022	0.625	-24.9			-3	0.003
	Matched	0.49309	0.46175	6.4	74.5		0.65	0.515
Rev. base area	Unmatched	0.22026	0.19898	5.2			0.63	0.529
	Matched	0.21198	0.21843	-1.6	69.7		-0.16	0.871
Distance to town	Unmatched	7.6282	8.17	-7.5			-0.9	0.368
	Matched	7.7744	7.5626	3	60.9		0.32	0.752
Distance to county	Unmatched	23.775	23.972	-2.1			-0.26	0.796
	Matched	23.557	24.167	-6.6	-208.5		-0.69	0.492
Ln('01 inc. p.c.)	Unmatched	7.0863	7.0027	15.5			1.83	0.067
	Matched	7.0726	7.1214	-9.1	41.6		-0.99	0.322
'01 poverty rate	Unmatched	0.16251	0.18986	-11.3			-1.36	0.174
	Matched	0.16845	0.14755	8.7	23.6		0.92	0.355

Note: Balancing test results reported here are calculated using the psmatch2 command in STATA.