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Farmland Reforestation in China

by

Peter Alfred Kelly

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University of California, Berkeley

Farmland Reforestation in China

by Peter Alfred Kelly

To the memory of my mother

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Acknowledgements

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Weights and measures

15 *mu*=1 hectare

8.3 *yuan*=$1 as of the survey in June 2007, 6.8 *yuan*=$1 as of February 2010

2 *jin*=1 kilogram

9 *jin/mu* ~= 1 bushel/acre

Abstract

Farmland Reforestation in China

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Peter Alfred Kelly

Doctor of Philosophy in Agricultural and Resource Economics

University of California, Berkeley

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As the world's largest payments for environmental services program, China's Sloping Land Conversion Program has reforested vast areas of environmentally sensitive farmland since 1999 and provided subsidy payments to millions of poor farmers in mountainous western China. This dissertation analyzes the socio-economic effects of the program in Shaanxi Province.

Chapter 1 examines the effects of enrollment in the Sloping Land Conversion Program on household labor market outcomes. It uses the exact timing of enrollment to identify a causal link between enrollment and non-farm employment, and finds that enrollment has a small but statistically significant positive effect on non-farm employment at the household level. The probability of an adult without non-farm employment beginning such employment in a particular year increases from 1.4% in years without new household enrollment to 2.1% in years with enrollment. This amounts to an increase of national labor supply of at least 600,000 individuals. The analysis measures enrollment in different ways to distinguish among competing channels of the effects of enrollment on employment, and finds that employment effects arise not from alleviating constraints, as other researchers have suggested, but rather from simple farm to non-farm labor substitution.

Chapter 2 focuses on problems in the implementation of the program, including farmers not receiving subsidy payments to which they are entitled, and the over-reporting of areas eligible for subsidies on the part of local governments. On average, villages reported 72% more area enrolled in the program than was actually the case, and 15% of enrolled farmers received at least a portion of the subsidies to which they were entitled late or not at all. The chapter finds that both misaligned incentives and low managerial ability contribute to inefficient outcomes. Villages that are poor and remote (and are assumed to be less able to fund administrative costs without over-reporting and less likely to be audited) over-reported more, while farmers were less likely to receive subsidy payments to which they were entitled if the village leader had lower managerial ability and a larger

village to manage. In the Sloping Land Conversion Program, finely tuned targeting that might be optimal in a smaller program is impractical due to administrative costs.

Chapter 3 compares determinants of enrollment at the parcel and household levels for parcels where farmers made the decision of whether to enroll to parcels where local governments made the decision. It finds no evidence that farmers place more weight on productivity relative to ecological factors, but rather that decisions made by local governments are more easily predicted by plot characteristics such as slope and soil quality, and to some extent by a desire to create contiguous forests, whereas farmers place more weight on land characteristics relative to land on the same farm and on education and other household characteristics. The most important difference between farmer and local government decision-making is the frame of reference, the scale within the landscape to which land under consideration for enrollment is compared, not the relative weights placed on different criteria of suitability for enrollment.

Chapter 1. Effects of China's Sloping Land Conversion Program on Non-farm Labor Market Participation: Separating Substitution and Income Effects using the Timing of Participation

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Abstract: This paper examines whether, and through what mechanisms, China's Sloping Land Conversion Program (SLCP) promotes non-farm employment among its participants. The SLCP, which bears similarities to the Conservation Reserve Program in the United States, pays farmers to plant trees on highly erodible cropland, and has effected major land use changes in western China over the past decade. With 15 million households, it represents by far the largest payments for environmental services program worldwide. Under the program, farmers retire their land indefinitely but only receive subsidies for 5-8 years (not including finite extensions). Whether participants successfully move into non-farm employment remains uncertain. This paper uses a household dataset collected by the author's collaborators in Shaanxi Province, which contains year-by-year enrollment information on 3397 pieces of land and employment information on more than 3165 individuals over the period 1998-2006. The analysis uses variation in the exact timing of enrollment, and measures enrollment in several ways to distinguish among competing channels of the effects of enrollment on employment. An instrumental variable derived from the results of land lotteries during de-collectivization removes the possibility of endogenous farmer choice biasing the results. Enrollment has a small but significant and robust positive effect on non-farm employment, increasing the probability of an adult beginning non-farm employment from 1.4% to 2.1% in years in which the household enrolls new land. This amounts to an increase in national labor supply of at least 600,000 individuals. The effect arises not from alleviating constraints, as other researchers have suggested, but rather from simple farm to non-farm labor substitution.

1. Introduction

Payments for environmental services programs

Many of the world's poor live in mountainous and other ecologically fragile regions. Whether poverty stems from geographic conditions, contributes to ecological degradation, or merely happens to exist in many fragile environments, the attraction of a program that promises to both reduce poverty rates and improve the environment is obvious. In recent years, a number of developing countries, including Costa Rica, Mexico, and China, have implemented payments for environmental services (PES) programs (see for example, Hyde et al. 2003; Mayrand and Paquin 2004; and Xu, Z et al. 2005; Alix-Garcia et al. 2005). Such programs aim to achieve the dual goals of poverty reduction and ecological restoration by paying farmers to adopt sustainable practices, often by planting trees to reduce soil erosion.

According to the Coase Theorem (Coase 1960), efficiency is not affected by whether a farmer or society holds the right to choose agricultural practices, assuming no transaction costs in negotiating the payments necessary to bring about a social optimum. However, the transaction costs of society suing individual farmers over practices that generate negative externalities are prohibitively high, especially in developing countries with many small farms. Thus, a PES scheme, in which farmers hold the right to generate negative externalities, but in which these rights can be purchased by taxpayers or private groups, may well be socially efficient, as well as desirable from the farmers' perspective (Zilberman et al. 2008).

Nevertheless, an attempt to achieve multiple objectives with one policy instrument can prove problematic, as first suggested by Tinbergen (1956). A policy targeted at protecting the environment is likely not the most cost-effective way to reduce poverty rates. Multiple, carefully targeted policies may most efficiently achieve dual policy objectives.

A number of PES programs have been established around the world, most sponsored by national governments, but a significant number also by NGOs such as the Nature Conservancy, by international organizations such as the Global Environment Facility, and even by private companies. Among the best known such programs are the Conservation Reserve Program in the United States, designed primarily to set aside highly erodible land, and smaller schemes are in Mexico and Costa Rica, where farmers are paid to plant trees to protect watersheds[1].

The long-term success of a PES program depends on whether its participants can find alternative livelihoods to growing crops. Unless subsidies are extended indefinitely or farmers find alternative livelihoods, farmers will either become impoverished or resume cultivation and reverse the ecological benefits of the PES program.

The Sloping Land Conversion Program[2]

[1] In Mexico, farmers signed only 269 PES contracts in 2003, for a total area of 127,000 ha reforested and payments of $3.7 million (Alix-Garcia et al. 2005). In Costa Rica, a total of only 21,838 ha were reforested under PES contracts from 1997-2002 (Redondo-Brenes 2005).

[2] This section is based on interviews with officials in China's State Forestry Administration (including Jincheng Zhao, Chen Xie, and many others), local government officials, farmers, and researchers (special

China's Sloping Land Conversion Program (SLCP), formerly known as Grain for Green, is by far the largest PES program in any developing country. Although official figures probably overstate the area enrolled, the SLCP is approaching its target of 15 million hectares by 2010, roughly the area enrolled in the Conservation Reserve Program. At more than $2 billion per year, the SLCP's budget exceeds that of the Conservation Reserve Program (even without adjusting for differences in purchasing power parity), and is roughly equal to the total government budget of Costa Rica. More than 15 million households are participating, more than the total number of farm households in the United States. Even in a country with the population of China, SLCP is the third most widespread rural investment project, behind roads and irrigation systems (Zhang et al. 2006).

The SLCP, begun in 1999, is the most recent of a series of Chinese government programs to replant marginal cropland and barren hillsides, but the first that resembles a modern PES program[3]. Most of the enrolled area is in western China, the poorest area of the country and the one facing the most serious erosion hazards[4]. As is common in other PES programs, the SLCP has more than one objective. It aims to reduce erosion and restore ecological balance, to support farmers' incomes, and in the longer term after the subsidies expire, to move farmers into other employment endeavors, such as growing high-value crops or taking on non-farm employment. At this time, carbon sequestration is not an official goal of the program.

The SLCP is, in principle, a voluntary program similar to the Conservation Reserve Program. However, in China there is no private ownership of farmland, and executive departments have substantial leeway in implementing laws, meaning that participation is in practice mandatory for many farmers. Although farmers have limited autonomy in determining whether to participate in SLCP, most participants in the sample say that they are better off as a result of participating. Some farmers say they are worse off, but others say they would like to enroll even larger areas than they have.

Farmers in villages eligible for the program attend required village meetings in which village officials explain the program and how it is implemented in their area. At the meetings, the farmers are told which pieces of land must be enrolled, which may not be enrolled, and which they can choose whether or not to enroll. The path of least

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thanks to Jintao Xu of the Peking University Department of Environmental Sciences), as well as sample contracts and administrative records from various township government offices. A general introduction to the SLCP and associated regulations can be found on the State Forestry Administration's website at http://www.tghl.gov.cn, and an introduction written from the perspective of an outside economist in Xu et al. (2005).

[3] Since 1949, the State Forestry Administration (SFA) has sponsored a number of programs to reforest steep land, using a combination of forestry-administration staff and villagers mobilized in campaign-style efforts. Total reported reforestation has actually exceeded the total area of China, because marginal land has been repeatedly planted with trees, either after the trees fail to survive or after the land was temporarily returned to grain production. The SLCP, begun in 1999, is the first program in China to resemble a modern PES program.

[4] The loess plateau of the Yellow River basin in northwest China has the highest erosion rate in the world, with deep gullies a prominent feature of the landscape in large sections of several provinces. In the western reaches of the Yangtze River, upstream of the Three Gorges Dam reservoir, farmers traditionally grow maize on mountainsides much too steep to cultivate with machinery. Serious wind erosion and desertification plague much of China's northwest.

resistance for the farmer is to follow the local government's plan to enroll certain areas and not others. Those farmers who enroll sign a contract with the SFA or another designated local government unit, and agree to plant trees on land that has been rented from or allocated by the village. Appendix 1 contains an English translation of one version of the contract, from northern China. In southern China, subsidy payment rates are exactly 50% higher per hectare, but there are no other substantive differences in the contract. The details of the implementation vary with the type of trees to be planted, and the program has gone from an in-kind grain subsidy to a cash subsidy[5]. The contract states that land is to remain enrolled indefinitely even though subsidies are for only 5-8 years (not including finite extensions)[6]. In the sampled villages at the time of the survey, there existed no procedure for un-enrolling a plot once it had been enrolled. The program was designed with the hope that farmers would voluntarily substitute non-farm employment and/or high-value crops as income sources to replace their lost pre-enrollment grain production income.

Reforestation and employment

As with other PES programs, non-farm employment is important to the long-term sustainability of SLCP. Nationwide, non-farm employment has played a major role in reducing rural poverty in China in recent decades (deBrauw 2002; Bowlus and Sicular 2003). Within the sample, which is probably representative of the remote areas where SLCP is most important, non-farm employment of farmers as increased substantially over the past ten years of program implementation. The changes have been broad-based, including both local and outside work locations, part-time and full-time work, men and women, and industry and service jobs. What role, if any, the reforestation program has played in these trends has not been well understood. Empirical evidence is mixed; Ahearn et al. (2006), for example, find that the Conservation Reserve Program in the U.S. tends to keep farmers on the farm, while Groom et al. (2007) and Uchida et al. (2009) find that SLCP enrollment tends to promote non-farm employment.

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[5] Upon enrollment, in the appropriate season, the SFA provides ecological-tree seedlings free of charge, or shares the cost of orchard seedlings with the farmer. In either case, the village trains farmers in planting the seedlings. In most villages, planting the trees is a required community undertaking, whereas taking care of the trees is the responsibility of the individual farmer. If the plots pass a series of inspections, the farmer receives an annual subsidy payment to compensate for the opportunity costs of retiring his grain-producing land. In the early years of the program, this payment was in kind in the form of grain; it later changed to a cash payment, but because grain markets are well developed farmers did not consider this a substantive change. Almost all plots enrolled eventually pass inspection; when tree survival rates are low, farmers are generally given new seedlings and their plots are declared passing as long as they make a good-faith effort to reforest the area by planting the new seedlings.

[6] Payments last for 5-8 years, after which the farmer must either continue to keep the land enrolled for the remainder of his land-use contract (which is extended by the reforestation contract to 50 years), or pay an unspecified fine. (No farmer in the sample knew how much the fine would be for violating the land retirement contract, and only one had actually cut his trees and paid such a fine. The vast majority of farmers saw the contract as binding.) Although subsidies have been extended, both unofficially through local procedures to spread payments over time and now officially nationwide, they are still in principle for a finite period of time.

Hypotheses and approach

The objective of this paper is to determine whether, and through which mechanisms, SLCP may cause increases in non-farm employment. Other researchers (Uchida et al. 2009; Groom et al. 2007) have tackled these questions using difference-in-difference methods comparing participants to non-participants, and have found small and not especially robust effects. Both papers argue that because poor households are more affected by participation than rich households (a result that is found in only some specifications in this paper), SLCP is alleviating constraints. These papers have several weaknesses. First, participants differ from non-participants on a number of variables. Matching techniques may be unable to control for differences in unobserved variables, and cannot adequately address the problem of endogenous choice of participation (to the extent that such choices are voluntary). In addition, other researchers only use data from 2-3 points in time, making it impossible to determine exactly when employment changes occurred relative to enrollment.

This paper, in contrast, uses annual data from 1998-2007 on both employment and enrollment, allowing the construction of a conditional probability model in which the probability of an individual obtaining employment in a particular year is a function of the household's enrollment in that same year. (The results suggest the entire impact occurs within one year.) Such a specification improves the statistical power of the estimation, allowing for a much greater variety of robustness checks, including an instrumental variable for enrollment and placebo tests to check for reverse causation or biased standard errors. The IV estimation eliminates the possibility of endogenous farmer choice biasing the results by using an interaction of the timing of village enrollment quotas and household land characteristics to predict household enrollment in particular years. The paper also uses a larger dataset, collected with more attention to recall bias and other measurement issues, than in past research.

Like other literature, the paper finds that SLCP enrollment has a small but statistically significant positive effect on non-farm employment. The effect increases as the labor savings from not growing crops increase, but appears unrelated to whether farmers are made better or worse off by SLCP. The results support a simple story of labor reallocation similar to that told by farmers, and do not support any of several stories in the literature related to alleviating constraints or to income effects. The paper also addresses the question of which demographic groups are most affected by SLCP. Although some groups are much more likely to enter the labor force than others, the paper has limited statistical power to determine which groups are most affected by SLCP.

2. Theoretical framework

Reforestation is one of many factors that may influence decisions to enter the non-farm labor market. The New Economics of Labor Migration (Stark and Bloom 1985; Mora and Taylor 2005) emphasizes the role of household and community variables in labor migration in developing countries. Because reforestation is probably not the main factor in migration decisions, this paper uses household fixed effects to capture the effects of household and community variables, both observed and unobserved. The data are from a sample of only 700 households, so there is generally insufficient statistical power to identify the effects of interactions between reforestation and household and community variables. Therefore, like other literature on the SLCP, this paper does not incorporate a comprehensive model of labor markets or focus on such interaction variables. Economic literature on PES has proposed five possible mechanisms for how a land retirement program might affect non-farm employment. Of the five hypotheses, this paper focuses on the first three, arguing that the fourth is not relevant to the context studied and briefly addressing the fifth in the validity checks section[7].

Each of the hypotheses is based on the assumption that enrollment is mandatory or exogenous from the perspective of the farmer, an assumption relaxed in the empirical section with an instrumental variable. As an additional caveat, tests of the following hypotheses are intended to better understand the effects of the reforestation program, not to measure welfare changes. Because the effects on employment are induced choices, it does not make sense to think of them as positive or negative welfare changes.

1. The Labor Substitution Effect. Under the labor substitution effect hypothesis, as described in, for example, Uchida et al. (2009), retiring land saves on farm labor, and some farmers use the time they save to work off-farm, while others use it to increase their leisure[8]. More formally, in a household with separable production and consumption decisions facing no constraints except on time and land,

$$U=f\,(T_L,\ I_F(T_F,L) +\ I_N(T_N))$$

$$\text{s.t. } T_L+T_F+T_N=T$$

where T, I, and L denote time, income and land devoted to crop production, respectively; subscripts subscripts L, F, and N denote leisure, farm work, and non-farm work respectively. Under the additional assumption that land and labor are complements in production, or

$$d^2I_F\,/\,dT_F dL > 0$$

[7] An additional approach often used in labor economics, a duration model of unemployment, is not appropriate in this context. As discussed later, only 4% of adults in the sample cycled in and out of employment—the rest were either always-in, always-out, joined once, or left once. The duration of unemployment prior to SLCP is unknown, but likely a simple function of age for those who have never been employed. Data are censored both before SLCP and in 2007 8 years into the program, and addressing the censoring issue would require imposing functional form assumptions. In addition, jobs obtained through passive means (discussed later in the paper) are not of interest in the analysis and could not be clearly separated in a duration model.

[8] This paper follows Uchida et al. (2009) in referring to this as a labor substitution effect. Since it is not a substitution effect in the sense of substituting different inputs in the production of a single output, it might more precisely be referred to as a labor reallocation effect.

the reduction in L associated with reforestation will increase T_L and T_N as farmers shift their time from crop production to non-farm work and leisure. Put another way, a reduction in cropland from reforestation reduces the marginal productivity of farm labor, and thereby the relative marginal productivity of off-farm labor.

2. The Income Effect. Under the income effect hypothesis, the direction of the effect of reforestation on non-farm employment depends on the direction of the effect of reforestation on income. When reforestation is mandatory, as often is the case in China, reforestation could be associated with lower as well as higher income. Assume that labor requirements associated with crop production are negligible, but that production and consumption decisions are interconnected. Then

$$U = f(T_L, I(T_N+E))$$

$$\text{s.t. } T_L+T_N=T$$

where E denotes income change from reforestation program participation. Under the assumption of declining marginal utility of income, or

$$d^2f / dI^2 < 0$$

as well as positive wages, and positive utility of leisure,

$$sign(dT_N/dE) = - sign(dI/dE).$$

Under the income effect, where enrollment increases income ($dI/dE>0$), it will decrease non-farm working time, while where it decreases income it will increase non-farm work time.

3. The Liquidity Effect. Under the liquidity effect, formulated by Uchida et al. (2009), the direction of the effect of reforestation on employment also depends on the income change from participation. However, under the liquidity effect, gaining more income through the program enhances the probability of beginning off-farm employment, which is the opposite prediction as the income effect. A farmer who is credit-constrained is unable to make an investment in a job search, i.e., invest time, pay for transportation, etc. in order to gain a higher expected permanent income. Disregarding labor associated with on-farm activities before or after reforestation,

$$U = f\,(T_L,\; I(T_{Nt}))$$

$$s.t.\; T_{Nt} <= max\,(\,T_{Nt-1},\, T_{Nt-1}+g(E)\,)\;|\; R < R_{bar}$$

where t subscripts indicate time, g() is a monotonically increasing function, and R represents household resources available for job searching (savings and potential loans). For households that are credit constrained ($R < R_{bar}$), off-farm employment can expand only when the income change from participation is positive.

4. Relaxation of output constraints. In Groom et al. (2007), one of the mechanisms by which SLCP affects non-farm employment is by relaxing output constraints. In their paper, farmers continue growing crops even when non-farm opportunities are more lucrative because of poorly developed markets for grain, including taxes that must be paid in grain, and because land that a household does not use is subject to being administratively re-allocated to others in the village.

This paper does not consider output constraints because of qualitative evidence that strongly indicates that the underlying assumptions do not apply to the time and place the data were collected. Farmers and village leaders were virtually unanimous in reporting that grain markets were well developed, and that taxes in grain and administrative re-allocations of land had been abolished before the relevant time period. They said that family responsibilities and difficulty finding non-farm jobs (resulting in part from low levels of education and China's household registration system that restricts most urban jobs to city-dwellers) were the primary factors keeping working-age individuals on the farm[9].

5. Spillover Effects. Under the spillover effect hypothesis, one's employment might be affected by one's neighbor's reforestation through social networks. This hypothesis is tested in the validity checks section of the paper.

A related hypothesis, the idea that land retirement leads to rural population losses that reduce local employment opportunities, especially in businesses that serve farmers, is

[9] Respondents also reported that, while land rentals are often permitted, villagers are not interested in renting-in the type of marginal land associated with SLCP. Because of the difference in land quality between land likely to be rented and that enrolled in SLCP, as well as the fact that official constraints to non-farm employment are mainly on the urban receiving end, the results in the paper cannot necessarily be used to predict the effect of a liberalization of land rental markets on non-farm employment in China.

not discussed in this paper. This mechanism has been studied in the context of the Conservation Reserve Program in the United States (Economic Research Service 2004), where farmers have significant purchasing power that helps to support rural economies. In China, farmers have limited purchasing power, and out-migration may actually stimulate the rural economy to the extent that a migrant's remittances exceed his previous on-farm productivity. However, it is unlikely that remittances are large enough to create more than a small fraction of a job in rural areas for every migrant, and such second-order effects on employment are beyond the scope of this paper.

3. Survey design

Sampling scheme

To test the above hypotheses, a household survey of 682 households in Shaanxi Province was carried out by a team of enumerators in June 2007. Shaanxi Province was chosen because it has the largest reforested area of any province nationwide, because its reforestation subsidies are likely to be important relative to the below-average incomes in the province, and because it contains different types of counties that differ in subsidy rates and program timing. (No other survey of reforestation in China has been designed in such a way as to take advantage of either source of variation.)

After a small pilot area was reforested in the first year (1999), only certain counties received a new reforestation quota in 2000 and 2001. The counties selected to receive new reforestation quotas in 2000-2001 were those with the steepest land of the prefecture, the administrative division between county and province. Starting in 2002, and continuing until new enrollment was scaled back in the middle of the decade, all counties received reforestation quotas annually. The sample was stratified at the county level to contain three counties that received quotas in 2000-01 and three that did not.

Shaanxi Province also contains two different per-hectare subsidy payment rates, and the sample is stratified with three counties paying each rate. Nationwide, there are only two payment schemes, 140 *yuan* per *mu* in the Yellow River basin and 210 *yuan* per *mu* in the Yangtze River basin, with the difference intended to compensate for the higher grain yields, and therefore higher opportunity costs of participating, in southern China, but within the sample all counties are near the boundary in payment rates.

The probability of selecting a specific county was proportional to the amount of area reforested in that county. Townships and villages were selected in the same manner where data were available, and randomly where data were not available. Only villages that were reasonably accessible were selected[10]. Within each village, households were chosen at regular intervals off of a village roster, and households were eligible to participate in the survey whether or not they had participated in the reforestation

[10] This accessibility criterion excluded one township in Hua County that was across a mountain pass from the county seat, and two townships in lightly populated sections of Ziyang and Xunyang counties that could not be reached safely as a result of landslide hazards. The survey team replaced the inaccessible Hua County township with a less distant township in the same county that was probably equally isolated; reaching the replacement township required obtaining permission to pass through a military base. In an attempt to compensate for the omissions in Ziyang and Xunyang counties, more remote sections of less mountainous counties were slightly over-sampled.

program. Because the survey was conducted during harvest season, the response rate was high, and reasons for nonparticipation were documented at the household level. The final sample included 6 counties, 22 townships, 44 villages, and 682 households; this is a large sample size given the transportation conditions in the mountainous areas where reforestation is mainly taking place, and relative to other similar surveys.[11] For more information about the design of the questionnaire and implementation of the survey, see Appendix 2.

4. Descriptive statistics

Household characteristics

Approximately 86% of households in the sample participated in SLCP. This is representative of the study region—in many villages, there were few or no households that did not participate. Concurrent to program enrollment, between 1998 and 2006, 57% of households saw at least one member begin a non-farm occupation (Table 1). Those 57% were actually more dependent on crop-related income than households in which no one began a non-farm occupation, where potential members of the labor force tended to already be employed in 1998.

Crop income had been declining in importance since before SLCP began, but remained primary for most households, before the peak year of enrollment in 2003. As might be expected, participants reported lower income from growing crops after enrollment. Among all sub-groups, few had any fallow land a decade ago, and most of this land was in rotation with crops; little of the land enrolled in SLCP was not in use before the program.

The significant differences between households who participated in SLCP and those who never participated illustrates one of the weaknesses in other literature on SLCP and employment (e.g., Uchida et al. (2009); Groom et al. (2007)), the difficulty in finding comparable households, and highlights the importance of using the amount and exact timing of participation as a source of identification.

Individual characteristics

The sample contains more than 3165 individuals, of whom 1156 were employed outside of the family farm for at least part of the time between 1998 and 2006 (Table 2). Employment patterns are broadly consistent with patterns in rural China more generally. Men and more educated individuals are most likely to be employed, often in the construction sector, and large numbers of young adults leave the farm for work. Most of

[11] The State Forestry Administration's annual household survey covers 400 households. Two surveys conducted by Professors Jintao Xu and Shiqiu Zhang at Peking University, on which all existing household-level analysis of the program in English-language journals have been based, surveyed only approximately 350 households each. The only larger-sample household survey that has been conducted on the program (NWSUAF's Wuqi survey) employed a very short questionnaire and was conducted in a single model forestry county, limiting the usefulness of its results.

those who are not working are not working mainly because of their age, family responsibilities, health, or limited skills and education, not because their labor is needed on the farm.

SLCP participants and non-participants do not differ greatly on demographic or employment characteristics, and the differences that are statistically significant do not show a consistent pattern. For example, those in non-participating households work fewer hours and remit less, but are more likely to be working outside of the county in a factory or service institution, as opposed to within the county in construction or agriculture-related occupations.

The fraction of individuals in non-participating households in the sample who began working during the period 1998-2006 is almost identical to that of individuals in participating households (16.1 versus 16.4%). However, given random sampling error from the small number of non-participating households, the 95% confidence interval for the population proportion for individuals beginning work in non-participating households ranges from under 13 to nearly 20%. A difference of 3 percentage points from 16% would represent an economically significant 1.5 million individuals for all of China. Thus, the figures in Table 2 are inconclusive, and consistent with a zero, a meaningfully positive, or even a meaningfully negative impact of household SLCP participation on non-farm employment over the entire 10-year period. The regression section of this paper will look much more closely at the exact timing and nature of SLCP enrollment to identify employment impacts.

Table 2 shows that very few jobs are in the agriculture and forestry sectors. There is almost no market for farm labor in rural China, and only 22 individuals in the sample had worked as an agricultural laborer at any time in the past decade. Of these 22 jobs, all but 6 started before 1998. Another two worked in the forestry sector, both in careers that began before 1998. Thus any direct effect of SLCP on employment in these sectors is likely to be negligible. Most new jobs were outside of the home county in the secondary and tertiary sectors.

Table 2 shows a large increase in non-farm employment (most jobs began in the past 10 years), but only a modest increase in the number of days worked for those employed. This paper focuses on people obtaining jobs, not on the time that they work or the amount that they earn. This is not only because beginning new jobs has been the most important trend, but also because specific hours worked may be subject to greater recall bias, and it would also likely require a different model of employers and employees negotiating interior solutions.

Land characteristics

Households in the sample cultivate a mean of nearly 5 small pieces of land, approximately 39% of which are enrolled in SLCP (Table 3). Nearly 85% of the households in the sample have both enrolled and non-enrolled land, which reflects both the intent of SLCP and the way in which land was distributed to households during the 1980’s de-collectivization. During the village land lotteries in the 1980’s, households were allocated a combination of good and bad land, meaning that there is negative correlation of land characteristics within households. Land characteristics are comparable between households in which at least one member started a non-farm job in the past decade, and those in which no one did so.

Enrolled plots tend to be steep, far away from the farmer's house, have poor soil, and not to be irrigated. (In fact, most are much too steep to irrigate using conventional techniques.) Species of trees and bushes planted on enrolled land vary greatly from one village to another, and many plots are planted with several types of trees. The most commonly planted species are prickly ash, locust/acacia tree, and walnut, which together account for approximately half of the trees planted under SLCP in the sample.

Intercropping of trees and grain is allowed for at least one year on nearly ¼ of the enrolled plots, but few enrolled plots are reported to be actually intercropped. In most cases, the year of enrollment represents the end of row crop production on the plot, or at least a dramatic reduction in the planting density[12].

Among non-enrolled plots, over 80% are currently planted to grain, most commonly to maize in the summer followed by wheat or rapeseed in the winter. The remainder are planted to orchards or forest trees outside of the context of the SLCP, or to other crops such as potatoes and vegetables.

Farmers reported autonomy in whether to enroll for fewer than half of the plots in the sample. The percentage of non-enrolled plots that could have been enrolled had the farmer chosen to do so (46%) was slightly higher than the percentage of enrolled plots that could have been not enrolled had the farmer not wished to enroll them (43%). Under national policy, farmers have autonomy regarding whether to enroll most plots, with certain exceptions such as especially steep land and basic grain production land, which generally must and may not be enrolled, respectively. However, this policy is routinely ignored at the local level. In practice, most farmers in the sample followed the plan set by the local government even when they had the authority to deviate from it.

Graphical and tabular results

Table 4 presents a simple tabulation of employment changes and SLCP enrollment. Out of 28,485 individual-year observations in the sample, 15,961 represent individuals over age 15 who were not employed in the previous year. Of these observations, 613 represent years in which an individual began non-farm employment[13], and 1832 represent years in which an individual's household enrolled at least one new piece of land in SLCP. There were 85 events in which the beginning of non-farm employment coincided with new enrollment, compared to an expected value of 70 events under the null hypothesis that new enrollment and new non-farm employment are independent. This difference of 15 events, out of 573 participating households in the dataset, corresponds to

[12] The survey team interviewed farmers in their homes and did not systematically visit enrolled land. Anecdotal observations, however, suggest that more than 3% of enrolled plots are intercropped in the sense that grains are scattered among the trees or bushes planted under the program. Farmers tended to report land as intercropped only when the amount of inter-planted grain was substantial.

[13] Beginning non-farm employment here refers to the transition from not having a non-farm job to having one. It does not include changes from one non-farm job to another, but does include a small number of returns to work after gaps in employment. In the dataset, 519 individuals began new non-farm jobs and continued to work until the present, while only 116 had other employment patterns involving entering and exiting the workforce.

approximately 400,000 new non-farm jobs out of 15 million participating households that have participated in SLCP nationwide. This is similar to the magnitude of the effect estimated later in this paper using more sophisticated regression-based techniques.

Figure 1 graphically illustrates the correlation between the amount of reforestation and the probability of an individual beginning non-farm employment. Paralleling Table 4, the unit of observation is the individual-year. The X axis represents new enrollment at the household level in a particular year, and the Y axis represents the probability of individuals over age 15 who are not currently employed obtaining a job in a particular year.

In Figure 1, the line labeled lowess best fit is derived from observations in which new enrollment is strictly greater than zero (the majority in the full dataset are equal to zero). In contrast, the line labeled linear fit is simply a straight line drawn between the probability of an individual beginning a non-farm job conditioned on the household not enrolling any new land in SLCP (approximately 1.1% annually) and the probability of an individual beginning a non-farm job conditioned on the household enrolling all of its land in SLCP in the year in question (approximately 2.6%). The probability of a young adult joining the non-farm labor force is much greater than 1% per year, but the effect of SLCP is similar across age groups.

The correlation shown in the figure is consistent with a linear increasing relationship between SLCP enrollment and new non-farm employment, with perhaps a positive effect on employment of simply enrolling in SLCP, even with only a small amount of land. This is similar to the findings from the regression results presented later in this paper.

Figure 2 presents farmer opinions on the potential causal links between SLCP enrollment and non-farm employment. The results are based on the following open-ended question, asked of the head of household: “Do you believe that SLCP may have influenced off-farm employment in your family? If so, what kind of effect might it have had?” Responses were coded into four categories:

--“No effect” means that the respondent said that SLCP was irrelevant to non-farm employment in the household, said that there was no effect, or had no opinion.

--“Positive labor substitution” refers to the idea that SLCP might increase non-farm employment by reducing on-farm labor requirements, essentially citing the labor substitution effect described in the theoretical section.

--“Negative labor substitution” indicates that the respondent believed that SLCP increases on-farm labor requirements and might thereby reduce non-farm employment. In some villages, SLCP was accompanied by technical assistance on growing orchard trees, which can have greater labor requirements than they grains they displaced.

--“Alternative livelihood” refers to the idea that, either in the short term or after subsidies expire, SLCP reduces household income, which promotes non-farm employment by increasing the marginal utility associated with non-farm income relative to the marginal utility of leisure.

Nearly 98% of respondents either thought that SLCP had no effect on non-farm employment in their household, or cited the labor substitution effect as a means by which it might have increased non-farm employment. Not a single farmer’s response was consistent with the income, liquidity constraint, or output constraint effects proposed by economists, though some did mention in other parts of the survey that they had found jobs through friends and relatives, which would be consistent with spillover effects.

5. Identification

Estimating equations

Most of the results in this paper are based on estimating $\underline{\mathbf{b}}$ in the following equation:

$$P(N_{it}=1 \mid N_{it-1}=0) = [(E_{it}-E_{it-1}), \underline{\mathbf{Y}}_{t}, \mathbf{H}_{\underline{i}}, \underline{\mathbf{X}}_{i}, \underline{\mathbf{A}}_{it}]*\underline{\mathbf{b}}+e$$

where N=non-farm employment
E represents SLCP enrollment (units vary by specification)
Y represents a vector of dummy variables for year
H represents a vector of dummy variables for household
X represents time-invariant individual characteristics
A represents age and age squared
i and t represent individual and time (year) respectively

In the empirical estimation, the probability of beginning non-farm employment[14] for a particular individual in a particular year is a function of whether the household enrolls any new land in SLCP in that year (or how much); observed and unobserved characteristics of the household and year; and the individual's age, sex, and educational attainment. The equation is estimated using a linear probability model with weights corresponding to the total number of households in each village divided by the number sampled. A weighted function is used because the sample sizes in individual villages were chosen for arbitrary logistical reasons without knowledge of the village population. Without any weighting, the results would in effect greatly overweight small villages.

The empirical estimating equation differs from that in other literature on SLCP, such as Uchida et al. (2009) and Groom et al. (2007), which use changes in employment over longer time periods as a function of SLCP participation status. The approach used in this paper is more precise in that it makes use of the timing and amount of participation, which improves statistical power. This makes it possible to separate different causal channels for the effects of reforestation on employment without imposing strong assumptions, and to use a much larger set of dummy and control variables and other strategies to address omitted variable bias, correlated standard errors, and other econometric issues.

Sources of variation in the independent variable

The identification strategy in this paper is based on regressing individual-year changes in non-farm employment on household-year changes in SLCP enrollment. The process by which variation in the independent variable was determined can be described

[14] As in the rest of the paper, non-farm employment here includes local jobs as well as those in cities.

in two steps. First, households were administratively assigned land (approximately 20 years before SLCP began). Second, land was selected for enrollment in SLCP.

Nearly 90% of the household land in the sample was acquired as part of the 1980's de-collectivization of farmland (52% in 1981 alone), with most of the remainder acquired during 1990's administrative reallocations[15]. In both the 1980's and 90's, land was allocated on the basis of household demographics. Some villages allocated land based on household size, others on the size of the household labor force (the definition of which varied), and others using a combination of the two criteria (for example, allocating grain-producing land on the basis of household size and cash crop producing land on the basis of household labor). Village officials used two main procedures to determine which families received which specific pieces of land. One procedure divided village land[16] into large pieces of uniform quality, and divided each piece into equally sized plots to be distributed to each household. The other procedure was to divide a piece of land into fewer plots than the number of households in the village and use a lottery drawing to determine which household received which plot. By design, each household received a combination of good and inferior land within the village, meaning that unobserved land characteristics are likely orthogonal to household characteristics.

Whether land was enrolled in SLCP was determined by local government authorities on the basis of land characteristics, especially slope. Farmers reported having some role in the decision of whether to enroll particular plots of land for 45% of the plots in the sample, though in practice they rarely deviated from the local government's plan. Farmers tended to follow the plan in part because there was little precedent for individual farmers making land-use decisions, and in part because the decision of which plots to enroll was often obvious. Both farmers and the SFA preferred to enroll marginal steep land in SLCP, and given the nature of local topography, many villages had a bimodal distribution of land types. The timing of enrollment was determined by the SFA, and was something over which farmers had almost no control. Total new enrollment grew each year from 1999 to 2003 as the SFA gained experience and obtained larger budgets for SLCP, and then fell sharply in 2004 as global grain prices rose and the most suitable land for enrollment was exhausted. In principle, the steepest land was retired first. However, to facilitate the administration of the program, not every administrative region (county, township, or village) received quotas allowing new enrollment in every year. For example, an administrative region designated as high priority by the SFA might receive a first round of enrollment quotas in 1999 but not receive a second round until 2001. From the perspective of farmers, and even some county forestry officials, changes in the allocation and amounts of quotas from one year to the next were completely unpredictable.

In summary, whether a household enrolled land in a particular year was determined primarily by the interaction of the characteristics of their land (the variation in which within a village was originally determined by lottery) with the SFA's administrative procedures regarding the timing of enrollment. Because the analysis includes both location and year dummy variables, any selection bias story that invalidates an impact of

[15] Such re-allocations were discontinued prior to the beginning of SLCP in 1999.

[16] In fact, land normally stayed within the same "small group" within the village, though not all villages are divided into small groups. In addition, some villages that were not divided into small groups merged and became a small group of a larger village between de-collectivization and the time of the survey.

enrollment on non-farm employment would need to involve both nonrandom assignment of land and nonrandom assignment of enrollment timing conditional on land characteristics. In addition, the bias would have to affect changes in employment, not merely levels to affect the results.

The robustness section includes an additional check using an instrumental variable to guard against the remaining possibility of selection or omitted variable bias. The product of the amount of steep land at the household level and the amount of SLCP enrollment at the village-year level is used as an instrument for SLCP enrollment at the household-year level.

6. Construction of variables for empirical estimation

Dependent variable

The dependent variable, changes in employment, takes a value of 100 if the individual moves from not employed to employed via active means in a particular year, and 0 otherwise. Obtaining a job through active means, as defined in this paper, includes using a private or government employment agency, applying to advertised positions, or simply going to a city with no leads and actively seeking a job through various means in person. This category accounts for nearly 2/3 of the jobs in the sample. In contrast, passive means of obtaining a job include being placed by one's school or military unit upon graduation or completion of service, or via a friend or relative. Passive means also includes any other special circumstances under which the time that a career starts is not under the control of the job-seeker, such as a village leader whose position starts according to an election cycle, or where an employer took the initiative to seek out the worker. The distinction is important because while SLCP might cause farmers to obtain jobs through active means within a short time after enrollment, SLCP is not likely to immediately lead to new jobs obtained through passive means. Any correlation between the timing of reforestation and the beginning of jobs located through passive means is likely a result of selection bias or reverse causation.

Most jobs fit neatly into either the active or passive categories. However, how to classify jobs that were obtained through friends and relatives is debatable. The main results in the paper all classify jobs obtained through friends and relatives as passive. This classification is based on the assumption that although a job-seeker who obtains a job through friends and relatives may have initially asked them for assistance, friends and relatives are rarely in a position to locate immediate openings, and that the time between asking a friend or relative for assistance and obtaining employment through them follows a random distribution with a median of at least several years. The robustness section presents results in which this assumption is modified to re-classify jobs obtained through friends and relatives as active job searching.

Enrollment

SLCP enrollment is measured in different ways in different empirical specifications in order to test the competing theoretical hypotheses. The simplest two specifications do not attempt to distinguish among the alternative theories and merely test whether enrolling any new land is associated with employment changes, and whether there is an increasing relationship between the amount of land enrolled and employment change.

"Any new enrollment" is a dummy variable for whether a household enrolled any new land in SLCP in a particular year. A value of 1 indicates new enrollment for the household during the year, 0 no new enrollment. No procedure exists to un-enroll land[17]. "Land change" is the proportion of total household land holdings by area enrolled in SLCP in a particular year. The denominator of total household land holdings is simply current land holdings. During the period 1998-2006, there were no administrative redistributions of land, and fewer than 1% of plots were involved in a land rental between households. There was no land rental market for the relatively low quality land that would be a candidate for SLCP enrollment.

The remainder of the variables representing enrollment are used to distinguish among the competing theoretical hypotheses of why SLCP enrollment might affect employment. Both the income and liquidity effect hypotheses are tested using the grain budget change as a proxy for income change. The dependent variable as described above captures the net effect, including both income and liquidity effects. A modified dependent variable, measuring ends of non-farm careers among those who are already employed in the previous period, captures only the income effect.

The grain budget change includes two components, opportunity costs in grain production foregone, and subsidies received. Any changes in production costs and secondary crop production are ignored. Accounting is based on grain, rather than cash, because, although subsidies are now paid in cash, most land enrolled in SLCP was enrolled at a time when subsidies were paid in grain. For more detail on how the grain budget change is calculated, see Appendix 3.

Calculating labor savings is somewhat more complex. The concept of labor savings in this paper represents the difference in household labor between the labor that would have been used to grow crops on a particular plot of land if SLCP did not exist and the labor that was actually used to care for the trees on that same plot (usually negligible). Unfortunately, the labor that would have been used in the absence of SLCP cannot be directly observed, and farmers are limited in their ability to accurately recall labor use in the years before enrollment.

Farmers reported that labor requirements varied substantially by crop, but not from one plot of land to another growing the same crop. Since crop rotation is universal in the region, a farmer that grows a *less* labor-intensive crop on a plot the year before it is enrolled will save a substantial amount of labor relative to the counterfactual of having not enrolled and rotated into a more labor intensive crop the following season. In contrast, a farmer that grows a *more* labor-intensive crop on a plot the year before it is enrolled will save only a small amount of labor the following year by enrolling in SLCP[18]. A proxy for labor savings from SLCP is thus calculated as follows:

[17] One farmer, who illegally cut down the trees he had planted under SLCP and paid a fine, is coded as still enrolled in the dataset. Most farmers had no idea what the fine would be for violating their SLCP enrollment contracts and treated them as binding.

[18] The following simplified example illustrates the relationship between labor savings and crop rotation. Suppose all land in question is planted to a 2-year rotation of potato (the more labor-intensive crop) and maize (the less labor-intensive crop), a typical pattern in the study area. In 2002, households M0 and M1 plant maize and households P0 and P1 plant potato. In 2003, household M0 plants potato, household P0

$E_t - E_{t-1} = - [L_1, \ldots L_C] \bullet * [A_1, \ldots A_C]$

where
L indicates the proportion of total household crop-related labor devoted to crop c in year t-1; $\Sigma L=1$
A indicates the proportion of household land devoted to crop c in year t-1 and retired in year t; $\Sigma A \, \varepsilon \, [0 \ldots 1]$

The negative sign on the expression reflects the negative serial correlation in plot-specific labor requirements resulting from crop rotation.

7. Results

Effects on Non-Farm Employment

Table 5 presents a set of regressions of new non-farm employment on SLCP enrollment and control variables. Both the act of one's household enrolling and the proportion of a household's land enrolled are significantly associated with an increase in the probability of an individual beginning new non-farm employment. This result is robust to alternate specifications, though alternate functional forms cannot be rejected. Placing both the act of enrollment and the amount enrolled on the right-hand side makes the coefficients on both variables insignificant as a result of multicollinearity (see Table 5), while coefficients on enrollment squared are non-significant (results not shown)[19].

The coefficients represent the percentage change in non-farm employment as a function of the independent variables. The results imply that a household enrolling 40% of its land, a typical level of enrollment, would see the probability of each adult obtaining a non-farm job increase by 0.9 percentage points[20]. This corresponds to a cost of 29,000 RMB per induced job in the north or 43,000 in the south[21], large costs considering that the mean annual wage is only 7,400 RMB per year in the sample. Thus SLCP is not a cost-effective means to promote non-farm employment relative to hiring farmers to build

plants maize, and households M1 and P1 enroll in SLCP. Household M1 saves more labor in 2003 relative to household M0 than does household P1 in relation to household P0.

[19] A positive coefficient on enrollment squared would suggest that the marginal effect of enrollment increases as farmers enroll more and presumably better land, while a negative coefficient would suggest that the act of enrolling changes perceptions of the future of farming independently of the amount enrolled. Neither story is supported by the data.

[20] The magnitude and significance of the results are similar using a probit instead of a linear probability model (see Appendix 4).

[21] Because coefficients do not differ significantly between north and south and other evidence in the paper suggests that amounts of subsidies paid are not the primary determinant of program-induced employment changes, the effect of SLCP on employment is assumed to be the same on a per-hectare basis between the north and the south. With per-hectare subsidy payment rates 50% higher in the south as in the north, the cost per induced job is thus estimated to be 50% higher in the south.

public works projects. However, the program may be an effective means to sustain farmers' livelihoods while protecting the environment.

The coefficients on control variables, evaluated at the mean levels, indicate that males and young adults are most likely to begin non-farm employment, and those who have more education are less likely (because their employment rate was already high before the program). Year dummy variables indicate that the rate of movement into non-farm employment has increased over the past decade.

Distinguishing theoretical hypotheses

Table 6 presents results from a set of regressions intended to differentiate between the labor savings, income, and liquidity hypotheses. Columns 1, 3, and 7, and 8 show significantly positive effects of labor savings controlling for the amount of land enrolled[22]. This is consistent with the hypothesis that labor savings are a key channel of the effect of enrollment on employment. The results are larger and more significant among those with at least one year of education. Among those with at least one year of education, the coefficient on labor savings is also significant when only considering plots where the farmer had no choice as to whether to enroll, and therefore where reverse causation or endogenous choice are not plausible.

Columns 2, 4, and 5 show no statistically significant effect of the grain budget change (subsidies minus opportunity costs) on employment changes. These grain budget change coefficients include both the liquidity effect (expected to be positive as those who benefit from the program see constraints to non-farm employment relaxed) and the income effect (expected to be negative as those who benefit prefer more leisure). So the insignificant coefficients may be a result of the two effects canceling out, or of inadequate statistical power, as well as of the absence of either effect. The only statistically significant evidence of either a liquidity or an income effect is found in Column 6, where the income effect appears to predominate for poor households. Poor is defined here as below the sample median of housing area divided by the square root of household size in 1998.

However, results from a separate regression designed to isolate the income effect from the liquidity effect do not support the idea that there are income or liquidity effects, even for poor households. Running a regression with the end of employment, as opposed to the beginning of employment, as the dependent variable, removes the liquidity effect. Among those who already have a job, liquidity is not a plausible explanation for whether they end their employment. Among the poor who are already working, the large the benefit from SLCP the less likely they are to stop working, and for the non-poor there is no effect. These results do not support either the liquidity or the income effect, but rather suggest that stable income from SLCP may reduce cycling in and out of temporary employment among the poor. It should be noted that this result is not particularly robust in the sense that the signs are reversed among non-poor and insignificant for the entire sample. In general, the coefficients on income changes are not only insignificant but also sensitive in sign to the exact specification.

[22] All labor savings proxy results listed as significant at the 10% level in the table are significant at the 5% level using conventional standard errors.

Instrumental variables results

The program is in effect mandatory for most farmers, so selection bias is not likely to be serious enough to justify the use of a matching estimator or Heckman selection model. However, because selection bias cannot be ruled out, a set of 2-stage regressions were run with an instrumental variable.

The instrumental variable is the predicted household-year enrollment based on an interaction between household land characteristics and village enrollment timing, the underlying exogenous sources of variation in both of which are discussed earlier. The instrumental variable is constructed as follows:

$IV_{ht}=E_h*Y_{vt}$

$E_h=[A_{h1} \ldots A_{h5}]*[P_{v1} \ldots P_{v5}]'$

$\underline{Y_v}=[Y_{v1998} \ldots Y_{v2006}]$

where E=expected enrollment proportion
A=household area proportions by slope classification
P=village probability of enrollment by slope classification
Y=vector of village enrollment proportions by year
h, v, t subscripts represent household, village, and year

In the first stage (see Appendix 5), the instrumental variable significantly predicts household-year enrollment, with an R^2 of 0.25. In the second stage, measuring the effects of employment, the coefficients on predicted enrollment are at least as large as those on actual enrollment, though the standard errors are larger (see Table 7)[23]. There is no evidence of a violation of the exclusion restriction; the Hausman test statistic for the IV regression cannot be rejected (P=0.64).

Sub-sample results

The sample was divided by both individual and community characteristics. To maximize statistical power and minimize the potential for spurious results from data-mining, the sample was divided as closely as possible in half on each dimension. For age, the cutoff between young and old was set at 29.5 years, a compromise between the average age of all adults (34) and the average age of those beginning new careers (25). As shown in Table 8, the coefficient on percent land enrolled is positive in almost every sub-sample, with or without the use of the IV. The only exception is among a small number of family members of cadres, where the insignificant negative coefficient may

[23] Table 9 uses village enrollment and household slope as control variables, and includes returns to the farm to improve the power of the instrument. These modifications to the estimation have little effect on the coefficient on actual enrollment but reduce the standard errors on the instrument by controlling for its un-interacted components.

reflect family members remaining at home or on the farm to support their cadre relatives during a busy period of SLCP paperwork.

The magnitude and significance of the results does vary among sub-samples. Men and those with more education, groups that are more likely to be employed, are more likely to change their employment status in association with SLCP. It is unclear from the coefficients in Table 8 whether younger individuals, who are moving into the labor force much more rapidly than older individuals, are more likely to be affected by SLCP, or whether the effect varies by whether the household or village is poorer than the respective sample medians.

Timing of Effects

Table 9 shows that only current-year enrollment has a statistically significant effect on new non-farm employment. The positive but statistically insignificant coefficients in the y-1 row are consistent with a story in which enrollment increases non-farm employment in the year after enrollment as well as in the year of enrollment or a story in which all employment impacts occur within the year of enrollment. The coefficients in the y+1 row are results of a specification check estimating the (spurious) effect of future enrollment on past employment, and find no evidence of such a correlation. By focusing on short-term effects, the analysis gains statistical power crucial to distinguishing among different theoretical hypotheses. Although this may come at the expense of underestimating the magnitude of the long-term effects, coefficients in Table 9 do not show convincing evidence of any long-term effects beyond those that are apparent in the short-term.

8. Implicit theoretical assumptions

This section discusses five implicit assumptions in the paper.

1. Employment decisions are made without regard for future labor requirements or income streams from forest or orchard trees. The assumption on labor requirements is reasonable in the study region, where significant labor requirements associated with SLCP trees are rare. The mean adult in a household in the dataset that has enrolled spends only approximately 5 days or parts of a day per year taking care of the trees, compared to several months on farming.

For forest trees, farmers appear to essentially ignore the possibility of future income. Although they receive forest ownership certificates, their enrollment contracts state that they do not have the right to change the land use, which they interpret as meaning that they need to apply for permission to cut any trees. From farmers' perspective, whether this permission would be granted in the future is highly uncertain, and few have any clear idea of the prices or yields of local forest trees. In addition, many of the forest trees in the dataset have no commercial value, or commercial value only when grown under favorable conditions, which are not typical of SLCP land.

For orchard trees, some farmers do take seriously the possibility of future income. Table 10 presents results separately for land planted to ecological (forest) and economic (orchard) trees. In practice, nearly half of all area enrolled in the sample is difficult to classify as either economic or ecological, because it is planted to trees with low economic value such as walnut and chestnut, which as classified as economic in some villages and ecological in others. The coefficients on employment are positive for each classification of tree, and appear larger for economic trees than for ecological trees (which would be consistent with the liquidity hypothesis to the extent that planting economic trees enhances income, but not with the labor substitution hypothesis to the extent that economic trees require more labor). However, given that the sample size of unambiguously ecological trees is small, the coefficient on the effect of planting ecological trees has a very high standard error, and is not statistically different from the other coefficients.

2. New non-farm jobs reflect individual initiative as opposed to the effects of propaganda or government programs associated with SLCP. This assumption is well supported by qualitative evidence. Interviews with various forestry and local government officials indicate that no significant government employment programs associated with SLCP exist. In fact, during the peak enrollment years, many local government officials were called away from unrelated departments to help administer SLCP. Local employment programs may have in fact been short-staffed concurrently with heavy local enrollment. As an empirical test, Table 10 presents results excluding jobs found through government agencies, which are virtually identical to the main results and significant at the 1% level.

Among hundreds of pieces of SLCP-related propaganda observed on billboards and in local media during the field survey, none urged farmers enrolling in SLCP to seek non-farm employment. Most touted the ecological benefits of the program or stated that it was in the public interest.

3. Jobs found through friends and relatives should be classified as obtained through "passive" means. If instead jobs found through friends and relatives are classified as active, the results are significant only at the 10% level (see Table 10). SLCP enrollment appears to affect non-farm employment only for those who search for jobs independently, and including jobs found through friends and relatives dilutes the overall effect.

4. Households have unified preferences. The models in this paper speak of a household maximizing utility by choosing whether its members should work in non-farm jobs. In practice, different household members might disagree about whether an individual should obtain a non-farm job. Although such disagreements are not observed in the dataset, which consists of responses from the household head only, the findings are also consistent with an intra-household bargaining story.   The effect of enrollment, independent of that of the amount of enrollment, might reflect young family members using SLCP as a justification to leave the farm for work, leaving older family members behind. More formally, negotiation costs might increase the cost of processing information related to amounts of enrollment, and lead to decisions based on simplified criteria such as whether any land was enrolled. Determining whether decisions are actually made as a result of intra-household bargaining is beyond the scope of this paper.

Table 11 presents results using the household as the unit of analysis and the number or proportion of family members working as the dependent variable. This specification recognizes that decisions may be made at a household level or be interdependent for different household members, though it does not allow for the use of household fixed effects[24] or disaggregated individual-level control variables. Results are significant at the 5% level and similar in magnitude to those using individual dependent variables. An alternative approach, running the analysis at the individual level and clustering standard errors at the household level, produces slightly smaller standard errors than clustering standard errors at the village level, the more conservative approach used in the paper.

5. The effect of SLCP on employment is small in a general equilibrium sense. The paper assumes that the number of individuals seeking work because of SLCP is too small to have any effect on wage rates. This is probably reasonable given that 80% of new jobs in the sample are outside of the worker's home county, and the estimated number of workers is fewer than 1 million, or less than 1% of rural migrant labor nationwide.

9. Other identification checks

Separating voluntary from mandatory plots. As discussed earlier, farmers' autonomy is limited in practice. Unlike in analyses of the Conservation Reserve Program (Economic Research Service 2004), farmer choice is probably of secondary importance. "Mandatory" plots refer to plots that the farmer reports must be enrolled or may not be enrolled, or on which the farmer is unsure whether he has autonomy to deviate from a government plan. "Voluntary" plots refer to plots refer to plots on which the farmer believed that he had autonomy to decide whether to enroll, although even on voluntary plots enrollment was possible only at specified times, and farmers rarely deviated from the government's default plan even on voluntary plots. This in part reflects a custom of government control over land use, and in part a bimodal distribution of land types within villages (steep marginal land and flat fertile land) in which any decision-maker would likely reach the same conclusion as to which plots are suitable for enrollment.

As one might expect given limited farmer autonomy on voluntary plots, the coefficients representing effects on employment on voluntary and mandatory plot enrollment in Table 10 are almost the same (though separating plots into voluntary and mandatory does reduce the statistical power of the results).

Running a reverse causation placebo check. To the extent that enrollment is voluntary, employment changes could cause enrollment. A placebo test was run, in which household enrollment in year t was regressed on passive employment changes among household members in year t-1. For example, a household in which a family member obtained a job upon high school graduation in year t-1 and enrolled in year t would contribute to a positive coefficient in the placebo test. The results of the placebo test are both statistically and economically insignificant, meaning that there is no evidence in the data that employment causes enrollment.

Examining effects of village enrollment. Effects of SLCP on employment could potentially operate at the community level through social networks or other mechanisms. However, there is no evidence of this in the data. Table 10 shows that when both household and village enrollment are placed in the same regression, the coefficient on household enrollment remains highly significant and essentially unchanged, while the

[24] With household fixed effects, the number of independent variables would exceed the number of nonzero values of the dependent variable.

coefficient on village enrollment is small, negative, and insignificant. In addition, the survey asked farmers with whom they discussed employment opportunities; these employment social network members were often living in urban areas and were less likely to be SLCP participants than were fellow villagers.

10. Other potential biases

As in other survey-based social science, the results may be subject to a variety of measurement biases and biases in standard errors. These potential biases and how they were addressed are discussed in Appendix 6.

11. Conclusions

Like other payments for environmental services programs, SLCP attempts to achieve both ecological and economic benefits. Although the program is often mandatory in practice, its long-term success depends on providing alternative livelihoods for farmers who retire cropland. Many SLCP participants have begun non-farm employment, but in the context of China's rapid expansion of urban employment, large numbers would undoubtedly have done so in the absence of SLCP.

This paper finds that enrollment in SLCP has a significant and robust positive effect on non-farm employment. In each year of enrollment in SLCP, the probability of an individual over age 15 not currently employed in the non-farm sector beginning such employment rises from 1.4% to 2.1%, an increase of 0.7 percentage points or approximately 50%. With 15 million households enrolled throughout China and each participating household in the sample enrolling new land in an average of 1.35 different years, this corresponds to an increase in national labor supply of approximately 600,000. This estimate includes only impacts in the year of enrollment, and does not include whole-family migrations, so it is likely to be a lower bound on total labor supply changes, which could exceed one million individuals. While this is still small in relation to China's population, the effect is very large in relation to other payments for environmental services programs, the largest of which, the U.S. Conservation Reserve Program, enrolled only 430,499 farms (FSA 2008).

The results suggest that reforestation affects non-farm employment primarily via a simple labor substitution effect, not a relaxation of liquidity (Uchida 2009) or output (Groom et al. 2007) constraints. The results also point to the possibility of an offsetting income effect (at least among poor households), i.e., that the majority of participants who benefit from SLCP are less likely to enter the labor force than those who are hurt by the program. The econometric analysis is basically consistent with the stories told by farmers, that SLCP is irrelevant to employment in most households but that some use the time savings to seek non-farm employment and alternative livelihood.

The conclusions for PES schemes are mixed. Although SLCP is largely mandatory, most farmers say that they are better off as a result of participating. In that sense the program is an economic success. And it has caused farmers to move into the non-farm sector, as well as providing substantial ecological benefits that are not measured in this paper. However, the circumstances surrounding the move toward non-farm employment are not alleviated constraints but rather lowered labor productivity on the farm. PES programs can accomplish many good things, but alleviating the substantial barriers to poor farmers finding quality jobs is probably not among them.

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Table 1. Household characteristics

	Total	Std dev	Participation Yes	Participation No	Anyone new job Yes	Anyone new job No
Observations	682		573	90	377	289
Crop income 2006	2503	3188	2273	4031*	2640	2325
Crop income 1994 (0,1)	0.85		0.84	0.88	0.88	0.8*
Crop income 1998 (0,1)	0.84		0.84	0.88	0.87	0.8*
Crop income 2002 (0,1)	0.84		0.83	0.88	0.87	0.8*
Crops main income source 1994 (0,1)	0.68		0.65	0.83*	0.71	0.63
Crops main income source 1998 (0,1)	0.65		0.63	0.8*	0.69	0.6
Crops main income source 2002 (0,1)	0.6		0.58	0.76*	0.61	0.58
Livestock income 2006 (0,1)	939	3968	965	753	889	1004
Had fallow land 1998 (0,1)	0.15		0.15	0.16	0.16	0.14
House area 1998 (m2)	100	71	101	94	102	96
House area 2006 (m2)	121	88	123	108	124	117

* Indicates a difference that is statistically significant at the 0.05 level.

Table 2. Individual characteristics

Note: All variables without standard deviations listed are dummy variables.

	Total	Std dev	Participation		Employment status past 10 years				
			Yes	No	Never	Always	Began	Ended	Other
Observations	3165		2738	427	2009	436	519	85	116
Age	36	20	36	35	37	40	26	38	33
Years of education	6.1	4.1	6.1	6.1	4.9	7.4	8.9	8.3	7.8
Female (0=male)	0.49		0.48	0.51	0.57*	0.18*	0.43*	0.43	0.5
Never employed	0.64		0.63	0.67					
Always employed	0.14		0.14	0.11					
Began employment	0.16		0.16	0.16					
Ended employment	0.027		0.027	0.027					
Other	0.036		0.037	0.032					
IF CURRENTLY EMPLOYED									
Non-farm hours per day	9.4	1.8	9.4	9.2*		9.4	9.3		8.3*
Non-farm months per year	8.6	3.2	8.6	8.6		7.6*	9.6*		6.8*
Monthly earnings	857	1124	856	868		1021*	936		841
Annual remittances	2039	3397	2110	1635*		2967*	2237		1640
Working outside county	0.61		0.6	0.67*		0.46*	0.8*		0.52
Occupational sector									
Agriculture and forestry	0.026		0.029	0.008*		0.039	0.013*		0.039
Construction	0.29		0.3	0.25*		0.4*	0.22*		0.31
Industry and mining	0.23		0.24	0.21		0.2	0.3*		0.16*
Service	0.45		0.44	0.54*		0.36*	0.46		0.49
How job was found									
Active job search total	0.64		0.64	0.69*		0.63	0.66		0.46*
Just went looking in city	0.45		0.44	0.5*		0.48	0.45		0.2*
Worked through agency or advt	0.15		0.15	0.13		0.08*	0.18		0.24*

Worked through govt agency	0.046		0.045	0.061		0.067	0.03		0.019*
Passive job acquisition total	0.36		0.36	0.31*		0.37	0.34		0.54*
Arranged by friends and relatives	0.32		0.33	0.27*		0.33	0.31		0.52*
Arranged by work unit	0.037		0.037	0.043		0.04	0.026		0.019
IF EMPLOYED AT THE TIME									
non-farm days per year 1998	211	103	208	232*		197*		256*	201
non-farm days per year 2002	223	100	221	237*		203*	264*	251*	244*
non-farm days per year 2006	240	97	239	249		211*	276*		213*
IF NOT WORKING									
Primary reason not working is busy farming	0.11		0.11	0.15					

* Indicates a difference that is statistically significant at the 0.05 level.

Table 3. Land characteristics

	Total	Std dev	Enrolled Yes	Enrolled No	Anyone new job Yes	Anyone new job No
Observations	3397		1317	2080	1971	1426
Area (*mu*)	2.4	12.6	3.1*	2	2.6	2.2
Distance from home (m)	799	841	999*	673	791	810
Enrolled (1=yes, 0=no)	0.39		1	0	0.39	0.39
Slope classification (0=flat, 4=cliff)	2.1	1	2.6*	1.7	2*	2.1
Soil quality (1=good, 2=medium, 3=poor)	2.2	0.7	2.6*	2.1	2.2	2.3
Irrigation (1=paddy, 2=irrigated, 3=none)	2.8	0.5	2.9*	2.8	2.8*	2.9
Grain grown in 2006 (1=yes, 0=no)	0.51		0.03*	0.82	0.51	0.51
Ecological trees grown in 2006	0.23		0.55*	0.03	0.23	0.23
Economic trees grown in 2006	0.18		0.38*	0.06	0.18	0.18
Any intercropping allowed	0.09		0.23*		0.09	0.09
Had a choice of whether to enroll or not	0.44		0.39*	0.46	0.43	0.45

* indicates a difference that is statistically significant at the 0.05 level.

Table 4. Correlation between new enrollment and new employment

A tabulation of individual-year events for all individuals over age 15 from 1998-2006

		Household enrolled new land: No	Yes	Total
Individual began non-farm employment	No	13601	1747	15348
	Yes	528	85	613
	Total	14129	1832	15961

Table 5. Baseline Regression Results

	(1)	(2)	(3)	(4)^	(5)	(6)^^	(7)
Percentage of land enrolled	2.323	0.848		1.721	2.218	2.323	
	(2.10)**	(0.38)		(1.70)*	(2.19)**	(2.12)**	
Any new enrollment (0/1)		0.931	1.323				1.236
		(0.87)	(2.52)**				(2.58)***
Education	-0.328	-0.329	-0.329	-0.250		-0.328	
	(2.38)**	(2.39)**	(2.39)**	(1.95)*		(2.51)**	
Education^2	0.024	0.024	0.024	0.018		0.024	
	(1.69)*	(1.70)*	(1.70)*	(1.49)		(2.14)**	
Age	-0.405	-0.406	-0.406	-0.380		-0.405	
	(5.79)***	(5.79)***	(5.79)***	(7.10)***		(7.33)***	
Age^2	0.003	0.003	0.003	0.003		0.003	
	(5.11)***	(5.12)***	(5.11)***	(5.45)***		(6.02)***	
1998	-1.092	-1.093	-1.093	-1.019	0.000	-1.092	0.000
	(3.88)***	(3.89)***	(3.88)***	(4.12)***	(.)	(3.06)***	(.)
1999	-1.767	-1.779	-1.779	-1.594	-0.558	-1.767	-0.576

	(4.73)***	(4.85)***	(4.86)***	(5.48)***	(2.55)**	(4.09)***	(2.73)***
2000	-1.050	-1.066	-1.060	-1.033	0.055	-1.050	0.041
	(2.09)**	(2.11)**	(2.11)**	(2.57)**	(0.14)	(2.11)**	(0.11)
2001	-1.029	-1.051	-1.050	-1.213	0.241	-1.029	0.216
	(2.18)**	(2.24)**	(2.24)**	(3.14)***	(0.60)	(2.19)**	(0.54)
2002	0.051	-0.001	-0.003	-0.080	0.947	0.051	0.895
	(0.10)	(0.00)	(0.01)	(0.23)	(2.15)**	(0.10)	(1.90)*
2003	0.129	0.043	0.040	0.207	0.945	0.129	0.861
	(0.23)	(0.07)	(0.07)	(0.46)	(1.90)*	(0.25)	(1.65)*
2004	1.010	0.982	0.977	0.980	1.700	1.010	1.664
	(2.38)**	(2.37)**	(2.33)**	(2.36)**	(4.00)***	(1.88)*	(3.98)***
2005	2.033	2.018	2.020	2.354	2.644	2.033	2.624
	(4.31)***	(4.21)***	(4.19)***	(4.86)***	(7.42)***	(3.64)***	(7.16)***
2006	2.915	2.900	2.896	2.719	3.442	2.915	3.421
	(5.80)***	(5.76)***	(5.77)***	(5.23)***	(8.96)***	(4.74)***	(8.90)***
Constant (2007 omitted)	12.265	12.271	12.273	11.551	0.907	12.265	0.914
	(5.96)***	(5.96)***	(5.96)***	(8.22)***	(8.46)***	(7.53)***	(8.38)***
Observations	15553	15553	15553	15562	17111	15553	17111

Robust absolute z statistics in parentheses; *** 1%, ** 5%, * 10% significance
All estimations include a complete set of household dummy variables (not shown)
Estimations include probability weights & robust (village clustered) standard errors unless noted
^(4) does not use probability weights
^^(6) reports conventional standard errors
Dependent variable: Whether individual obtained a new job during the year (0/100)

Table 6. Distinguishing theoretical hypotheses

	(1)	(2)	(3)	(4)	(5)	(6)^	(7)^^	(8)^^^
Percent land enrolled	6.847	3.297	7.112				5.000	4.584
	(2.47)**	(1.93)*	(2.29)**				(2.62)**	(2.41)**
Labor savings proxy	4.125		4.059				3.091	3.069
	(1.91)*		(1.91)*				(2.04)**	(1.92)*
Male (0/1)	2.317	2.317	2.316	2.318	2.318	1.351	0.729	0.719
	(6.57)***	(6.57)***	(6.57)***	(6.57)***	(6.57)***	(3.37)***	(3.62)***	(3.41)***
Education	-0.336	-0.336	-0.336	-0.338	-0.338	-0.112		
	(2.44)**	(2.44)**	(2.44)**	(2.44)**	(2.44)**	(1.00)		
Education^2	0.025	0.025	0.025	0.025	0.025	0.008		
	(1.72)*	(1.72)*	(1.72)*	(1.72)*	(1.72)*	(0.88)		
Age	-0.408	-0.408	-0.408	-0.408	-0.407	-0.386	-0.363	-0.367
	(5.85)***	(5.85)***	(5.85)***	(5.85)***	(5.85)***	(3.73)***	(8.39)***	(8.35)***
Age^2	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003
	(5.17)***	(5.17)***	(5.17)***	(5.17)***	(5.16)***	(3.38)***	(7.11)***	(7.17)***
Income effect		-0.000	-0.000	-0.000	0.000	-0.001		

		(0.54)	(0.40)	(0.28)	(1.33)	(3.74)***		
Any new enrollment				1.556				
				(2.12)**				
Constant	11.262	11.270	11.262	11.272	11.261	9.804	8.049	8.118
	(6.19)***	(6.19)***	(6.19)***	(6.18)***	(6.20)***	(3.83)***	(10.30)***	(10.09)***
Observations	15508	15508	15508	15508	15508	7322	18588	18182
R-squared	0.11	0.10	0.11	0.10	0.10	0.08	0.04	0.04

^(6) Poor households only (below median in house area/sqrt(household size) in 1998)
^^(7) Includes only individuals with at least one year of education.
^^^(8) Includes only individuals with at least one year of education and redefines enrollment as enrollment of plots on which the farmer had no choice of whether to enroll.
All estimations include a complete set of household and year dummy variables (not shown)
Dependent variable: Whether individual obtained a new job during the year (0/100)

Table 7. Instrumental variable estimation

	(1)	(2)	(3)^	(4)^	(5)^^	(6)^^
Predicted enrollment (IV)		4.737		2.731		4.780
		(2.51)**		(1.89)*		(3.12)***
Percent land enrolled	2.739		2.047		2.702	
	(3.38)***		(2.54)**		(3.03)***	
Village percent land enrolled	-1.266	-1.639	-0.765	-0.978	-1.305	-1.701
	(1.96)**	(1.84)*	(1.18)	(1.41)	(2.16)**	(2.18)**
Average slope classification	0.058	0.033	-0.126	-0.130	0.026	-0.000
	(0.26)	(0.15)	(0.75)	(0.82)	(0.11)	(0.00)
Male (0/1)	0.711	0.709	0.687	0.685		
	(2.73)***	(2.73)***	(5.01)***	(5.04)***		
Education	-0.080	-0.081	-0.046	-0.046		
	(1.78)*	(1.76)*	(1.07)	(1.03)		
Education^2	0.008	0.009	0.008	0.008		
	(2.75)***	(2.73)***	(2.52)**	(2.49)**		
Age	-0.381	-0.380	-0.379	-0.379		
	(18.22)***	(18.48)***	(15.61)***	(15.55)***		
Age^2	0.003	0.003	0.003	0.003		
	(16.91)***	(17.10)***	(14.48)***	(14.41)***		
Any cadre income 1998	0.385	0.420	0.393	0.421		
	(0.73)	(0.80)	(0.85)	(0.91)		
Below median housing per capita (0/1)	-0.298	-0.312	-0.341	-0.350		
	(1.72)*	(1.71)*	(3.46)***	(3.48)***		
Constant	8.606	9.769	6.079	6.024	0.375	0.370
	(19.51)***	(14.27)***	(12.34)***	(13.07)***	(0.56)	(0.61)
Observations	17905	17905	17905	17905	19315	19315
R-squared	0.02	0.02	0.02	0.02	0.01	0.00

Robust t statistics in parentheses
** significant at 5%; *** significant at 1%

^(3), (4) no probability weights
^^(5), (6) no demographic control

variables
All estimations include a complete set of village and year dummy variables (not shown). Village percent land enrolled is a village-year variable; average slope classification is a household-level variable. Dependent variable is change in employment for any reason (including returns to the farm) (-100,0,100).

Table 8. Sub-sample Results

A. IV Results by Individual Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Percent land enrolled (using IV)	6.938	1.398	2.400	7.672	7.889	1.660
	(3.21)**	(0.17)	(0.55)	(3.00)**	(4.34)***	(0.45)
Village percent land enrolled	-3.365	0.680	-0.881	-3.022	-3.341	-0.584
	(4.00)**	(0.18)	(0.54)	(2.45)*	(3.02)**	(0.39)
Average slope classification	-0.212	0.425	0.059	-0.030	-0.129	0.189
	(1.51)	(0.92)	(0.19)	(0.17)	(0.31)	(1.21)
Male (0/1)	0.077	1.980			1.214	0.275
	(0.47)	(3.60)**			(3.28)**	(1.19)
Education	-0.051	0.213	-0.036	-0.057	0.208	-0.176
	(1.01)	(1.15)	(0.58)	(0.85)	(0.42)	(0.53)
Education^2	0.007	-0.009	0.006	0.007	-0.007	0.034
	(1.31)	(1.46)	(1.00)	(1.05)	(0.33)	(0.60)
Age	-0.043	1.301	-0.286	-0.490	-0.551	-0.187
	(1.00)	(1.19)	(17.73)***	(9.81)***	(8.49)***	(4.53)***
Age^2	0.000	-0.040	0.002	0.004	0.005	0.002
	(0.70)	(1.69)	(23.92)***	(8.44)***	(7.23)***	(4.84)***
Any cadre income (0/1)	-0.402	2.081	0.437	0.284	0.801	-0.173
	(2.59)**	(1.21)	(0.62)	(0.51)	(1.05)	(0.96)
Below median housing per capita (0/1)	-0.215	-0.338	-0.002	-0.540	-0.538	0.041
	(1.82)	(0.68)	(0.01)	(6.09)***	(1.39)	(0.24)
Constant	2.290	-9.639	6.165	8.927	7.303	3.813
	(.)	(0.85)	(4.63)***	(19.05)***	(2.23)*	(2.92)**
Observations	11929	5976	9136	8769	8540	9365
R-squared		0.03	0.02	0.02	0.02	0.01

(1) Ages 30 and up; (2) Ages 15-29; (3) Female; (4) Male; (5) At least some secondary school; (6) Primary school or no education

B. Results by individual characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Percent land enrolled	1.598	4.806	1.866	3.678	4.432	0.838
	(3.91)**	(3.16)**	(2.31)*	(1.77)	(3.19)**	(2.56)*
Village percent land enrolled	-1.720	-0.268	-0.721	-1.838	-2.256	-0.352
	(3.77)**	(0.12)	(0.76)	(1.70)	(1.77)	(0.66)
Average slope classification	-0.108	0.363	0.068	0.049	-0.070	0.205
	(0.76)	(0.82)	(0.22)	(0.31)	(0.16)	(1.30)
Male (0/1)	0.075	1.985			1.212	0.275
	(0.47)	(3.54)**			(3.29)**	(1.19)
Education	-0.053	0.218	-0.036	-0.056	0.184	-0.176
	(1.02)	(1.21)	(0.59)	(0.84)	(0.37)	(0.53)

Education^2	0.008	-0.010	0.006	0.007	-0.006	0.034
	(1.38)	(1.54)	(1.02)	(1.06)	(0.27)	(0.60)
Age	-0.045	1.306	-0.286	-0.491	-0.552	-0.187
	(1.10)	(1.19)	(17.71)***	(10.08)***	(8.64)***	(4.52)***
Age^2	0.000	-0.040	0.002	0.004	0.005	0.002
	(0.80)	(1.69)	(23.88)***	(8.70)***	(7.33)***	(4.81)***
Any cadre income (0/1)	-0.358	2.030	0.443	0.320	0.849	-0.168
	(2.72)**	(1.11)	(0.64)	(0.58)	(1.12)	(0.91)
Below median housing per capita (0/1)	-0.238	-0.335	-0.004	-0.547	-0.548	0.039
	(2.03)*	(0.68)	(0.01)	(5.79)***	(1.36)	(0.23)
Constant	1.989	-12.354	6.563	8.458	7.246	4.173
	(1.43)	(1.11)	(3.99)**	(12.74)***	(2.02)*	(3.11)**
Observations	11929	5976	9136	8769	8540	9365
R-squared	0.01	0.03	0.02	0.03	0.03	0.02

(1) Ages 30 and up; (2) Ages 15-29; (3) Female; (4) Male; (5) At least some secondary school; (6) Primary school or no education

C. IV results by household and community characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Percent land enrolled (using IV)	2.490	9.366	5.636	-8.542	3.134	5.257
	(0.53)	(1.92)	(2.96)**	(1.34)	(0.27)	(2.85)**
Village percent land enrolled	-0.924	-3.333	-1.979	0.361	-0.939	-2.754
	(0.49)	(4.93)***	(2.29)*	(0.14)	(0.32)	(5.13)***
Average slope classification	-0.052	0.042	-0.009	0.727	0.149	-0.041
	(0.14)	(0.13)	(0.05)	(1.15)	(1.03)	(0.11)
Male (0/1)	0.937	0.372	0.709	0.519	0.970	0.566
	(2.21)*	(2.03)*	(2.66)**	(0.77)	(7.75)***	(1.51)
Education	-0.068	-0.072	-0.097	0.283	-0.106	-0.051
	(0.94)	(0.70)	(2.01)	(0.55)	(1.49)	(0.75)
Education^2	0.007	0.009	0.011	-0.023	0.008	0.009
	(1.26)	(1.20)	(2.62)**	(0.67)	(1.51)	(1.42)
Age	-0.423	-0.344	-0.366	-0.609	-0.380	-0.387
	(10.28)***	(9.42)***	(13.47)***	(2.07)*	(20.03)***	(9.63)***
Age^2	0.004	0.003	0.003	0.006	0.003	0.003
	(9.14)***	(8.29)***	(12.32)***	(1.79)	(14.62)***	(8.30)***
Constant	9.676	3.836	4.504	15.401	8.376	6.310
	(9.40)***	(5.63)***	(7.61)***	(2.71)**	(11.10)***	(5.81)***
Observations	9692	8261	16755	1150	8599	9354
R-squared	0.03	0.02	0.02	0.06	0.03	0.02

(1) Above median per capita housing area in 1998; (2) Below median per capita housing area 1998; (3) No cadre income in 1998; (4) Cadre income in 1998; (5) Village average income >650 RMB/year; (6) Village average income <650 RMB per year

D. Results by household and community characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Percent land enrolled	3.120	2.107	2.700	2.842	3.481	2.430
	(2.70)**	(2.09)*	(4.37)***	(0.67)	(5.46)**	(1.91)

Village percent land enrolled	-1.128	-1.398	-1.108	-3.405	-1.001	-1.777
	(1.18)	(1.92)	(1.65)	(1.85)	(1.18)	(1.94)
Average slope classification	-0.063	0.209	0.048	0.579	0.144	0.018
	(0.15)	(0.64)	(0.24)	(0.85)	(1.52)	(0.05)
Male (0/1)	0.938	0.379	0.707	0.546	0.970	0.566
	(2.19)*	(2.02)*	(2.66)**	(0.81)	(6.69)***	(1.51)
Education	-0.067	-0.078	-0.097	0.279	-0.106	-0.051
	(0.94)	(0.75)	(2.02)*	(0.53)	(1.49)	(0.74)
Education^2	0.007	0.010	0.011	-0.023	0.008	0.009
	(1.24)	(1.30)	(2.64)**	(0.67)	(1.78)	(1.41)
Age	-0.423	-0.344	-0.366	-0.614	-0.380	-0.388
	(10.26)***	(9.17)***	(13.69)***	(2.12)*	(22.60)***	(9.75)***
Age^2	0.004	0.003	0.003	0.006	0.003	0.003
	(9.09)***	(8.03)***	(12.53)***	(1.83)	(15.83)***	(8.38)***
Constant	9.719	6.316	7.899	15.472	8.394	6.035
	(10.97)***	(17.15)***	(16.76)***	(3.07)**	(29.82)***	(5.21)***
Observations	9692	8261	16755	1150	8599	9354
R-squared	0.03	0.02	0.02	0.07	0.03	0.02

(1) Above median per capita housing area in 1998; (2) Below median per capita housing area 1998; (3) No cadre income in 1998; (4) Cadre income in 1998; (5) Village average income >650 RMB/year; (6) Village average income <650 RMB per year

All panels of Table 8 include complete sets of year and village dummy variables (not shown) and cluster standard errors at the county level. The dependent variable is change in employment for any reason (including returns to the farm) (-100,0,100). Robust t statistics are shown in parentheses. ** significant at 5%; *** significant at 1%.

Table 9. Timing of effects

	(1)	(2)	(3)	(4)	(5)
Percent land enrolled	2.118	2.182	2.334	1.998	2.006
	(2.48)**	(2.58)**	(2.51)**	(2.39)**	(2.32)**
Percent land enrolled year-2			-0.337		
			(0.63)		
Percent land enrolled year-1		0.486	0.419		
		(0.62)	(0.55)		
Percent land enrolled year+1				-0.971	-0.887
				(1.09)	(1.12)
Percent land enrolled year+2					-0.048
					(0.06)
Male (0/1)	0.620	0.647	0.618	0.765	0.857
	(3.37)***	(3.34)***	(3.04)***	(3.65)***	(3.49)***
Education	-0.171	-0.146	-0.127	-0.222	-0.191
	(3.05)***	(2.48)**	(1.93)*	(3.08)***	(2.71)***
Education^2	0.012	0.010	0.008	0.014	0.008

	(2.45)**	(2.06)**	(1.37)	(2.38)**	(1.19)
Age	-0.312	-0.368	-0.449	-0.333	-0.309
	(8.55)***	(8.74)***	(9.30)***	(8.16)***	(8.23)***
Age^2	0.003	0.003	0.004	0.003	0.003
	(8.23)***	(8.21)***	(8.20)***	(7.78)***	(7.34)***
Constant	7.443	5.367	6.895	28.196	9.495
	(8.39)***	(6.48)***	(7.98)***	(20.58)***	(9.67)***
Observations	22416	20440	17925	19852	17335

All estimations include a complete set of household and year dummy variables (not shown).
Dependent variable: Whether individual obtained a new job during the year (0/100)

Table 10. Robustness checks

	(1)	(2)^	(3)^^	(4)	(5)	(6)
Percent land enrolled				2.647		2.354
				(2.34)**		(2.15)**
Percent land enrolled voluntary	2.429		2.469			
	(1.48)		(1.51)			
Percent land enrolled mandatory		2.278	2.307			
		(1.43)	(1.44)			
Village proportion land enrolled				-0.638		
				(0.86)		
Percent land enrolled ecological					0.235	
					(0.13)	
Percent land enrolled economic					3.637	
					(1.76)*	
Percent land enrolled ambiguous					2.496	
					(1.43)	
Male (0/1)	2.459	2.459	2.461	2.505	2.461	2.119
	(6.13)***	(6.11)***	(6.13)***	(6.39)***	(6.13)***	(6.57)***
Education	-0.393	-0.393	-0.393	-0.398	-0.392	-0.299
	(2.75)***	(2.76)***	(2.76)***	(2.82)***	(2.76)***	(2.23)**
Education^2	0.028	0.028	0.027	0.028	0.027	0.022
	(1.79)*	(1.79)*	(1.79)*	(1.80)*	(1.79)*	(1.52)
Age	-0.424	-0.424	-0.424	-0.424	-0.424	-0.368
	(5.90)***	(5.90)***	(5.90)***	(5.88)***	(5.90)***	(5.85)***
Age^2	0.003	0.003	0.003	0.003	0.003	0.003
	(5.10)***	(5.09)***	(5.10)***	(5.07)***	(5.10)***	(5.13)***
Constant	10.973	10.104	10.047	11.925	10.838	10.186
	(5.04)***	(4.72)***	(4.68)***	(6.31)***	(5.27)***	(6.07)***
Observations	13847	13847	13847	13901	13847	15564
R-squared	0.11	0.11	0.11	0.11	0.11	0.10

^(2) Excludes jobs found through government agencies

^^(3) Re-classifies jobs found through friends and relatives as active

All estimations include a complete set of household and year dummy variables (not shown)
Dependent variable: Whether individual obtained a new job during the year (0/100)

Table 11. Results using household-level dependent variables

	(1)	(2)	(3)	(4)
Percent land enrolled	0.025	0.108	0.026	0.109
	(2.02)**	(2.32)**	(2.02)**	(2.32)**
Poor (0/1)	-0.001	0.000	-0.001	-0.004
	(0.32)	(0.04)	(0.51)	(0.41)
Household head education			0.000	0.002
			(0.66)	(0.88)
Household head age			0.003	0.024
			(3.24)***	(7.46)***
Household head age^2			-0.000	-0.000
			(3.24)***	(7.17)***
Constant	0.009	0.060	-0.046	-0.399
	(2.55)**	(3.72)***	(2.56)**	(6.61)***
Observations	5950	5950	5860	5860
R-squared	0.02	0.02	0.02	0.03

Robust t statistics in parentheses
** significant at 5%; *** significant at 1%

Dependent variable is change in proportion of household members with non-farm employment (including new jobs found using both active and passive methods, as well as decreases) for Columns 1 and 3, or change in number of household members with non-farm employment for Columns 2 and 4. Includes a complete set of village and year dummy variables (not shown).

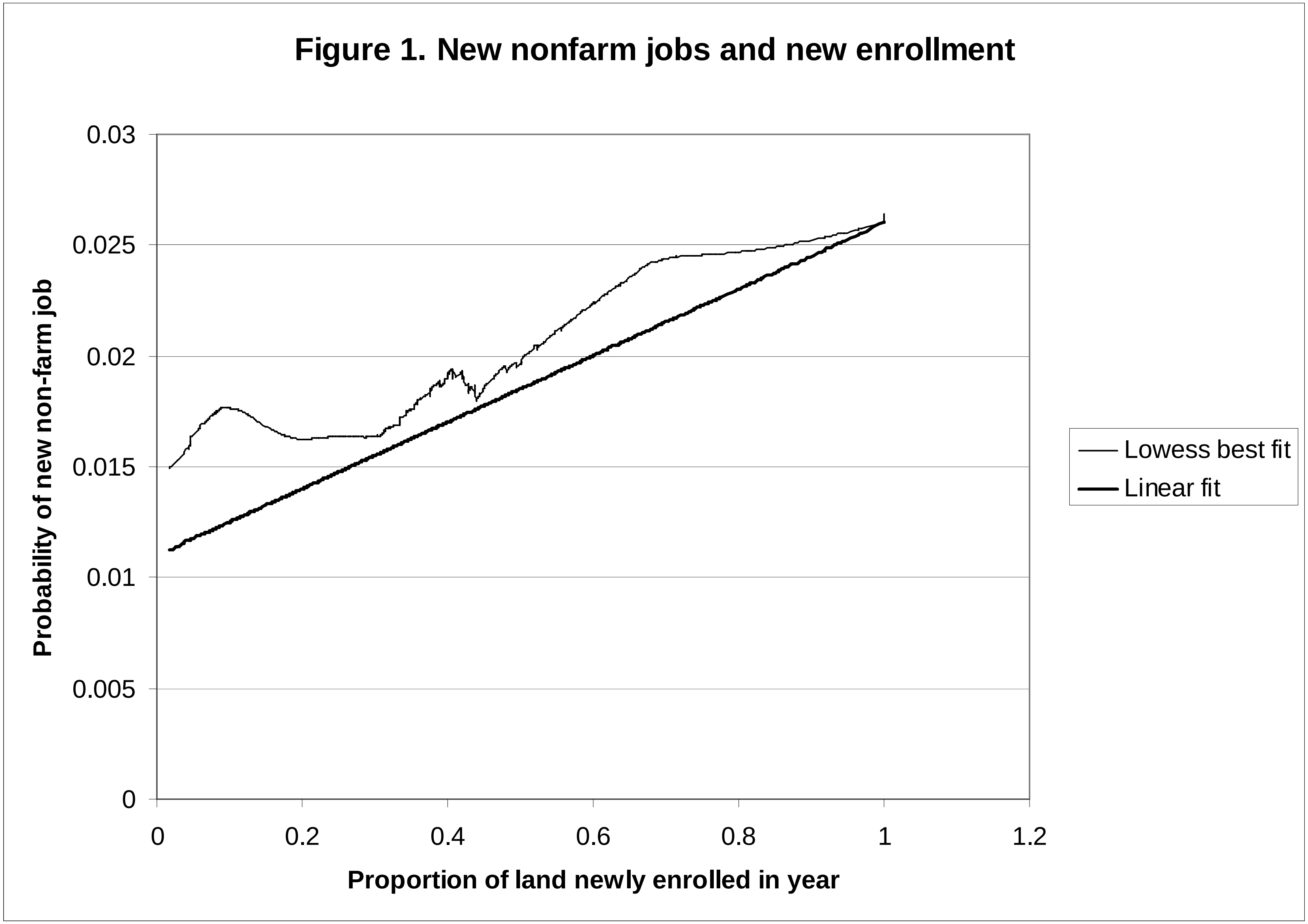
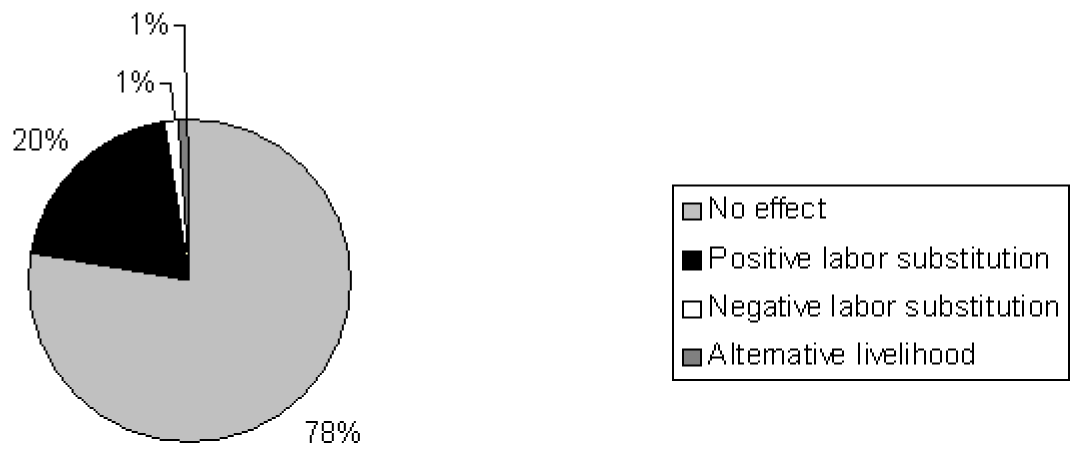


Figure 1. New nonfarm jobs and new enrollment
0.03
0.025
0.02
0.015
0.01
0.005
0
Probability of new non-farm job
0
0.2
0.4
0.6
0.8
1
1.2
Proportion of land newly enrolled in year
Lowess best fit
Linear fit

Figure 2. Farmer opinions on the effect of SLCP on non-farm employment



Appendix 1. Sample enrollment contract

Related policy and explanation

1. For each *mu* retired, the central government will deliver 200 *jin* of grain per year, and 20 *yuan* in cash, for a period of 5 years.
2. For land enrolled in retire land return forest (grassland) including cropland and wasteland, tree seedlings or grass seeds will be provided free of charge by the county forestry or livestock department. Costs of economic tree seedlings above 50 *yuan* per *mu* are the farmer's responsibility.
3. According to the principle of "Who plants trees (grass) manages them and gets the benefits", usufructory rights to land under the program are extended to 50 years; during the term of the contract, the farmer is allowed to bequeath, transfer, rent, mortgage, or sell land rights according to relevant laws; if the farmer is unable to contract, rights may be transferred in an open auction or to a tenant, but the land use may not be changed, the land may not be returned to cultivation, otherwise the land will be confiscated and other penalties may be imposed according to applicable laws and regulations.
4. According to the "Retire 1 return 2" policy, for every *mu* of cropland retired, in addition to planting trees or grass on that one *mu*, should plant trees or grass on one nearby *mu* of wasteland; where the area of wasteland is large, they should practice "retire 1 return 3" or even more.
5. The content of this card is standardized by the county (municipality, district) people's government forestry administration, and is assigned a serial number. Each plot (place) of land has one card, and shall be stamped by the county (municipality, district) forestry administration, livestock bureau, land bureau, grain bureau, township people's government, village committee, and signed by the participating household, at which time it shall constitute a contractual relationship.
6. This card and other related documents should be presented to the local designated location to receive grain and cash and the legal forest (grassland) ownership certificate; upon loss, immediately apply for a replacement.
7. Seven copies of this card will be produced, and distributed to and retained by the participating household, local village committee, township people's government, and county forestry, livestock, grain, and land departments.

County (municipality, district) Forestry Bureau: Inspection notes __________ Stamp________ Date_________

County (municipality, district) Livestock Bureau: Inspection notes __________ Stamp________ Date_________

County (municipality, district) Grain Bureau: Inspection notes __________ Stamp________ Date_________

County (municipality, district) Land Bureau: Inspection notes __________ Stamp________ Date_________

People's Township Government: Inspection notes __________ Stamp________ Date_________

Villager's Committee: Inspection notes __________ Stamp________ Date_________

Participating household signature or stamp ___________ Date ____________

Appendix 2. Questionnaire design and survey management

The survey comprised two questionnaires, a household questionnaire for the household head and a village questionnaire for the village accountant. Most of the household questionnaire consisted of questions about either specific plots of land (with additional questions for plots of land that had been reforested) or specific household members (with additional questions for those with non-farm employment).

The household questionnaire collected information on each plot of land, including detailed topographic, agronomic, and economic characteristics, as well as the legal status of each plot. Topographic characteristics include the size, the location on a map, the location in relation to roads and other geographic features, the slope, and the exposure. Agronomic characteristics comprise fertility, irrigation status, drought sensitivity, and cropping history (including any reforestation trees, multiple croppings, and/or intercroppings). Economic characteristics include the crop-yield history, grazing history, labor history, and willingness to accept a hypothetical rental (with or without reforestation enrollment). Legal status encompasses how and when the plot was acquired, the type of land tenure arrangement, whether the plot was enrolled in the reforestation program, and whether the farmer reported that he had had a choice about whether to enroll the plot.

Additional questions were asked about plots that had been enrolled in the reforestation program, including a special set of questions for those that had been enrolled and planted to orchards. These questions inquired about the year enrolled, the length of the contract, direct and opportunity costs associated with enrollment, tree survival, ecological sustainability, and carbon sequestration. For plots that had been planted to orchards, questions investigated orchard-establishment costs and labor requirements, the amount and timing of expected yields, and several measures of the success of the orchard in relation to expectations at the time of planting. The section on plot characteristics included a number of questions about the program that had not appeared in previous questionnaires, some designed to determine which enrollment options the farmer faced (must enroll, may not enroll, or had a choice) on an individual-plot level.

Some basic demographic information was collected about all household members, such as age, sex, relationship to the household head, education, and the timing and circumstances of entering or leaving the household within the past ten years. Additional questions were asked about family members with non-farm employment: their occupations, work locations, amounts of time worked, the history of how much they worked, their incomes, their remittances, and how they had found their jobs. For those family members not currently working non-farm, the survey determined why they were not working and whether they could have found a job if they had wanted to. This section was much more detailed than that of most previous surveys, in that it asked for a detailed employment history of individual family members.

As in previous surveys, household-level questions permitted a tabulation of various assets and income sources. However, the questionnaire also included questions about the farmers' perceptions about the program rules, some contingent valuation questions, and some open-ended questions about the program. Income source questions allowed a detailed accounting of the amounts of reforestation subsidies and when they were received. The questions on the farmers' perceptions of program rules were much more detailed than those asked in previous questionnaires. These questions elicited farmers' opinions about minimum and maximum enrollment levels within and outside of contiguous areas designated by the village; minimum and maximum enrollment levels by slope; whether the government had permitted planting orchards on land designated for ecological forest; and whether farmers had expected to have the right to cut the trees. Respondents were also asked a set of questions about whether they had been formally or informally prohibited from fallowing land before the program began; and for those who had planted orchards, about why they hadn't planted orchards before the program. Contingent valuation questions included the present value of the right to cut the trees (if farmers thought they had had such a right), and questions regarding hypothetical orchard returns designed to elicit discount rates and risk preferences. Finally, farmers were asked a set of open-ended questions about whether they believed that reforestation had had an effect on labor markets, and about why the specific plots they retired had been chosen for the program.

The questionnaire also included a set of questions designed to easily identify low-quality data, such as whether someone other than the household head was interviewed, and whether village officials were present at the interview. While most questions in the survey related to time-invariant variables, those that involved recall were carefully pre-tested. The only recall questions in the questionnaire relate to easily recalled information such as employment status, not to levels of income or consumption.

The village questionnaire contained all of the same questions asked of the farmers about program rules, plus basic data about village demographics, infrastructures, employment, and cropping. The most detailed section of the village questionnaire asked for a division of village land by land use and slope, and for the amount of area reforested in each year of program implementation. Reforestation area was broken down by slope and by type of tree planted, for each year of program implementation. Out of 44 villages in the sample, one was a national model forestry village *(qianjia lvhua cun)*, in which farmers were encouraged to remain in the village to take special care of SLCP trees. Data from this village are included in the descriptive statistics but not in any of the regression results because of the distinct data-generating process for effects on employment. The results in the paper should be generalized only to normal villages, not to the small number of national model forestry villages.

To the extent possible, all stages of the survey were conducted in accordance with management principles in the FAO publication "Conducting Agricultural Censuses and Surveys". The survey team consisted of 16 Master's students in Economics and Forestry from the Northwest Sci-Tech University of Agriculture and Forestry, and two team leaders, from the same university. A training manual was developed specially for the survey, and a pre-test conducted, both with the help of faculty and students at the Center for Chinese Agricultural Policy in Beijing who had worked for numerous rural economic surveys. All questionnaires were checked a minimum of three times for carelessly omitted information, by the enumerator, a peer, and at least one supervisor, and meetings were held every evening to discuss issues that had arisen during the day.

Beijing BOYA Information Technology, a data-entry contractor, entered the data according to a set of specially written procedures. A spot check revealed that they met their goal of 99.9% accuracy.

All members of the survey team were financially independent from the State Forestry Administration, and none reported any conflicts of interest. Most interviews were conducted privately with farmers in their homes, with no one who might have had a stake in the outcome of the research present. Enumerators and supervisors were paid nearly twice what they had earned for similar surveys in the past, and were offered an incentive to find flaws in the questionnaire. Enumerators were not paid by the questionnaire, and there was no evidence that any completed questionnaires had been fabricated.

Appendix 3. Assumptions used to calculate grain budget changes associated with SLCP enrollment.

Each plot prior to reforestation is assumed to have been planted with a locally stylized 5-year crop rotation, regardless of the actual crops planted in the particular year prior to enrollment. The rotation includes three summers of maize, three winters of wheat, and the remainder of the time other crops or fallow. Such a rotation is typical in the dataset, where land is planted about 60% of the time to maize or wheat, 20% to other crops, and the rest of the time fallow. Because farmers have great difficulty recalling production costs, and because prices and yields for secondary crops vary widely, the model focuses on harvests of maize and wheat, and assumes that production costs and revenues from secondary crops are roughly equal. In other words, every hectare produces 3/5 of a hectare of maize and 3/5 of a hectare of wheat annually, with zero production costs attributable to the maize and wheat.

The coefficient of annual variation in field-specific maize and wheat yields is assumed to equal 0.4, based on agronomic studies of non-irrigated maize-wheat rotations in northwest China (Huang et al. 2005; Fan et al. 2005). Based on this estimated coefficient of variation, long-term adjusted mean grain yields are calculated as follows from yields reported for the year before SLCP enrollment:

• Reported yield/0.6 when the farmer describes the reported yield as “below average”

• Reported yield when the farmer describes the reported yield as “about average” or is unsure of its place in the distribution

• Reported yield/1.4 when the farmer describes the reported yield as “above average”

If the plot was not planted to maize or wheat in the year before enrollment, or the farmer is unable to estimate the yields, the mean adjusted maize or wheat yield prior to enrollment for all enrolled plots in the village is used. In most villages in the sample, most enrolled plots are found in a contiguous area. Substituting mean yields at the household level would not be appropriate; land characteristics are negatively correlated within households due to the way in which land was distributed during de-collectivization.

Farmers receive subsidies according to official payment rates, but do not receive associated management fees. The survey questionnaire asked farmers whether they actually received subsidies that were owed to them. Virtually all participants eventually received their subsidies, though many received them late as a result of paperwork delays or poor tree survival. Because many farmers often reported receiving subsidy rates rounded up as well as down from the official payment rates, variation in payment rates reported at the farm level was ignored (and assumed to result primarily from recall bias rather than actual variation in the program implementation). Many villages retained management fees (10-15% of the total subsidies) to cover the cost of managing trees at the village level, or charged miscellaneous fees such as shipping and handling for seedlings. For simplicity, the management fees are coded as “not received” by farmers.

Appendix 4. Results using probit instead of a linear probability model.

The following regressions use county dummy variables and a time trend. Household and year dummy variables cannot be used in the probit specification because including them leads to large numbers of perfectly predicted outcomes and unstable coefficients. The reported coefficients are probit elasticities.

Appendix 6 Table 1. Baseline regression results using probit specification

Dependent variable: New non-farm employment (0/1)

	(1)	(2)	(3)	(4)^	(5)	(6)^^
Any new enrollment (0/1)		0.204	0.307			
		(1.13)	(3.48)***			
Proportion land enrolled	0.52	0.216		0.458	0.463	0.52
	(3.09)***	(0.62)		(2.84)***	(3.33)***	(3.58)***
Male (0/1)	0.431	0.431	0.431	0.443		0.431
	(8.59)***	(8.58)***	(8.56)***	(9.13)***		(7.03)***
Education	0.125	0.125	0.125	0.158		0.125
	(3.76)***	(3.79)***	(3.77)***	(5.45)***		(3.65)***
Education^2	-0.005	-0.005	-0.005	-0.007		-0.005
	(2.66)***	(2.63)***	(2.61)***	(4.21)***		(2.85)***
Age	-0.013	-0.013	-0.013	0.004		-0.013
	(0.72)	(0.72)	(0.71)	(0.27)		(0.95)
Age^2	0	0	0	-0.001		0
	(0.94)	(0.94)	(0.94)	(2.56)**		(1.14)
Cadre in 1998 (0/1)	-0.107	-0.109	-0.109	-0.053		-0.107
	(0.65)	(0.66)	(0.66)	(0.37)		(0.92)
Poor in 1998 (0/1)	-0.115	-0.114	-0.113	-0.127		-0.115
	(1.53)	(1.49)	(1.50)	(2.00)**		(1.78)*
Year	0.08	0.08	0.08	0.082	0.065	0.08
	(7.52)***	(7.73)***	(7.79)***	(9.65)***	(7.31)***	(6.37)***
Observations	15552	15552	15552	15561	17111	15552

Robust absolute z statistics in parentheses; *** 1%, ** 5%, * 10% significance

All estimations include a complete set of county dummy variables (not shown)

Estimations include probability weights & robust (village clustered) standard errors unless noted

^(4) does not use probability weights

^^(6) reports conventional standard errors

Appendix 5. First-stage instrumental variable results.

Variable	Coefficient
Instrument	0.594
	(3.35)**
Village percent land enrolled	0.075
	(1.37)
Average slope classification	0.005
	(1.35)
Male (0/1)	-0.000
	(0.45)
Education	-0.001
	(0.50)
Education^2	0.000
	(0.95)
Age	-0.000
	(0.11)
Age^2	0.000
	(0.20)
Any cadre income (0/1)	0.009
	(1.27)
Below median housing per capita (0/1)	-0.005
	(3.31)**
Constant	0.398
	(19.39)***
Observations	11929
R-squared	0.25

Robust t statistics in parentheses
** significant at 5%; *** significant at 1%
Dependent variable: Proportion land enrolled.

Coefficients on dummy variables for village and year are not shown.

Appendix 6. Measurement biases and biases in standard errors

1. Measurement error, recall bias, and attenuation bias. Data may have been measured imprecisely, especially as a result of recall bias or with respect to the exact timing of enrollment and employment changes. In contrast to the less clear memories of certain variables that are not used in the paper, farmers appeared to be able to clearly remember the key variables of enrollment and employment changes. Enumerator training stressed the importance of clearly defining and eliciting the exact timing of these changes. To the extent that the timing of these changes was not reported precisely, estimated coefficients would be biased downwards as a result of attenuation bias.

Although the hypothesis that results are biased by measurement error cannot be rejected, there is no evidence in the data of measurement error in timing. Such measurement error would not only bias the estimated coefficient on enrollment in year t downwards, but also bias the coefficients on enrollment in t+1 and t-1 upwards, and the analysis fails to reject the null hypotheses that these coefficients are zero. Results are robust to excluding the control variables, some of which may be subject to greater measurement error or recall bias, and using merely the two key variables with location and time dummy variables as controls.

Another potential source of measurement error is late deliveries of grain subsidies resulting from administrative delays. To the extent that subsidies were delivered in a later year, the timing of their effects could be mis-specified. However, most subsidies were delivered on-time, and the expectation of subsidies likely affected behavior even before the late subsidies were actually received.

2. Attrition and non-response bias. A weakness in the survey is that it does not capture whole-family migration. Any family that migrated to urban employment as a result of SLCP and left no members behind in the village to respond to the survey was not sampled. In addition, enumerators found during pre-testing that older individuals did not know the answers to many of the questions in the survey, and thereby excluded a small number of households with no members between the ages of 18 and 65. Therefore, the analysis may underestimate the effect of SLCP on employment.

Several strategies were used to address this bias. First, the year-by-year employment status of all individuals who had been members of the household at any time within the past decade was recorded, including their status before they joined the household and after they left. The only missing values for employment status were for individuals who were not alive as of the year in question. Second, total off-farm migration was compared to national statistics. The migration to new jobs in other counties in the sample extrapolates to a national rural-to-urban migration of approximately 150 million individuals over the period 1998-2006, which is roughly comparable to national estimates. This suggests that the under-counting of whole-family migration may be small. Third, results were weighted according to the number of households in 1998 as opposed to the number of households in 2006 (see Table 10); results are essentially unchanged and remain significant at the 1% level. (The baseline results are weighted by 2006 instead of 1998 households because changes in the number of households estimated by village accountants between 1998 and 2006 often reflected limited information about villages that had been separate prior to mergers occurring during the past decade, and did not reflect households who migrated but did not change their official residency status from rural to urban.)

3. Unobserved sample separation problem. The analysis estimates average treatment effects under the assumption that treated and untreated observations are comparable. The results could be misleading if different subsets of the sample are subject to different data-generating processes. On the other hand, dividing the sample according to criteria determined after the data were collected is a form of data snooping. Therefore, the sample is not sub-divided for the main analysis. As a robustness check, results were calculated from subdividing the sample according to most of the criteria in Groom et al. (2006)[25]. For no sub-sample are the results of the opposite sign as presented in the paper and statistically significant.

4. Effects of outliers. The independent variables for land and labor effects are proportions and do not contain any outliers. The non-significance of the liquidity and income effects may be a result of the effects of influential observations from unusually large farms. Such influential observations could bias estimated coefficients and standard errors, and may even be derived from different data-generating processes. Table 10 presents results with income changes rescaled as ranks within the sample. The coefficient remains small and insignificant.

5. Errors correlated at levels other than the village. To the extent that policies are centralized and multiple stages of clustering were used during sampling, clustering standard errors at the village level may not be the most conservative approach. Table 10 presents results clustered at other levels of aggregation. Results remain significant at the 5% level with standard errors clustered at the township or county-year levels. Because there are only six counties, clustering at the county level would not yield consistent estimates. Income effects remain statistically insignificant when rescaled as ranks within the sample to minimize the effects of outliers.

6. Observer expectancy effect. As in most social science research, enumerators were aware of the purpose of the study. Respondents were not told the exact hypotheses to be tested in this paper until after answering the quantitative section, but may have inferred that a major purpose of the survey was to determine the effects of SLCP enrollment on employment. Although the timing of employment and enrollment changes were recorded in different formats separated by two pages, responses to one could potentially have biased responses to the other to the extent that the purpose of the study was not blind. For example, a respondent might have been more likely to report a piece of land that was retired in the fall of year t but not planted with trees until the spring of year t+1 as enrolled in year t if there was a change in household employment in year than if there were a change in household employment in year t+1.

Such an observer expectancy effect could potentially explain a coincidence of enrollment and employment changes, but appears unlikely to explain the correlation between the size of the land enrolled and employment changes. In addition, the time component of the instrumental variable is based on information from others in the village and does not include the reported timing of the farmer's own enrollment.

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[25] The village questionnaire did not include a question parallel to that in the survey used by Groom et al. 2006 on whether land rentals among villagers were restricted. Because land rentals are rare, especially for marginal land potentially suited to SLCP, and done on an ad hoc basis, such a question was not considered meaningful.

Appendix 6 Table 1. Alternative weighting and clustering

	(1)^	(2)	(3)^^	(4)^^^
Percent land enrolled	2.234		2.330	2.330
	(2.07)**		(2.23)**	(2.45)**
Male(0/1)	2.321	2.311	2.315	2.315
	(6.56)***	(6.57)***	(6.46)***	(6.64)***
Education	-0.338	-0.336	-0.336	-0.336
	(2.39)**	(2.43)**	(2.53)**	(2.59)***
Education^2	0.025	0.025	0.025	0.025
	(1.70)*	(1.73)*	(1.74)*	(2.09)**
Age	-0.409	-0.405	-0.405	-0.405
	(5.72)***	(5.79)***	(5.22)***	(6.70)***
Age^2	0.003	0.003	0.003	0.003
	(5.07)***	(5.11)***	(4.50)***	(6.07)***
Rank of income effect		-0.000		
		(0.04)		
Constant	12.268	12.238	12.216	12.216
	(5.85)***	(5.53)***	(5.65)***	(6.64)***
Observations	15553	15553	15553	15553
R-squared	0.10	0.10	0.10	0.10

Robust t statistics in parentheses
** significant at 5%; *** significant at 1%

^ (1) weighted by 1998 population
^^ (3) Standard errors clustered by township
^^^ (4) Standard errors clustered by county-year
All estimations include a complete set of household and year dummy variables (not shown)

Dependent variable: Whether individual obtained a new job during the year (0/100)

Chapter 2. Sources of inefficiency in a massive payments for environmental services program

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Abstract: As the world's largest payments for environmental services program, China's Sloping Land Conversion Program (SLCP) has reforested vast areas of environmentally sensitive farmland since 1999 and provided subsidy payments to millions of poor farmers in mountainous western China. The massive program has seen administrative problems, with some communal wasteland areas enrolled and receiving subsidies that are not officially eligible and some farmers enrolling and not receiving subsidies to which they are entitled. This paper documents the scale of these problems and develops several hypotheses regarding the behavioral sources of them. The hypotheses are tested using a unique household-level dataset. On average, villages reported 72% more area enrolled in the program than was actually the case, and 15% of enrolled farmers received at least a portion of the subsidies to which they were entitled late or not at all. The paper finds that both misaligned incentives and low managerial ability contribute to inefficient outcomes. Villages that are poor and remote (less able to fund administrative costs without over-reporting and less likely to be audited) over-reported more, while farmers were less likely to receive subsidy payments to which they were entitled if the village leader had lower managerial ability and a larger village to manage. In the Sloping Land Conversion Program, finely tuned targeting that might be optimal in a smaller program is impractical due to administrative costs.

1. Introduction

Many of the world's poor live in mountainous and other ecologically fragile regions. Whether poverty stems from geographic conditions, contributes to ecological degradation, or merely happens to exist in many fragile environments, the attraction of a program that promises to both reduce poverty rates and improve the environment is obvious. In recent years, a number of developing countries, including Costa Rica, Mexico, and China, have implemented payments for environmental services (PES) programs (see for example, Hyde et al. 2003; Mayrand and Paquin 2004; and Xu, Z et al. 2005; Alix-Garcia et al. 2005). Such programs aim to achieve the dual goals of poverty reduction and ecological restoration by paying farmers to adopt sustainable practices, often by planting trees to reduce soil erosion.

China's Sloping Land Conversion Program (SLCP), formerly known as Grain for Green, is by far the largest PES program in any developing country. Although official figures probably overstate the area enrolled, the SLCP is approaching its target of 15 million hectares by 2010, roughly the area enrolled in the Conservation Reserve Program. At more than $2 billion per year, the SLCP's budget exceeds that of the Conservation Reserve Program (even without adjusting for differences in purchasing power parity), and is roughly equal to the total government budget of Costa Rica. More than 15 million households are participating, more than the total number of farm households in the United States. Even in a country with the population of China, SLCP is the third most widespread rural investment project, behind roads and irrigation systems (Zhang et al. 2006).

The SLCP, begun in 1999, is the most recent of a series of Chinese government programs to replant marginal cropland and barren hillsides, but the first that resembles a modern PES program[26]. Most of the enrolled area is in western China, the poorest area of the country and the one facing the most serious erosion hazards[27]. As is common in other PES programs, the SLCP has more than one objective. It aims to reduce erosion and restore ecological balance, to support farmers' incomes, and in the longer term after the subsidies expire, to move farmers into other employment endeavors, such as growing high-value crops or taking on non-farm employment. At this time, carbon sequestration is not an official goal of the program.

[26] Since 1949, the State Forestry Administration (SFA) has sponsored a number of programs to reforest steep land, using a combination of forestry-administration staff and villagers mobilized in campaign-style efforts. Total reported reforestation has actually exceeded the total area of China, because marginal land has been repeatedly planted with trees, either after the trees fail to survive or after the land was temporarily returned to grain production. The SLCP, begun in 1999, is the first program in China to resemble a modern PES program.

[27] The loess plateau of the Yellow River basin in northwest China has the highest erosion rate in the world, with deep gullies a prominent feature of the landscape in large sections of several provinces. In the western reaches of the Yangtze River, upstream of the Three Gorges Dam reservoir, farmers traditionally grow maize on mountainsides much too steep to cultivate with machinery. Serious wind erosion and desertification plague much of China's northwest.

The SLCP is, in principle, a voluntary program similar to the Conservation Reserve Program. However, in China there is no private ownership of farmland, and executive departments have substantial leeway in implementing laws, meaning that participation is in practice mandatory for many farmers.

Issues in implementation

Although most farmers say that they are better off as a result of participating, a number of issues have arisen in the implementation of the program. This paper focuses on what are perhaps the two most important implementation issues. First, some farmers have not received the subsidy payments to which they were entitled under the program, in full or in part, or have received payments late. Second, in many areas, local governments have reported non-agricultural land as being enrolled in SLCP, collecting subsidies on such land. Planting trees on non-agricultural wastelands, including areas that are too steep to plant crops, is within the spirit of the program, but only former cropland is supposed to receive subsidy payments. (The subsidies are intended to compensate farmers for the opportunity costs of not growing grain.)

Related literature

This paper draws on two strands of economic literature in addition to that on payments for environmental services, on misappropriation of funds from central governments or land acquisitions, and on biases in government statistics.

Misappropriation of funds by local governments can be a significant problem in developing countries. For example, Reinikka and Svensson (2004) find that only 13% of education grants in Uganda reach local school districts. The rest was captured by local government officials or politicians and used for campaigns or other purposes unrelated to education. In China, there is no history of farm subsidies prior to the Sloping Land Conversion Program, and land taxes on farmers were only recently eliminated. Thus there is no other directly comparable program to the Sloping Land Conversion Program.

In recent decades, large areas of agricultural land in China have been converted to industrial parks and other urban uses as part of the country's rapid development. The compensation received by farmers as part of such projects is typically much less than the value of the land. For example, during the development of Pudong, the eastern district of Shanghai, local governments sold land to developers for an estimated 90 billion *yuan* more than they paid to farmers (Ding 2007). Under China's 1998 Land Law, local governments can acquire collectively owned farmland, convert it to state-owned land, and then sell it for development, as long as they are acting in the public interest (Cao et al. 2008). Compensation levels are set at 6-10 times the annual value of crops grown, plus a resettlement allowance of 4-6 times, up to a maximum of 30 times annual productivity, which has created losers among landless farmers (He et al. 2009). Owing to ambiguity in land rights, proceeds from land sales for development are distributed among different stakeholders (farmers, villages, local, and central governments) in different ways in different areas, with farmers sometimes receiving as little as 5-10% of the total (Tao and Xu 2007).

Cost exaggerations, similar to the over-reporting of areas enrolled, are common in development projects. Although there is little grassroots auditing of the SLCP, Olken (2007) suggests that top-down auditing may actually be more effective, at least for Indonesian road building projects. In China, government economic statistics on rural areas have often been biased for many decades. Until recently, even economic studies often used biased techniques such as typical example sampling, a Soviet-influenced style in which typical representatives of different social classes were selected in order to show contradictions in socio-economic conditions (Travers 1982). Cai (2000) reviews reasons that village cadres have mis-reported statistics in China, including exaggerating farmers' incomes in order to increase their own salary, exaggerating grain outputs, and so on.

Issues in the context of the program

It should be noted that in some aspects of the program local governments are actually more generous to farmers under the program than national regulations would suggest, and that in the aggregate participants are probably better off than if national regulations were exactly followed in all respects. The details of SLCP implementation vary greatly from one location to another, as a result of both differing ecological conditions and decisions made by lower levels of government. In general, however, local governments make exceptions to rules limiting subsidies for orchard trees and when tree survival was poor. Farmers receive annual subsidies of 140-210 *yuan* per *mu* (approximately $300-400 per hectare, comparable to the opportunity costs of retiring good land) and are often allowed to grow orchard trees. In the sample, farmers reported that 57% of enrolled plots saw net income improve after enrollment, and only 6% saw a decrease in net income, with the rest seeing little change.

National regulations stipulate that no more than 30% of each county's reforested area can be planted to orchard trees. However, this regulation is routinely waived and the actual percentage is substantially higher, especially if one considers chestnut, walnut, and prickly pear as orchard trees. The definition of ecological forest varies from one place to another, and whereas it in principle admits only native forest trees, it is often generously interpreted to include native walnut trees or even orchard trees that are planted closely to help control erosion. In principle, payments last 5 years for orchard trees, and 8 years for ecological trees, although in the dataset most farmers even those planting orchard trees have 8-year contracts. Extensions were also formalized nationwide after the date of the survey. Many farmers said that planting orchard trees had simply not been customary before the program, and that the technical assistance provided as part of the program was as important a reason for planting orchards as were the subsidies.

Defining whether subsidies received

Whether farmers receive subsidies owed under the program is often not an all-or-none matter. In the early years of the program, ambiguous regulations and overwhelming paperwork led many plots to be enrolled before they had been approved. Most of these plots were eventually approved, but farmers often received their subsidies several years late, and in earlier surveys (Xu 2007) such payments were counted as never having been received. In most cases where subsidy payments did not begin on time, the ending date of the subsidies was also extended, as though the land had been retired at the time that the paperwork was completed.

To this day many farmers receive only the subsidies themselves, 140-210 *yuan* per *mu* to compensate for the opportunity costs of enrollment, and do not receive the 20 *yuan* per *mu* annual management allotment that national regulations also mandate. Local governments say that they withhold management fees only when the government, and not the farmers, provides management services for the reforested plots. In many villages, farmers reported other small fees, such as seedling delivery charges and document printing fees, and in some cases their subsidies have been applied to back taxes owed to the village. Although national regulations are vague, in general, central authorities consider it acceptable for local governments to charge administrative fees.

This paper classifies late subsidies payments as having been received late, and classifies subsidies as fully received if all subsidies except for the 20 *yuan* per *mu* management fee have been received by the farmer.

2. On-time payment performance

In some villages, fewer than half of farmers sampled have received all subsidies that they were owed on time, while in other villages all farmers reported receiving subsidies on time. Figure 1 shows the percentages of participating farmers in the sample receiving subsidies on time, late, and not at all (as of June 2007) by village. On time in Figure 1 (and in the rest of the paper) means that all subsidies that the farmer was owed in any year were delivered on time. "Never" in Figure 1 means that at least some portion of the subsidies that a farmer was owed have never been delivered. Late means that at least some of the subsidies owed were paid late, but that all were paid eventually.

Figure 1 shows subsidy payment performance grouped by township and county. The pairs of bars represent two villages sampled in each township. Each of the six large clusters represents a different county. Payment performance varies by a statistically significant margin at all three administrative levels, county, township, and village. In regressions of whether subsidies were paid on time (0/1) on sets of village, township, and county dummy variables (not shown), the F-statistics are significant at the 1% level in each regression. The P-value associated with the F is slightly lower for the county than for the other administrative levels, but township and village dummy variables remain jointly significant even when including county dummy variables.

Payments to sampled farmers are less likely to be received on time in the southern counties (the first three clusters in Figure 1) than in the northern counties (the last three clusters in Figure 1). The southern counties have official subsidy payment rates 50% higher than the northern counties with similar or slightly lower opportunity costs in the form of grain yields. (Nationwide, southern areas have higher grain yields, but this pattern does not hold within the relatively small study region, which includes the boundary between high and low subsidy payment rates.) Several factors have been proposed to explain the lower on-time performance in the southern counties. One is that local governments withheld part of the subsidies in the southern counties for public use with the justification that the subsidies were excessive in relation to opportunity costs of enrollment. Another factor is that the central government did not clarify the definition of northern and southern regions until 2000. In 1999, the boundary between northern and southern regions for purposes of determining subsidy rates followed provincial boundaries, placing the entire Shaanxi Province in the northern region. In 2000, the State Forestry Administration clarified the definition such that different counties in the same province would have different subsidy rates if some counties were in the Yellow River watershed and other counties were in the Yangtze River watershed. Thus southern counties in the sample retroactively changed from one subsidy rate to another, which may have resulted not only in late payments for the relatively small group of farmers enrolling in 1999 but also a paperwork backlog affecting later enrollees. A third factor is that there may have been administrative inefficiency specific to the first county, which accounts for most of the difference between the northern and southern counties. Qualitative evidence suggests that these three factors explain most of the difference between northern and southern counties in their on-time performance.

Late payment and non-payment of subsidies has been a problem throughout the program's implementation (see Figure 2). Some forestry officials said that the large amounts of enrollment in the peak years (especially 2003) resulted in administrative backlogs and late subsidy payments. Others said that unexpected cuts in enrollment quotas in 2004 (which coincided with a rise in grain prices and a new Prime Minister) led some villages to plant trees on land that they expected to be approved for enrollment but was not actually approved. Whatever the reason, farmers in the sample reported late payments related to new enrollment in every year of the program's implementation. Some of the "never" payments shown in Figure 2 may later become late payments, especially those in recent years, but whatever become of the never payments there is still a significant problem with late payments.

Figure 2 shows 10% non-payment or late payment, compared to 19% in Figure 1. There are two reasons for this difference. First, Figure 1 counts households as not having received subsidies if subsidies owed to them in any year were not received. For example, a household that enrolled new land in two different years and was paid on time for one of those two years would contribute to only the late count in Figure 1 but to both the late and on time counts in Figure 2. Second, Figure 2 is weighted by the number of observations per village, but Figure 1 is not. The number of households visited in each village was smaller in the first county, where the most serious problems with non-payment were found, than in other counties.

3. Over-reporting of enrolled areas

Over-reporting of areas enrolled is another widespread problem in the program implementation. The survey team conducted separate surveys of farmers and village accountants to ask how much land area was enrolled. The response rate for this question was 100% for farmers, but only 80% (35/44 villages) for village leaders. Some village accountants did not know or have good records of the amount of land enrolled in their village, or were only willing to provide a range. Others did not know how many farmers had enrolled in their village and were unwilling to allow the survey team to sample farmers who had not enrolled in the program, making it impossible to compute the amount enrolled per participant in official records. Given the sensitive nature of the question of areas enrolled to village officials, those who did not respond may have over-reported by a larger margin than those who did respond.

Table 2 shows that official records of areas enrolled systematically exceed the areas reported by farmers. The difference is so large as to be significant at the 1% level even with only 35 observations. Farmers reported 5 *mu*[28] enrolled per household, whereas village records showed an average of nearly 8 *mu* per household. On average, village leaders reported 2.6 *mu* per household more of area enrolled than did farmers, which is nearly 0.5 standard deviations. The average ratio of village to farmer reported enrollment was 1.72, and the average of the log of this ratio was 0.32, all highly significant. The 25th percentile, median, 75th percentile, maximum, and inter-quartile range were all higher for the village figures than for the farmer figures, though the difference in median was not as large as the difference in mean and the minimum was slightly lower in the village data.

The difference here between household-reported and village-reported areas enrolled is not a result of the well-known factors that cause household-reported consumption to be lower than national accounts data. The surveys also asked for total land areas, including both land enrolled and not enrolled in the program. The total land per household reported by farmers was actually slightly higher than that reported by village accountants, which is not consistent with a story of farmers forgetting about a fraction of their land. In fact, farmers are probably less likely to forget to mention land that is enrolled in the program of interest to the study and on which they are entitled to subsidy payments. Farmers were asked not for the total amount of land, but rather for the size of each piece of land in the household. It was not possible for a farmer to report a piece of land and its size without being asked whether it was enrolled in the program. It is also not plausible that the difference is a result of missing some very large enrollees, which would be comparable to one of the problems that can occur when comparing consumption to national accounts data. Land was divided almost equally during the de-collectivization process of the 1980s and 1990s demographics-based land redistributions, land sales are not possible, and less than 1% of the land in the sample has been rented from one household to another.

Figure 3 shows the distribution of farmer and village reported areas graphically. The data are sorted in ascending order by farmer reported area, which is shown with horizontal tick marks. The black vertical bars around the ticks represent the 95% confidence intervals for the farmer-reported areas, extrapolating from the farmers sampled to all farmers in each village. The confidence intervals are relatively narrow because in most villages the variance in areas enrolled among households was small, reflecting the fact that land was distributed equitably during de-collectivization not only in terms of area but also in terms of land quality. Generally, each household in a village obtained during de-collectivization a similar amount of low-quality land, the type of land that was later eligible to be enrolled.

The figure clearly shows that some villages have reported much larger areas enrolled than what farmers say is actually enrolled. However, some villages have

[28] 15 *mu*=1 ha

reported a smaller area enrolled than what farmers say is enrolled, and by statistically significant margins. In these villages, trees were planted in anticipation of receiving enrollment quotas, and the actual quotas fell short of those that had been applied for.

Figure 4 shows that farmers and village leaders do not agree about the areas enrolled in most villages. In 49% of villages, the village leader's report exceeds the farmer's report by a statistically significant margin. In 31% of the villages, it falls short of what the farmers say is enrolled, and in only 20% of villages is there no statistically significant difference between the land areas reported by farmers and village leaders. This indicates that problems in the program implementation are widespread in the sampled region.

4. Hypotheses regarding program implementation

The above evidence shows that both problems are widespread, but leaves open the question of why they are occurring. Do they represent a calculated attempt to balance local interests with the possibility of sanctions from higher levels of government? Do they reflect the realizations of uncertain outcomes? Or do they simply reflect low managerial ability? This paper proposes and tests three hypotheses to explain the sources and variation in problems with the program implementation.

1. Preferences for administrative evaluation hypothesis. Village and township leaders are evaluated as civil servants according to a system in which points are added up from different categories for purposes of job security and promotion. SLCP forms up to 1/3 of the agriculture section of the performance evaluation, which is one of many components of the overall evaluation.

Assume that the local government has a potential resource endowment E. This endowment can be consumed by the local leader in the form of program performance or as a numeraire good N. The leader maximizes the following optimization problem (presented using a Cobb-Douglas utility function, an assumption that simplifies the mathematics but is not necessary for the conclusion):

$$\max [P_1, P_2]\ U = (P_1+P_2+P_3)^a N^{1-a}$$

$$\text{s. t. } P_1+P_2+N = E$$

where a is a coefficient that varies from one village to another representing the weighting of the performance evaluation in the village leader's utility function

- P_1 represents on-time delivery performance
- P_2 represents accurate areas enrolled performance
- P_3 represents administrative performance in all other areas
- E is assumed to be a constant across villages

Combining the two equations gives

$$\max [P_1, P_2]\ U = (P_1+P_2+P_3)^a (E- P_1-P_2)^{1-a}$$

Taking a derivative with respect to P_1, setting the result equal to zero, and eliminating the denominator gives

$(P_1+P_2+P_3)^a((a-1)P_3-P_1-P_2+aE) = 0$

Since $\Sigma P > 0$,

$(a-1)P_3-P_1-P_2+aE = 0$ at the optimum

This implies that

$\delta P_1/\delta a = \delta P_2/\delta a = P_3+E$

or $\delta P_1/ \delta P_2=1$

Since P_3+E is positive, the first expression shows that the greater the weight that the village leader places on the performance evaluation, the better the performance of both on-time delivery and accurate reporting of areas enrolled. The second expression shows that there will be a positive correlation between on-time delivery and accurate reporting at the village level. Although there is no causal relationship between the two problems, they share a common cause, the village leader's unobserved preference for numeraire goods relative to administrative evaluation points.

2. Weighted utility maximization hypothesis. An alternative way to explain variation in over-reporting and non-payment is with a weighted utility maximization model in which the parameters differ by village.

In the weighted utility maximization problem for over-reporting, the local leader chooses the area to enroll (A) and the area to report but enroll (V) in order to maximize a function that includes the local government budget, the expected subsidies paid to farmers (assuming that the plan is approved and farmers get their subsidies) minus the opportunity costs of not growing grain, and possible penalties associated with over-reporting areas enrolled.

The utility for local government spending can be represented by

$(V+E)^a$

where E=enterprise-related income
a ε (0,1) and represents concave utility of government spending
and the subsidy rate is defined equal to 1

The utility for farmers can be represented by

$A(1-b)-A^2$

where b=grain yield before enrollment

and the squared term on A represents convex costs associated with enrolling land of increasing quality

The utility loss associated with penalties for over-reporting can be represented by

$pV/(L-A)$

where p=probability of getting caught in an audit (which increases with the accessibility of the village)
L=total land area of the village

The more over-reporting in relation to the area not actually enrolled in the village, and the more likely an audit is to occur, the higher the expected cost of penalties.

Assuming additively separable utility for the local leader and mandatory enrollment in practice from the farmer's perspective, the leader's maximization problem becomes

$\max [V,A]\ U=(V+E)^a + (A(1-b)-A^2) - pV/(L-A)$

Taking derivatives with respect to V and A, setting the results equal to 0, and dropping the denominators gives the following expressions:

$(L-A)a(V+E)^a-p(V+E) = 0$

$pV+L^2(b+2a-1)+L(-2ab-4a^2+2a)+ a^2b+2a^3-a^2 = 0$

Totally differentiating these expressions gives V' (E), V' (P), and V' (b)

$V'(E) = -1, <0$

$V'(p) = (L-A)a^2(V+E)^a-p(V+E)$
$=a[p(V+E)]- p(V+E), <0$

$V'(b) = (L-a)^2/p, \ >0$

These expressions say that the less enterprise-related income, the lesser the probability of audit, and the higher the crop yields, the more over-reporting.

A corollary to this hypothesis relates to the non-payment of subsidies. Assume that the village leader enrolls land before it is known what land area will be approved for enrollment. If the area approved is smaller than expected and the village leader has planted trees only on farmland (no over-reporting), farmers will not receive subsidies promptly. Therefore, the more over-reporting the fewer problems with subsidy delivery. This implies that the signs of the effects of enterprise income, audit probability, and crop yields on non-payment will be the opposite of those on over-reporting.

3. Managerial ability hypothesis. Assume that everyone faces the same incentives but that managing the program can be difficult in some villages. Thus the probability of subsidies not being received on time can be represented as follows:

P(N)=P(M)*(1-P(V))*(1-P(F))

where N=nonpayment (or late payment)
M=mistake arises
V=village leader catches mistake
F=farmer catches mistake
P(M)=f(administrative difficulty)
P(V)=f(village leader's managerial ability)
P(F)=f(farmer's managerial ability)

In other words, the more difficult the administrative task faced by the village leader, and the lower the managerial abilities of the village leader and the farmer, the more likely subsidies will not be paid on time. Administrative difficulty and managerial ability are not directly observable, but proxy variables do exist in the dataset. The size of a village is a proxy variable for the difficulty of administering the program in that village. Whether a village leader had an official stamp readily available to sign the survey form is a proxy variable for the village leader's managerial ability. Whether the farmer owns a small business is a proxy variable for the farmer's managerial ability.

5. Testing the hypotheses

Hypothesis #1

The data are not consistent with hypothesis #1 (see Figure 5). Because of the small number of observations, there is no statistically significant linear relationship between over-reporting and non-delivery at the village level. Some villages have problems with over-reporting, and others with non-delivery, and in other villages performance is good on both measures. What is significant is that no village in the sample is among the worst on both measures. Among villages in which the log of village leader reported area to farmer reported area exceeds 0.5, none have more than 30% of farmers not receiving subsidies on time. If the distributions of over-reporting and non-delivery were independent, the probability of this occurring would be only 2.8%. Under the null hypothesis of an economically significant positive relationship between over-reporting and non-delivery, the probability of such an outcome would be even lower. Among such villages with significant over-reporting, 5/12 have no delivery problems, compared to only 6/23 villages with less over-reporting.

The data in Figure 5 are not consistent with hypothesis #1, but are consistent with an alternative story of the relationship between over-reporting and non-delivery. In this story, which is similar to the way in which many local officials describe the implementation of the program, over-reporting is a way to ensure that farmers get their subsidies. Because of paperwork backlogs, the State Forestry Administration often delivered trees, which must be planted shortly after delivery and at a specific time of year, before the subsidies have been approved for a particular piece of land. If the village accurately reports the amount of farmland enrolled, the subsidies might or might not be approved for the full area. If the village reports a larger area of farmland enrolled than is actually the case, extra subsidies for wasteland areas are available in case the full amount applied for is not approved. This story can explain why there is not a necessary relationship between over-reporting and non-payment—sometimes applications are approved in full and farmers get their subsidies even without over-reporting. Over-reporting is sufficient, but not necessarily necessary, for farmers to get their subsidies on time. At higher levels of government, non-approvals of enrollment quotas may be in part related to the pervasiveness of over-reporting.

Hypothesis #2

The data are consistent with Hypothesis #2 (see Table 3). In spite of the small number of observations, all predictions have the expected sign and most are statistically significant. (The table shows the results of individual bivariate regressions, the more conservative way of presenting the results. In multivariate regressions that include more than one of the independent variables, significance levels are slightly higher.)

The estimated elasticity is highest for maize yields. A 1% increase in maize yields (using administrative records from 1998 before the program began) is associated with 0.4% increase in the ratio of village leader reported to farmer reported enrollment. In villages with high yields of maize, the most important crop in the region, farmers may prefer to continue growing maize. This would reduce the amount of actual cropland enrollment without a corresponding reduction in wasteland areas that might be planted to trees and receive subsidies that were supposed to be paid only for former cropland. Maize yields before the program were relatively low at 0.27 MT/*mu*, but do exceed China's 1998 mean maize yield of 0.35 MT/*mu* in some sampled villages (FAO 2010).

Elasticity is also relatively high, 0.25, for the distance to the county seat. The further away from the county seat a village is located, the more likely it is to over-report the amount of area enrolled. This finding, which is more significant in a multiple regression, is consistent with a story in which auditors focus enforcement efforts targeting over-reporting on easily accessible villages. Although the mean distance is only 21 km, most of the villages in the sample are located in mountainous regions, often with no paved roads, and specific pieces of land within less-accessible villages can be even less accessible to inspectors and land survey teams.

The most statistically significant finding, though the one with the lowest estimated elasticity, is the relationship between village enterprises and over-reporting. The more enterprises in the village the lower the level of over-reporting. The mean number of enterprises is 2, but most villages in the sample have no enterprises, and therefore little revenue from taxes or equity interests. Villages with no enterprises may over-report more because they see few alternatives in covering administrative costs or making up budgetary gaps should their full area actually enrolled not be approved. There was no significant relationship between village income and over-reporting and village income, suggesting that budgetary pressures, as opposed to poverty, lie behind over-reporting.

Hypothesis #3

The data are consistent with each prediction of hypothesis #3. All proxies for managerial ability are associated with better delivery of subsidies.

The probability of a farmer receiving any subsidy payments late was only 11% in a village where the village leader stamped the survey form, compared to 31% in a village where the village leader did not stamp the survey form. This difference is highly significant statistically as well as economically (See Table 3). Out of 44 villages in the sample, 17 did not stamp the village survey. Data on whether subsidy payments were delivered on time came from the household survey, not the village survey, the results of which village leaders were not able to influence. Village accountants who did not stamp the survey said that they didn't have a stamp readily available, an indication of low managerial ability[29].

As predicted, the more households in a village the less likely subsidies were to be delivered on time. This result is also highly significant. An increase of one percentile in village size was associated with approximately a 0.15 percentage point decrease in the probability that a farmer would receive all subsidies on time. Village sizes in the sample varied substantially, ranging from 46 to 800 households, with a mean of 292 and a standard deviation of 169 households.

The simplest explanation for this finding is that larger villages are more difficult to administer and therefore more likely to have problems delivering subsidies on time. However, an alternative explanation is that larger villages might are more accessible to auditors looking for evidence of over-reporting, and thus less likely to over-report, which as discussed above may be a way that villages reduce problems with on-time delivery. The evidence does not support this alternative story. On-time delivery of subsidy payments is possibly negatively correlated with the number of busses per day in the village, closely related to village size, but not significantly correlated with the distance to the county seat, which is more relevant to auditors who have their own transportation.

The data also support the hypothesis that the managerial ability of the farmer is relevant to whether subsidies are received. Those who have their own businesses, and are presumably more familiar with government procedures and more likely to notice and complain if they don't receive subsidies to which they are entitled, are only half as likely to not receive subsidy payments as other farmers. There is no similar correlation between receiving subsidy payments and any other measure of income, consumption, or assets in the dataset, suggesting that the effect of owning a small business on receiving subsidies is one of initiative and familiarity with government procedures rather than one of economic status. The number of government officials in the dataset is not large enough to determine whether officials themselves are more likely to receive the subsidy payments to which they are entitled compared to the general population.

[29] Although it is possible that some of those who did not stamp the survey did not wish to put anything about the program on the record, it is likely that not having a stamp available was true in most cases. Among those who did have a stamp, the stamp was often difficult to find.

6. Conclusions

Substantial problems exist in the implementation of the Sloping Land Conversion Program. On average, villages reported 72% more area enrolled in the program than was actually the case, and 15% of enrolled farmers received at least a portion of the subsidies to which they were entitled late or not at all. Villages would apply for subsidy quotas on land that was not cropland and therefore not eligible for subsidy payments. The State Forestry Administration would respond by approving only a fraction of proposed enrollments, even after trees had already been planted in some cases, leaving farmers without subsidy payments. Villages that are poor and remote (less able to fund administrative costs of the program without over-reporting and less likely to be carefully audited) would over-report more. Farmers were less likely to receive subsidy payments to which they were entitled if the village leader had lower managerial ability and a larger village to manage.

The results suggest that in a large-scale payments for environmental services program, administrative inefficiency can be high, even in a simple program without finely tuned targeting. In such a program, setting different payment rates depending on locally specific opportunity costs of enrollment and on environmental benefits associated with specific pieces of land may be impractical. Centralized databases and computerization may make administration of payments for environmental services programs in developing countries more efficient in the future, but for now administrative inefficiency may be large enough to make finely tuned targeting not optimal when all administrative costs are considered.

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Table 1. Determinants of whether farmers receive subsidy payments on time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Self employment income (0,1)							10.724	9.849	14.118	15.861
							(1.87)*	(1.84)*	(2.02)**	(2.32)**
Number of households in village (percentile in sample)			-0.149	-0.183						
			(2.59)***	(2.48)**						
Number of buses per day in village (percentile)					-0.141	-0.059				
					(3.00)***	(1.01)				
Whether village survey was stamped (0,1)	19.757	12.762								
	(6.14)***	(3.05)***								
County 1		-23.813		-33.020		-15.217				
		(4.58)***		(5.65)***		(2.35)**				
County 2		-0.932		-5.226		1.719				
		(0.17)		(0.80)		(0.29)				
County 3		0.000		-1.907		0.000				
		(.)		(0.27)		(.)				
County 4		-1.468		3.293		10.758				
		(0.24)		(0.51)		(1.62)				
County 5		0.944		4.367		11.618				
		(0.14)		(0.72)		(1.67)*				
County 6		4.908		0.000		11.286				
		(0.83)		(.)		(1.71)*				
Includes village dummy variables	no	no	no	no	no	no	no	yes	no	yes
Constant	69.231	76.883	87.555	94.511	87.206	80.445	79.661	100.000	80.000	100.000
	(28.36)***	(20.61)***	(26.66)***	(24.03)***	(36.43)***	(13.75)***	(46.50)***	(4.01)***	(37.66)***	(2.84)***
Observations	583	582	552	552	464	464	583	582	369	369
R-squared	0.06	0.11	0.01	0.11	0.02	0.07	0.01	0.27	0.01	0.28

Dependent variable: Percentage chance that a participant will receive all subsidy payments on time
Absolute value of t statistics in parentheses
*significant at 10%; ** significant at 5%; *** significant at 1%
Columns 9 and 10 include only households enrolling more than the median amount of land enrolled in the sample.

Table 2. Area enrolled as reported by village leaders versus households

Mu (1/15 ha) per household

35 Villages reporting

	Household reported	Village reported	Difference between village and household report	Ratio of village to household	Log ratio
Mean	5.14	7.77	2.62	1.72	0.32
Std Dev.	(3.21)	(6.02)	(5.60)	(1.36)	(0.64)
COV	1.60	1.29	0.47	1.26	0.49
t statistic[1]	9.48***	7.63***	2.77***	3.12***	2.92***
Min	1.17	1.07	-3.76	0.61	-0.49
25th pctile	2.77	3.09	-0.33	0.85	-0.16
Median	4.25	6.39	0.21	1.05	0.05
75th pctile	6.73	7.81	2.94	1.99	0.69
Max	15.30	21.93	17.29	5.80	1.76

[1] * indicates 10% significance, ** 5% significance, *** 1% significance for tests of $H_0=0$, except for the ratio, for which $H_0=1$.

Table 3. Determinants of the ratio of enrolled land reported by village leaders to that reported by farmers

Variable	Mean	Coefficient	t statistic	Elasticity	Number of observations
Distance to county seat (km)	21.41	0.0119	1.37	0.254779	35
Number of enterprises	2.27	-0.0364	-2.24**	-0.08263	35
Maize yield (metric tons per mu)	0.267	1.561	1.76*	0.416787	33

*Significant at 10% level; **significant at 5% level.

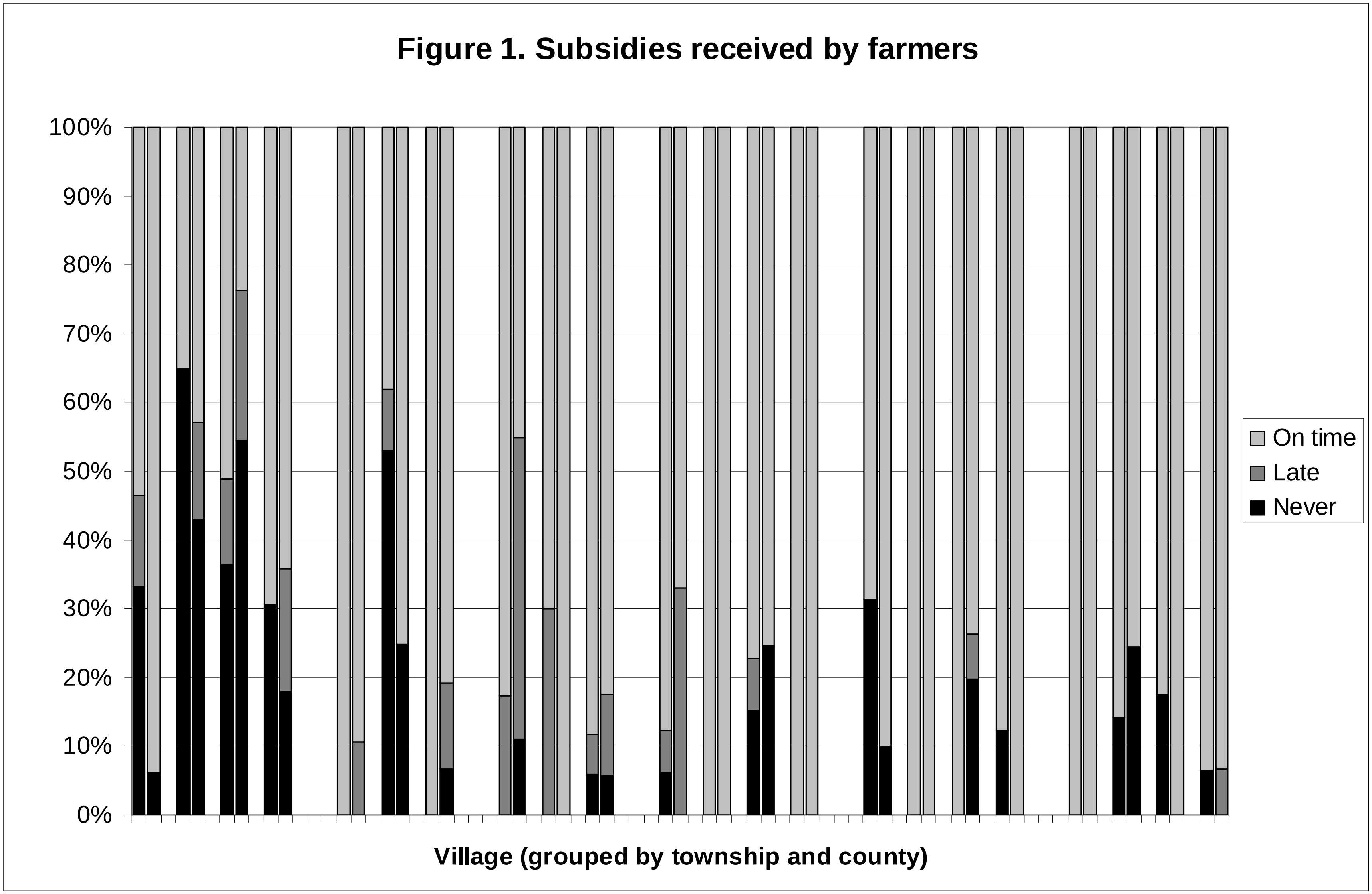


Figure 1. Subsidies received by farmers
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
On time
Late
Never
Village (grouped by township and county)

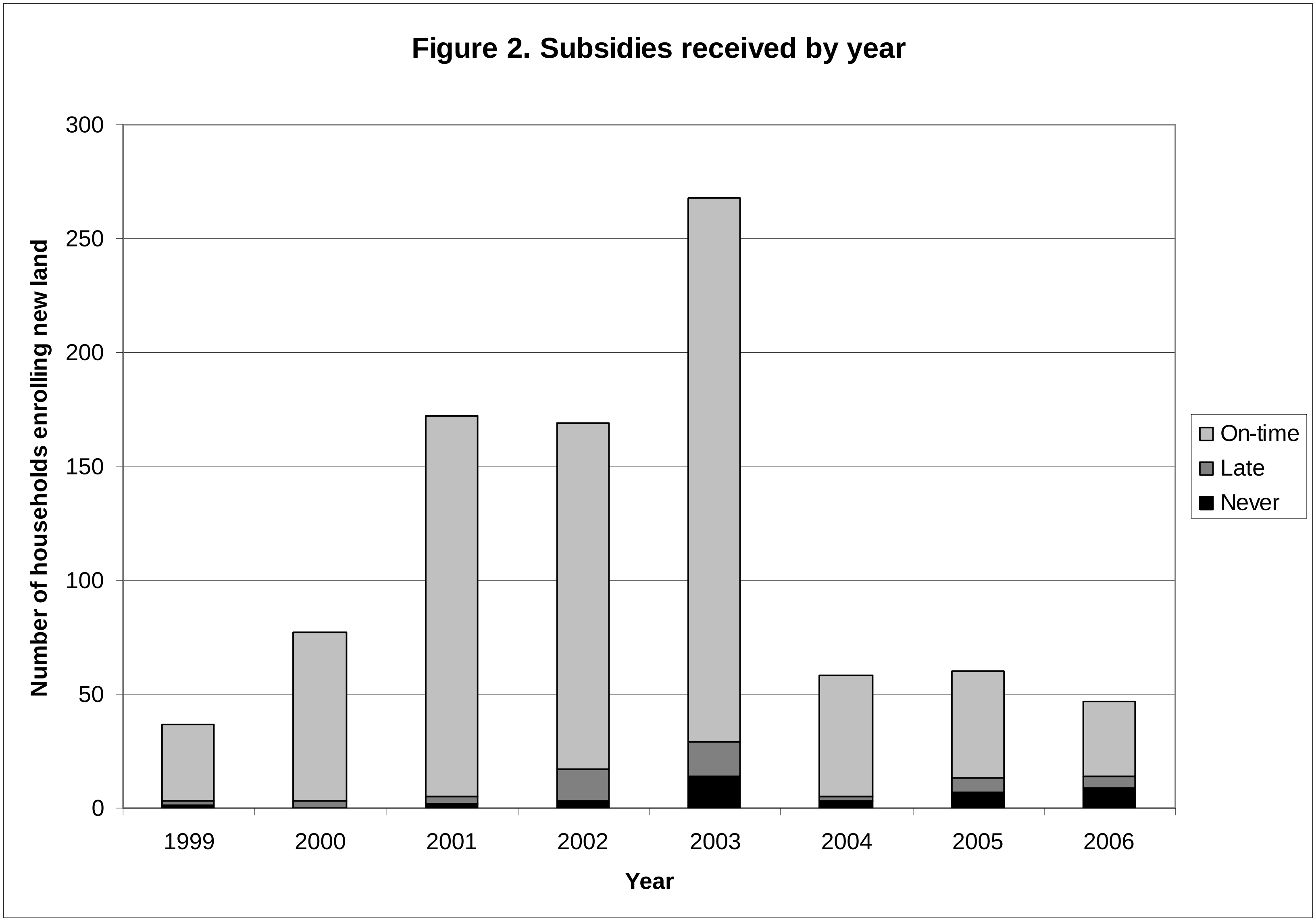


Figure 2. Subsidies received by year
Number of households enrolling new land
300
250
200
150
100
50
0
1999
2000
2001
2002
2003
2004
2005
2006
Year
On-time
Late
Never

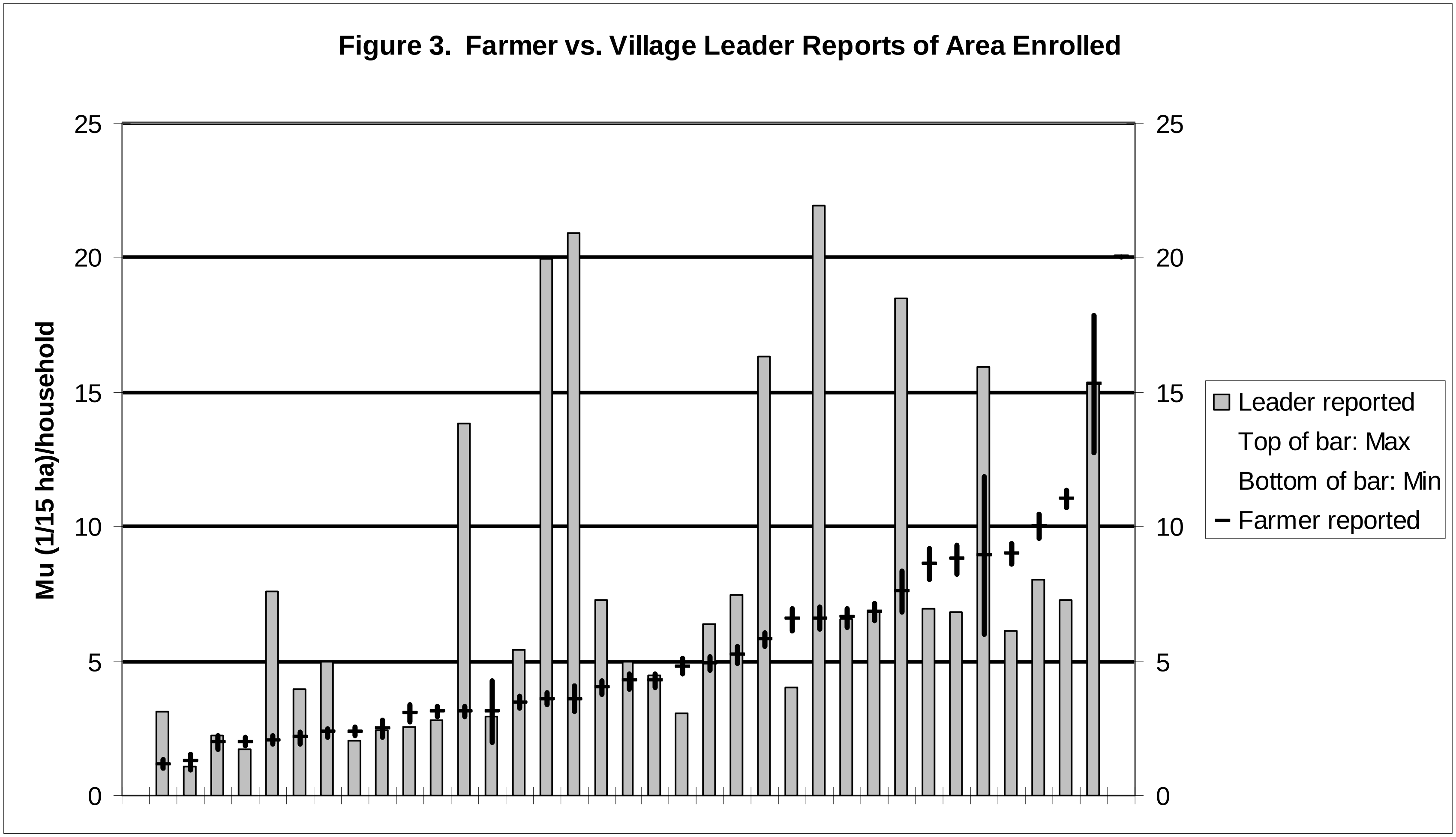
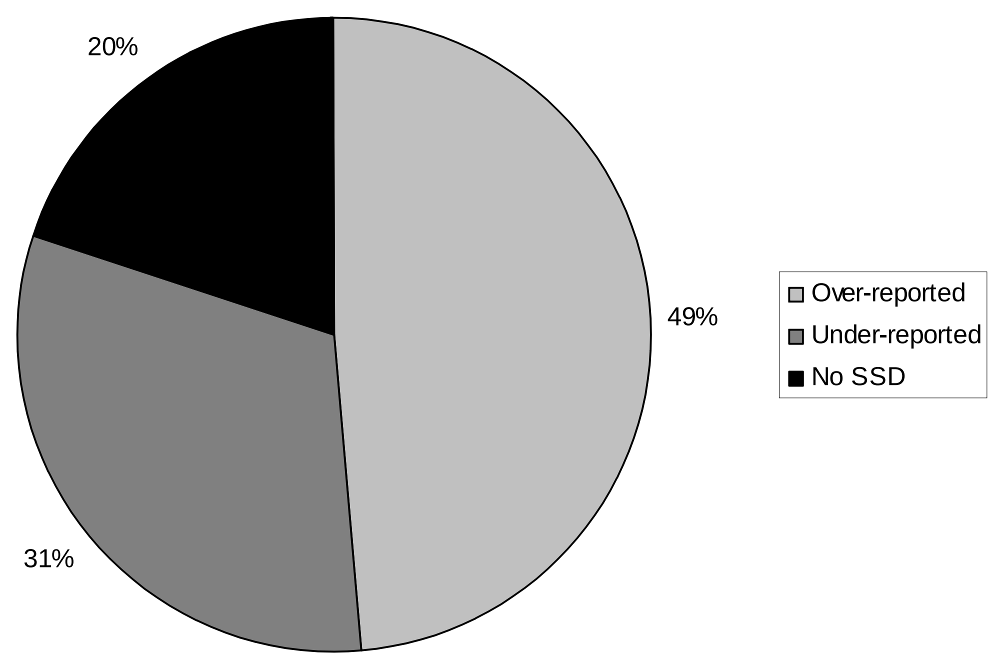


Figure 3. Farmer vs. Village Leader Reports of Area Enrolled
Mu (1/15 ha)/household
25
20
15
10
5
0
Leader reported
Top of bar: Max
Bottom of bar: Min
Farmer reported

Figure 4. Area enrolled as reported by village leader relative to farmer reports



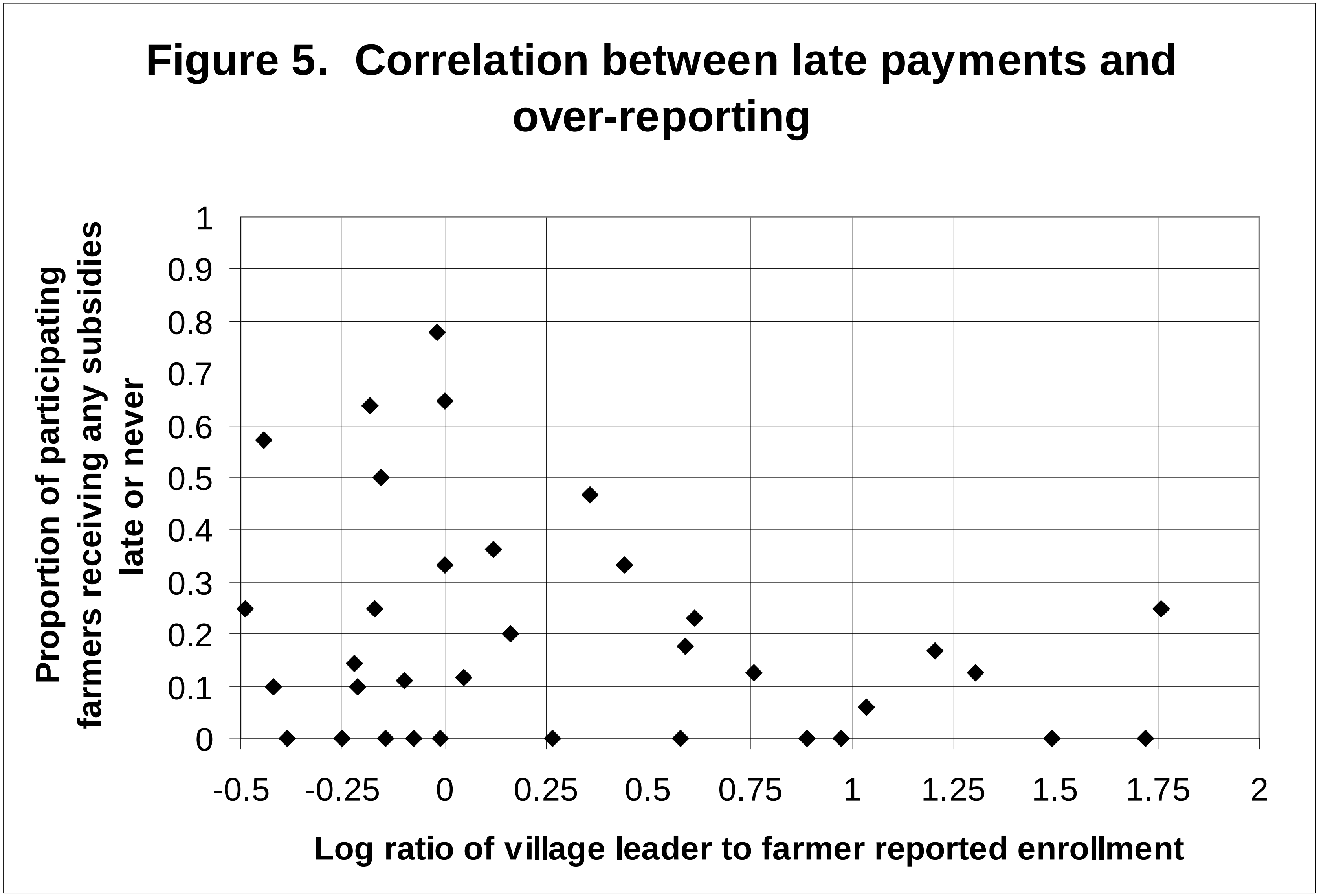


Figure 5. Correlation between late payments and over-reporting
Proportion of participating farmers receiving any subsidies late or never
1
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
-0.5
-0.25
0
0.25
0.5
0.75
1
1.25
1.5
1.75
2
Log ratio of village leader to farmer reported enrollment

Chapter 3. Farmer autonomy and targeting in China's Sloping Land Conversion Program

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Keywords: Payments for environmental services, Public Administration, Sloping Land Conversion Program, China

Abstract: This paper examines the determinants of enrollment at the parcel and household levels in China's Sloping Land Conversion Program (SLCP). The SLCP, which bears similarities to the Conservation Reserve Program in the United States, pays farmers to plant trees on highly erodible cropland, and has effected major land use changes in western China over the past decade. With 15 million households, it represents by far the largest payments for environmental services program worldwide, and implementation varies considerably from one village to another. Although the program is important both environmentally and socio-economically, how land is chosen for enrollment, and even whether it is de facto mandatory or not, is not well understood. This paper uses a household dataset collected by the author's collaborators in Shaanxi Province, which contains detailed parcel and household information on 3397 pieces of land from program inception in 1998 until 2006. On some parcels, farmers made the decision of whether to enroll, while on other parcels the local government decided for them. The paper finds no evidence that farmers place more weight on productivity relative to ecological factors, but instead place more weight on land characteristics relative to other land on the same farm, and also consider education and other household characteristics. Decisions made by local governments, in contrast, are more easily predicted by plot characteristics such as slope and soil quality, and to some extent by a desire to create contiguous forests. Farmers and local governments differ at least as much in their frame of reference, the scale within the landscape to which land under consideration is compared, as in the weights that they place on different criteria of suitability for enrollment.

1. Introduction

Payments for environmental service schemes, such as the Conservation Reserve Program in the United States, Sloping Land Conversion Program in China, and various policies in Latin America, aim to provide market incentives for land conservation. While such programs have the potential to both provide environmental amenities and support farm income, the extent to which they actually supply environmental amenities depends on the degree to which they are targeted towards cost-effective conservation measures in environmentally sensitive areas. An untargeted conservation program could become merely a supply management program, not a conservation program at all.

In principle, targeting could be based on either administratively selecting land, allowing farmers to select land, or a combination, which is the case for the SLCP. This paper compares the criteria used in enrollment decisions made by farmers to those in decisions made by local government officials.

A number of papers discuss targeting in payments for environmental services, both for the Conservation Reserve Program in the United States and for SLCP. Wu et al. (2001) find that a strategy that targets land with the highest benefit-cost ratio provides the largest environmental benefits for a fixed budget if output demand is perfectly elastic. If output demand is not perfectly elastic, output effects must be considered. However, these output effects are likely to be small for the SLCP. Uchida and Rozelle (2006) and Feng et al. (2005), using different data sources and methodologies, both find that because of the low quality of land enrolled in SLCP both production and price impacts are small. Thus an optimally targeted SLCP program, with no administrative costs, would simply target the land with the highest benefit-cost ratio.

Claassen et al. (2008) discusses benefit-cost targeting in the Conservation Reserve Program, which solicits competitive bids for points calculated according to an environmental benefits index. Both bidding and the use of an environmental benefits index can increase cost-effectiveness, but the administrative costs of both approaches would probably be prohibitive for a program such as the SLCP. In SLCP, steeper and less productive land is more likely to be enrolled, but exactly what criteria are most important, and even whether farmers or local government officials are making the decisions, can vary from one location to another and be less transparent than the process used in the United States. There are only payment levels for SLCP, one for the northern half of the country (and sample used in this paper), and another, 50% higher, for the southern half.

A number of studies of both the Conservation Reserve Program and SLCP have attempted to quantify the factors that go into making enrollment decisions. Brimlow (2009) notes that in various studies land quality, land productivity, and landowner characteristics all affect the probability of enrollment in the Conservation Reserve Program, but that the effects are not consistent across studies. In the United States, the effects of different factors can be difficult to identify because payment rates highly correlated with, and determined by, land characteristics. Chang and Boisvert (2009) take a different approach, modeling whole-farm and partial-farm enrollment as separate binary decisions. This approach is probably not applicable to the SLCP because few farmers enroll their entire farm in SLCP (in principle, none are supposed to), and because many decisions are made by local officials based on contiguous areas rather than the scattered parcels of individual households.

Uchida et al. (2005) provides the most detailed discussion of factors that go into making enrollment decisions in SLCP. They find that slope is the most statistically significant predictor of SLCP enrollment, significant at the 1% level, and that yields and distance from the farmer's house are also significant at the 5 or 10% level, depending on the specification. Parcels close to a road may also be more likely to be enrolled, because of the ease of monitoring, but the effect is not statistically significant. (This paper does not consider the distance to the nearest road because of the insignificant effects in other studies and difficulty in defining what constitutes the nearest dirt road.) They find that the quality of targeting varies by region, whether looking at slope or at yields, as measured by the proportions of less-suitable land enrolled and more-suitable land not enrolled[30].

Chen et al. (2009) and Cao et al. (2009) discuss factors that farmers consider in hypothetical decision about whether to convert land back to cropland after subsidies end. Subsidies have been extended beyond the original 5-8 year contracts, and given the political economy of farm subsidies, they may remain in place indefinitely. Hypothetically, subsidies are important to farmers in the program relative to environmental factors, and 23-37% would re-convert their land if they were to end. The probability of re-conversion declines with age and income, and increases with the number of farm laborers in the household and with household land holdings. Distance and slope are unrelated to hypothetical re-conversion decisions.

None of the studies discussed above distinguish decisions that were made by farmers from those that were made by local officials, who could have different objectives. To some extent, the objectives of local officials reflect those of farmers, especially with respect to agriculture. Rozelle and Boisvert (1994) find that village leaders are motivated by personal gain and a desire for independence to pursue industrial development, but that village welfare and maintaining agricultural productivity are also important goals. But local officials are also subject to top-down mandates and informational asymmetries. With 15 million households each enrolling an average of more than one tiny parcel, it is impossible for a local official to consider all of the land, and especially household characteristics that might factor into a farmer's decision.

Institutional context

The SLCP is, in principle, a voluntary program similar to the Conservation Reserve Program. However, in China there is no private ownership of farmland, and executive departments have substantial leeway in writing and implementing regulations, meaning that participation is in practice mandatory for many farmers. (For example, the National Forestry Law and SLCP Ordinance are only approximately 3500 words each, much shorter than the Farm Bill in the United States.) And courts rarely accept cases of

[30] Slope is the most important environmental factor in the region studied in this paper. Slope is less relevant in northern China where desertification is a more important issue than water erosion (Wang et al. 2007), but the study region has sufficient precipitation that water erosion is more relevant than desertification.

farmers attempting to sue a local government. Although farmers have often limited autonomy in determining whether to participate in SLCP, most participants in the sample say that they are better off as a result of participating. Some farmers say they are worse off, but others say they would like to enroll even larger areas than they have.

During China's rural de-collectivization of the early 1980's, households received usufructory rights to farmland for terms of several decades, which have been routinely extended. Most of these land rights were granted in exchange for fulfilling state grain procurement quotas, on so-called responsibility land. Now, grain procurement quotas have been phased out, and farmers are essentially renting land from the village under long-term contracts at a rental rate of zero. Farmers generally make agricultural production decisions as though they own the land (Jacoby et al. 2002), but do not have the authority to change the land use, and often receive only the agricultural value of land as compensation when their land is appropriated for public uses or urban development.

The SLCP in many ways resembles a mandatory program controlled by forestry and township officials, and is in many respects unpredictable from the perspective of farmers or even lower-level officials. Farmers in China do not have land ownership rights, and until very recently, farming was taxed rather than subsidized. The SLCP is arguably not only China's first ostensibly voluntary farm subsidy program, but also its first large-scale farm subsidy program of any kind. As in many other Chinese government programs, executive-branch implementation is crucial. The program is administered by many levels of bureaucracy down to the township government and village levels, and its authorizing regulations are written in general terms that afford minimal legislative or judicial authority.

Steps in program implementation

In consultation with village leaders, and sometimes with farmers, individual township officials write an annual reforestation plan, which they submit to the county office of the SFA. From there, the plan makes its way to the provincial forestry administration, to the SFA, and finally to the State Council, China's cabinet, for final approval. Each level tends to approve only a portion of the land area proposed by the level below it, as in a typical budgeting process. After the plan receives final approval, enrollment quotas are allocated back down the administrative hierarchy. Administrative costs have been very high; many townships spent a majority of their staff time administering the program in its early years. Each year, especially in the early years of the program, each level of government has had little or no idea of whether it will receive an enrollment quota. Because of declining grain surpluses, rising grain prices, and changes in communist party leadership, many local officials have complained that they could not predict even whether the program would be continued on a large scale at a national level.

Farmers in villages eligible for the program attend required village meetings in which village officials explain the program and how it is implemented in their area. At the meetings, the farmers are told which pieces of land must be enrolled, which may not be enrolled, and which they can choose whether or not to enroll. The path of least resistance for the farmer is to follow the local government's plan to enroll certain areas and not others. Those farmers who enroll sign a contract with the SFA or another designated local government unit, and agree to plant trees on land that has been rented from or allocated by the village. Appendix 2 contains an English translation of one version of the contract, from northern China. In southern China, subsidy payment rates are exactly 50% higher per hectare, but there are no other substantive differences in the contract. The details of the implementation vary with the type of trees to be planted, and the program has gone from an in-kind grain subsidy to a cash subsidy[31]. The contract states that land is to remain enrolled indefinitely even though subsidies are for only 5-8 years (not including finite extensions)[32]. In the sampled villages at the time of the survey, there existed no procedure for un-enrolling a plot once it had been enrolled. The program was designed with the hope that farmers would voluntarily substitute non-farm employment and/or high-value crops as income sources to replace their lost pre-enrollment grain production income.

2. Research questions and methods

This paper compares the factors that the local government uses to determine which areas to enroll with those that farmers use to determine which areas to enroll when they have autonomy. Specific research questions are as follows:

1. Compared to local governments, do farmers place more weight on crop yields relative to ecological factors in determining which land is to be enrolled?

2. Do farmers make enrollment decisions based on the opportunity costs of cultivating the land in the form of outside employment opportunities? Do local governments also consider family-specific opportunity costs?

3. Do farmers, wishing to enroll some but not all of their land to mitigate risk, consider land characteristics relative to other land on the farm? Do local governments

[31] Upon enrollment, in the appropriate season, the SFA provides ecological-tree seedlings free of charge, or shares the cost of orchard seedlings with the farmer. In either case, the village trains farmers in planting the seedlings. In most villages, planting the trees is a required community undertaking, whereas taking care of the trees is the responsibility of the individual farmer. If the plots pass a series of inspections, the farmer receives an annual subsidy payment to compensate for the opportunity costs of retiring his grain-producing land. In the early years of the program, this payment was in kind in the form of grain; it later changed to a cash payment, but because grain markets are well developed farmers did not consider this a substantive change. Almost all plots enrolled eventually pass inspection; when tree survival rates are low, farmers are generally given new seedlings and their plots are declared passing as long as they make a good-faith effort to reforest the area by planting the new seedlings.

[32] Payments last for 5-8 years, after which the farmer must either continue to keep the land enrolled for the remainder of his land-use contract (which is extended by the reforestation contract to 50 years), or pay an unspecified fine. (No farmer in the sample knew how much the fine would be for violating the land retirement contract, and only one had actually cut his trees and paid such a fine. The vast majority of farmers saw the contract as binding.) Although subsidies have been extended, both unofficially through local procedures to spread payments over time and now officially nationwide, they are still in principle for a finite period of time.

also attempt to allocate enrollment evenly across households, even to the extent that this conflicts with enrolling the most suitable land in the entire village?

To answer these questions, the author's collaborators from the Northwest Sci-Tech University of Agriculture and Forestry conducted a survey of 682 farmers in Shaanxi Province, representing 3397 parcels of land. Sampling was stratified according to the total land enrolled in SLCP for selecting villages, and random within villages. Most respondents had enrolled some but not all of their farms in the SLCP. The questionnaire asked farmers about the alternatives they were offered, as well as the choices they made and the characteristics of their land and household. Farmers were asked to classify land into three main categories, based on the way in which local officials were actually implementing the SLCP (not simply national regulations, under which all enrollment is voluntary): Land that was required to be enrolled in the SLCP, land that was not allowed to be enrolled in the SLCP, and land where the farmer had a choice of whether to enroll in the SLCP. Most farmers reported that they farmed some plots where they could choose whether to enroll and others where they had no choice about whether to enroll[33].

Farmers and local governments use comparable criteria in choosing which plots to enroll, most importantly slope and yield. Enrolling steep plots is both within the spirit of the SFA's goals of controlling erosion and desirable to the farmer in that steep plots are difficult to cultivate with machinery; very steep plots are difficult to cultivate even with animals or humans. Enrolling low-yielding plots is desirable from the farmer's perspective, and also helps local officials to meet grain production targets. Generally, the larger the proportion of steep land that a village or other region contains, the steeper a plot of land in that region must be to be considered for enrollment. However, even where the program is mandatory from the perspective of the farmer, many villages have steep land that is not enrolled and flat land that is enrolled, undermining the environmental benefits of the program.

The paper uses a linear probability model to estimate the weight that farmers and local governments place on different factors in determining which pieces of land are enrolled. The analysis includes only land for which there is a meaningful choice as to whether it is to be enrolled. No paddy or former paddy in the sample is enrolled in the program, because of high productivity and flat slope, and for practical purposes they are not eligible.

In general, the paper finds that although farmers and local governments use the same land characteristics to determine whether to enroll land, farmers place less weight on all land characteristics. Farmers also consider their own opportunity costs of working the land, and the characteristics of particular pieces of land relative to others that the farmer operates.

[33] Farmers said that they had a choice on about half of the total plots in the sample. On approximately ¼ of the plots, they reported that they were required to enroll, and the final ¼ were not eligible for enrollment. Farmers in the sample enrolled about 40% of their land, including 1/3 of the land on which they had a choice of whether to enroll. In most of the villages surveyed, farmers believed that they were required to participate, but that they could not enroll their entire farm.

3. Descriptive Results

Demographics

The demographics of the sample are roughly representative of rural China (see Table 1). The average household size in 1998 was approximately 4 individuals, often including older relatives. Anyone who lived in the household for at least 3 months during the year, or was an unmarried son or daughter of a household member whether or not the child lived in the village, was counted as part of the household. Land holdings were relatively small, averaging less than 1 hectare (15 *mu*) per household. However, only one household in the dataset had no land (all of its land was appropriated in 1999). On average, the head of the household had completed primary school, though 87 had no education and 4 had postsecondary education[34]. Among household members of all ages, 44% were identified as working on-farm in 1998, compared to 12% working off-farm. In many households, no one was identified as working on-farm because no one was doing farm work for more than a relatively small fraction of the year.

The average size of farmers' houses increased approximately 25% from 1998 to 2006, paralleling the growth of GDP in rural China[35]. Livestock holdings, in contrast, were little changed during the period at approximately one animal unit per household in both 1998 and 2006[36].

Land characteristics

Tables 2 and 3 describe the land allocated to farmers in the dataset, whether or not the land is part of the program. Plots tend to be small (2/15 of a hectare) and are on average nearly 1 km from the farmer's house. Many plots are much steeper than would typically be cultivated in the United States, and most are on the side of a hill, with some in flat areas. (All else being equal, a plot near the bottom of a hill will have a higher erosion rate than one near the top of a hill.) Soil quality is relatively low, as reported by the farmer, using an ordinal metric familiar to farmers in the sample. Although there are some paddies in the sampled region, most of the land in the sample, especially larger pieces of land and those that are enrolled, is not irrigated at all. Insolation is a function of the slope's exposure (with southern exposures having higher insolation than northern exposures) and steepness (see Appendix).

Most land in the dataset is responsibility land, in which the village collective rents land to farmer under a rent-free lease of 30-50 years, in exchange for fulfilling a state grain procurement quota (a requirement that has been phased out). Contract land is rented for cash, while farmers are allowed to use cleared wasteland in exchange for

[34] About 1% declined to state their education levels.
[35] In fewer than 1% of households was neither the household head nor the enumerator able to estimate the area of the house, largely for households that lived in caves.
[36] One animal unit is equivalent to one female beef cow, and animal unit coefficients range from 0.0025 for broilers to 1.9 for dairy bulls (Delaware Dept. of Agric. 2000). Households reported poultry only when the total number was at least 20. Most animal units in the dataset consist of swine, or of beef or work cattle. Because the survey asked about many different types of livestock and because the numbers of major livestock are usually zero or one, recall bias is likely to be small.

having made it suitable for agriculture. About 1% of the land in the dataset has other land tenure arrangements, mostly subleases among farmers in the same village.

A wide variety of trees are planted on enrolled land as part of the program, more than one species in every sample village and sometimes more than one species on the same plot of land. In principle, no more than 30% of the enrolled area in each county is to be planted to orchard trees, the rest to forest trees. In practice, however, most of the area is planted to trees that have potential economic value, but that are suited to inferior land and might also be considered forest trees. For example, chestnut and walnut can be grown for either nuts or timber.

Before the program began, the most common cropping patterns for local farmland were maize in the summer with wheat in the winter and rice in the summer with rapeseed in the winter. Enrolled land had typically been planted to maize-wheat prior to enrollment[37]. Relative to current yields on plots that were not enrolled, yields of both maize and wheat prior to enrollment were substantially lower. For both winter and summer crops, yields were usually at least average in the year immediately prior to enrollment.

Targeting

SLCP does not perfect target the steepest land for enrollment. As shown in Table 4, steep land is not necessarily enrolled in the program and flat land is sometimes enrolled. Among 3394 plots of land, 454 are flat or gently sloping but enrolled, while 431 are steep or very steep and not enrolled. Within particular villages, a substantial fraction of enrolled land is not as steep as some land that was enrolled, and a substantial fraction of non-enrolled land is steeper than other land in the village that was enrolled. (The boxed cells in the table indicate plots on which the enrollment decision would have been different had slope been the only criterion for enrollment.) Within individual households, there is much less overlap than within villages, but there are some households that did not enroll a piece of land that was steeper than one that they did enroll.

Factors considered

To better understand the reasons for poor targeting, the survey team asked both village leaders and farmers for perspectives on the factors by which enrollment decisions were made. Village leaders were asked to rank the importance of six factors in determining what land was to be enrolled, with 1 being most important (see Table 5). The main factors they cited were slope and creating a contiguous parcel with other enrolled land. Low yields and distance were secondary factors. Among the minority of village leaders who cited allocating quotas to poor families in the village or allocating quotas to all families as factors, these factors were not considered as important as the land characteristics. Some village leaders said that they used high erosion rates or a subjective assessment of the suitability for cultivation, but did not use any factors other than slope in

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[37] The number of observations for crops is less than the total number of plots because plots that were in the fallow part of a rotation cycle as of the reporting time were coded as missing.

predicting erosion and used slope and yields as proxies for the suitability for cultivation. Thus the table includes all underlying factors that village leaders considered, even for villages where the criteria were called by other names.

Farmers were asked an open-ended question about why certain plots were selected for enrollment and others were not selected, and their responses are tabulated in Table 6. Among those who knew (at least 20% did not know because the program was often mandatory), the factors farmers most often cited as the most important were steep slopes, low yields, creating a contiguous area, and distance. Contiguous areas were created by local policy and by lack of tractor access to land surrounded by enrolled land. Some farmers gave vague answers, such as according to the policy, according to the example of others, or according to what land is not suited for growing grain. Only 2 out of 499 respondents said that good land had been selected for the program in order to plant orchards on it; the rest of the responses are all consistent with enrolling inferior land.

Both farmers and local officials play a role in determining which plots were to be enrolled. Of all the plots in the sample, 1697 (52%) were mandatory in the sense that the farmer was either required to enroll or not allowed to enroll (see Table 7). On 45% of plots, the farmer reported having a choice regarding whether to enroll or not, and on 3% of plots the farmer agreed with the government's plan of which land to enroll and never bothered to inquire about whether it was possible to deviate from the plan.

Where farmers had a choice about whether to enroll, the default option or path of least resistance was for some land to enroll and for other land to not enroll. Most plots of land followed the default option set by the local government, which could reflect either the power of suggestion or farmers and local governments using similar criteria in their decision-making. Among plots where the farmer had a choice but the default was to enroll, 26% chose not to enroll. Among plots where the farmer had a choice but the default was to not enroll, only 8% enrolled. A total of 219 plots (6.8%) of all plots go against the default option provided by the government, including a handful of special situations[38].

4. Analytical results

Land characteristics and enrollment

The most important factors in determining whether land is enrolled in SLCP are slope and soil quality (see Table 8])[39]. Slope, soil quality, wheat yields (current for non-enrolled plots or prior to the program in the case of plots that have been enrolled), and distance from the farmer's house are all correlated with enrollment at the 1% level in unconditional linear probability regressions. The probability of a plot being enrolled

[38] Seventy-four plots were required to be enrolled but not actually enrolled because the government made a plan but did not receive the budget necessary to implement the plan. On 8 pieces of land, a farmer planted orchard trees since the program began without enrolling in the program. All plots where a farmer continued to grow grain between the rows of trees while waiting for the trees to mature are counted as enrolled in the table.

[39] Table 1 does not include village dummy variables because the aim is to present a correlation between enrollment and the characteristics of land, not between enrollment and the characteristics of land relative to other land in the village, which is discussed further later in the paper.

rises from 15% for flat land to 88% for very steep land. Among plots with a history of growing wheat, an increase of one metric ton per hectare in wheat yields (compared to a mean of 12 tons per hectare) is associated with a 3 percentage point decrease in the probability of enrollment. An extra kilometer of distance from the farmer's house (about a doubling of the average distance) is associated with a 10 percentage point increase in the probability of enrollment. The probability of enrollment ranges from 15% on good soil to 58% on poor soil.

The effects of crop yields, distance, and soil quality are much less in a conditional than in an unconditional regression, but remain mostly statistically significant, even when accounting for clustering at the village or household levels. Higher insolation is associated with a lower probability of enrollment (i.e., for a given slope, a northern exposure is more likely to be enrolled than a southern exposure), but the effect is statistically significant at the 5% level in only one specification.

Household demographics and enrollment

Some household characteristics are also correlated with whether land is enrolled in SLCP (see Tables 9 and 10). Because geography is the primary determinant of whether land is enrolled, all household specifications include village dummy variables[40].

There is a correlation between larger land holdings and a higher probability of enrollment at the household level, but it likely does not represent a causal relationship. This correlation likely results from the way in which land was distributed during de-collectivization—to promote equity in distribution, households that received more land received lower-quality land. Controlling for land characteristics reduces the estimated coefficient on the size of household land holdings, and fully controlling for all unobserved measures of land quality would likely further reduce its magnitude. The coefficient on land area per household member, a variable that might be more likely to factor into household decision-making regarding continuing to produce grain than total land holdings, becomes insignificant when controlling for observed land quality.

More household members working off-farm and fewer working on-farm before the program began are associated with higher probabilities of plot enrollment, but the magnitudes are relatively small, 1 or 2 percentage points per household member. The size of the effect in part reflects the fact that local government officials, not farmers, are making many of the decisions regarding enrollment. Land operated by more educated farmers is more likely to be enrolled, but the coefficient is only significant at the 10% level in the pooled sample.

In contrast to the findings of studies of the Conservation Reserve Program, there is no evidence that the age of the household head predicts enrollment (Sullivan et al. 2004). Nor is there any evidence that household wealth (using the size of the house or the size adjusted for the number of household members before the program began as a proxy for wealth) is related to the probability of enrollment. Households with more people or more livestock (either of which could be related to household grain consumption) are not more or less likely to enroll by a statistically significant margin. Because the sample size

[40] Not including village dummy variables produces spurious correlations. For example, remote areas tend to have both more land suitable for enrollment and more traditional family structures.

includes more than 3000 parcels, even a relatively small effect of one of these variables would be discernible, as can be seen from the statistical significance of the small effect of the number of household members working off-farm.

Farmer versus government decisions

Farmers and local government officials use similar land criteria in determining which parcels to enroll, but have different priorities, as shown in tables 11-14. All regressions include plot-level enrollment (0/100) as the dependent variable. For all four criteria, wheat yields, distance from the farmer's house, slope, and soil quality, land of lower agricultural value is more likely to be enrolled in SLCP by highly significant margins. This is true both where local officials are making the enrollment decisions and among parcels in which farmers are allowed to decide whether to enroll.

In the simple regressions with no control variables (columns 5 and 10 in each table), the magnitude and significance of the estimated coefficients is in all cases higher among parcels where the local government is deciding, as are the R^2 values of the regressions. In other words, land characteristics are better predictors of enrollment when local governments are making the decisions than when farmers are making the decisions. This is true for land characteristics that relate to the opportunity costs of enrollment (wheat yields and soil quality) as well as to slope, a proxy for the environmental benefits of enrollment. In contrast to what one might expect, there is no evidence in tables 4-7 that would suggest that farmers place more weight on the opportunity costs of enrollment and less on environmental benefits in choosing which parcels to enroll. Tables 4-7 instead suggest that farmers are considering factors other than land characteristics in making enrollment decisions.

One of the factors that farmers appear to be putting more weight on than local governments is land characteristics relative to other land farmed by the same household. In each of the four tables, the coefficient on the household mean is significant and of the opposite sign to the coefficient on the variable itself. The most likely plots to be enrolled are those that are suitable for enrollment and assigned to households whose other land is unsuitable. For example, controlling for the slope of the plot in question, the steeper the household's land on average the less likely the plot is to be enrolled. For each of the four characteristics in the four tables, the magnitude of the coefficient on the household mean of the variable is larger where the farmer is making the decisions than where the local government is making the decisions. Local governments are also more likely to enroll land where other land managed by the same household is less suitable for enrollment—they are implementing the spirit of the program, under which no one is supposed to enroll all of their land and subsidies are supposed to be widely distributed so that the program alleviates poverty as well as helping the environment. But farmers appear to consider land characteristics relative to other land they manage even more strongly, perhaps in an effort to diversify income sources in light of uncertainty regarding the future of the program.

In contrast to the findings for land characteristics relative to other household land, the more suitable other land is in a village the more likely a particular plot will be enrolled. In many villages, there are large contiguous enrolled areas, such as entire mountainsides. Although national regulations say nothing about such contiguous areas, local governments often create them in order to facilitate the administration of the program and because enrolled areas can block access to contiguous land for purposes of cultivation. For each of the variables except for wheat yields, the magnitude of coefficients on average village land characteristics are larger for land on which the local government decided whether to enroll than they are for land in which the farmer decided whether to enroll. This finding is consistent whether using the average for the entire village, or the average for other land in the same compass direction within the village (using 8 compass directions from the village committee headquarters). For distance, local governments appear most likely to enroll a plot when village land in general is relatively close-in but where land in the same compass direction as the plot in question is relatively far-away. Farmers appear not to consider distance relative to other land in village, as opposed to just the land they manage themselves.

There is no evidence in the data that local government officials consider household characteristics when making enrollment decisions (see Table 15). Among parcels where farmers did not have choice regarding whether to enroll, none of the coefficients on household characteristics are statistically significant, and for the proportion working off farm the sign is sensitive to whether control variables are included.

Farmers, in contrast, do appear to make enrollment decisions according to their household situation. On plots where farmers are able to decide whether or not to enroll, land managed by households that before the program had fewer members working on the farm, more members working off the farm, and a more educated head of household was more likely to be enrolled. Although the coefficient on the proportion working on farm is not statistically significant, it is of the expected sign (those with more household members engaged in farming are less likely to enroll) and is potentially larger than the reported magnitude. The prevalence of multitasking on small farms introduces measurement error in the number of household members working on farm, which through attenuation bias would tend to lower the estimated coefficient. The most significant difference between the coefficients for plots with and without choice is in the effect of education. When farmers have a choice about whether to enroll, the probability of enrollment goes up by approximately one percentage point for every year of education of the household head, compared to no effect of education when the decision is made for the farmer. In other words, farmers with more education are more likely to choose to shift away from growing grain.

5. Conclusions

The mixture of government and farmer decision-making in SLCP shows how different stakeholders will make different choices in payments for environmental services Because of the difficulty of cultivating steep land, farmers prefer to enroll it even if it is equally productive to flatter land, which tends to promote ecological targeting even without a price differential in payment levels. Farmers and local governments differ at least as much in their frame of reference, the scale within the landscape to which land under consideration is compared, as in the weights that they place on different criteria of suitability for enrollment.

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7. Appendix

Relative insolation refers to the intensity of sunlight striking the plot relative to that striking a flat surface at the same latitude, which is normalized to 1. Because of the importance of morning sunshine to crops, hill exposure angles are calculated relative to a south-southeast exposure, not to a due south exposure. Relative insolation is lower when weighting by plot size because land with southern exposures is likely to be divided into smaller pieces than land with northern exposures in the study area.

The following assumptions were used to calculate relative insolation on each plot of land. The simplifying assumptions have little effect on the estimated relative levels of insolation between one plot and another.

--Flat, sloped, steep, and very steep correspond to 0, 15, 25, and 35 degree slopes respectively.

--All land in the sample is located at 34 degrees north latitude. In fact, latitudes vary between 33 and 35 degrees, with exposure slightly more important at higher latitude.

--Land is not located in the shadow of adjacent land with an extremely steep slope.

Table 1. Household characteristics

Variable	Obs	Mean	Std. Dev.	Min	Max
Animal units 1998	680	1.11	1.58	0	13.75
Animal units 2006	680	1.14	1.43	0	9.25
House area 1998 (m2)	675	104.07	71.27	8	800
House area 2006 (m2)	675	130.47	95.94	8	800
Household size 1998	681	4.21	1.35	1	10
Land area (*mu*)	680	11.17	11.31	0	175
Land area per household member (*mu*)	680	2.83	3.24	0	58.33
Education of household head (years)	673	6.34	3.58	0	15
Age of household head (years)	678	49.78	11.06	28	81
Household members working on-farm (1998 proportion)	681	0.44	0.27	0	1
Household members working off-farm (1998 proportion)	681	0.12	0.18	0	1

Table 2. Land characteristics weighted by number of plots

Variable	Obs	Mean	Std. Dev.	Min	Max
Area (*mu*)	3397	2.23	3.43	0.05	110
Distance from home (m)	3397	797.88	840.49	0	10000
Relative insolation	3332	1.01	0.16	0.55	1.4
Slope					
Flat	3342	0.36	0.48	0	1
Sloped	3342	0.25	0.44	0	1
Steep	3342	0.35	0.48	0	1
Very steep	3342	0.013	0.12	0	1
Terraced	3342	0.023	0.15	0	1
Hill position					
At or near top	3344	0.087	0.28	0	1
Side	3344	0.54	0.5	0	1
At or near bottom	3344	0.072	0.26	0	1
Flat area	3344	0.3	0.46	0	1
Soil quality					
Good	3394	0.17	0.37	0	1
Medium	3394	0.41	0.49	0	1
Poor	3394	0.42	0.49	0	1
Irrigation status					
Paddy	3397	0.039	0.19	0	1
Irrigated	3397	0.095	0.29	0	1
Non-irrigated	3397	0.87	0.34	0	1
Major trees planted on enrolled land					
Acacia	1264	0.17	0.38	0	1
Chestnut	1264	0.07	0.25	0	1
Prickly pear pepper	1264	0.17	0.37	0	1
Walnut	1264	0.13	0.33	0	1
Tea	1264	0.058	0.23	0	1
Major crops on non-enrolled land					
Wheat (winter)	1679	0.66	0.48	0	1
Rapeseed (winter)	1679	0.15	0.36	0	1
Maize (summer)	1427	0.73	0.44	0	1
Rice (summer)	1427	0.083	0.28	0	1
Major crops prior to enrollment					
Wheat (winter)	1057	0.74	0.44	0	1
Rapeseed (winter)	1057	0.067	0.25	0	1
Maize (summer)	857	0.79	0.41	0	1
Rice (summer)	857	0.0011	0.034	0	1
Yields of major crops					
Current maize yield (*jin*/*mu*)	1063	452.85	220.72	0	1000
Current wheat yield (*jin*/*mu*)	1109	447.74	158.38	0	1100
Pre-enrollment maize yield (*jin*/*mu*)	720	408.49	182.61	0	1000
Pre-enrollment wheat yield (*jin*/*mu*)	806	336.02	158.07	40	1000
Relative yields in year prior to enrollment					
Previous winter crop below average yield	1004	0.17	0.38	0	1
Previous winter crop average yield	1004	0.77	0.42	0	1

Previous winter crop above average yield	1004	0.058	0.23	0	1
Previous summer crop below average yield	880	0.15	0.36	0	1
Previous summer crop average yield	880	0.8	0.4	0	1
Previous summer crop above average yield	880	0.05	0.22	0	1
Land tenure arrangement					
Private vegetable plot	3391	0.013	0.11	0	1
Responsibility land	3391	0.91	0.28	0	1
Contract land	3391	0.051	0.22	0	1
Cleared wasteland	3391	0.013	0.12	0	1

Table 3. Land characteristics weighted by size of plots

Variable	Obs	Mean	Std. Dev.	Min	Max
Area (*mu*)	3397	7.49	15.13	0.05	110
Distance from home (m)	3397	957.5	1156.1	0	10000
Relative insolation	3332	0.96	0.21	0.55	1.4
Slope					
Flat	3342	0.22	0.41	0	1
Sloped	3342	0.24	0.43	0	1
Steep	3342	0.42	0.49	0	1
Very steep	3342	0.11	0.31	0	1
Terraced	3342	0.011	0.11	0	1
Hill position					
At or near top	3344	0.19	0.39	0	1
Side	3344	0.58	0.49	0	1
At or near bottom	3344	0.046	0.21	0	1
Flat area	3344	0.19	0.39	0	1
Soil quality					
Good	3394	0.12	0.33	0	1
Medium	3394	0.39	0.49	0	1
Poor	3394	0.49	0.5	0	1
Irrigation status					
Paddy	3397	0.015	0.12	0	1
Irrigated	3397	0.066	25	0	1
Non-irrigated	3397	0.92	0.27	0	1
Major trees planted on enrolled land					
Acacia	1264	0.28	0.45	0	1
Chestnut	1264	0.078	0.27	0	1
Prickly pear pepper	1264	0.069	0.25	0	1
Walnut	1264	0.086	0.28	0	1
Tea	1264	0.055	0.23	0	1
Major winter crops on non-enrolled land					
Wheat (winter)	1679	0.65	0.48	0	1
Rapeseed (winter)	1679	0.12	0.33	0	1
Maize (summer)	1427	0.72	0.45	0	1
Rice (summer)	1427	0.047	0.21	0	1
Major winter crops prior to enrollment					
Wheat (winter)	1057	0.77	0.42	0	1
Rapeseed (winter)	1057	0.06	0.24	0	1
Maize (summer)	857	0.79	0.41	0	1
Rice (summer)	857	0.0014	0.038	0	1
Yields of major crops					
Current maize yield (*jin*/*mu*)	1063	492.88	223.34	0	1000
Current wheat yield (*jin*/*mu*)	1109	437.55	153.42	0	1100
Pre-enrollment maize yield (*jin*/*mu*)	720	404.38	170.88	0	1000
Pre-enrollment wheat yield (*jin*/*mu*)	806	304.68	156.72	40	1000
Relative yields in year prior to enrollment					
Previous winter crop below average	1004	0.2	0.4	0	1

yield					
Previous winter crop average yield	1004	0.74	0.44	0	1
Previous winter crop above average yield	1004	0.052	0.22	0	1
Previous summer crop below average yield	880	0.15	0.35	0	1
Previous summer crop average yield	880	0.81	0.39	0	1
Previous summer crop above average yield	880	0.042	0.2	0	1
Land tenure arrangement					
Private vegetable plot	3391	0.0074	0.086	0	1
Responsibility land	3391	0.87	0.34	0	1
Contract land	3391	0.085	0.28	0	1
Cleared wasteland	3391	0.011	0.1	0	1

Table 4. Overlap in slope between enrolled and non-enrolled plots

	No. Enrolled	No. Not enrolled	Percent enrolled
Flat	163	964	14.46
Gentle slope	291	548	34.68
Steep slope	741	425	63.55
Very steep slope	39	6	86.67
Terraced	56	22	71.79
Total	1290	1965	39.63

Enrolled land	By household	By village
Flatter than steepest land not enrolled	52	389
Not flatter than steepest land not enrolled	1182	845
Non-enrolled land		
Steeper than the flattest land enrolled	61	718
Not steeper than the flattest land enrolled	1941	1284

Table 5. Factors considered by village leaders in choosing land for enrollment

n=44

Factor	Number cited	Average rank	Std Dev Rank	Min Rank	Max Rank
Slope	37	1.61	1.05	1	6
Contiguous area	36	2.32	1.24	1	6
Yield	27	2.59	0.89	1	4
Distance	23	3.26	1.13	1	5
Poverty	16	4.25	1.65	1	6
Some for all	14	4.64	1.74	1	6

Table 6. Responses to an open-ended question of farmers as to how land was chosen for enrollment

Steep slope, ecological factors, or landslides	152
Don't know	107
Yields or fertility	106
Contiguous area (policy or access)	42
Distance	39
Policy and others setting an example	28
All retired or all but basic grain production land retired	13
Trees planted along roads or rivers	4
Not suited to growing grain	3
Land suited for orchards retired	2
Drought-sensitive	2
Little sunlight	1

Table 7. Choice status and land enrollment

	Enrolled	Not Enrolled	Total
Must enroll	767	74	841
May not enroll	8	848	856
Choice	521	942	1463
******Designated	419	147	566
******Not designated	67	766	833
******No designations	13	13	26
Satisfied, unsure if have choice	7	76	83

Table 8. Effects of land characteristics on probability of enrollment [%]

	[1]	[2]	[3]	[4]	[5]	[6]	[7] village clusters	[8] household clusters
Sloped [0/1], Flat omitted	20.03					15.59	15.59	15.59
	[9.82]***					[6.12]***	[3.64]***	[3.94]***
Steep [0/1]	48.40					41.47	41.47	41.47
	[25.96]***					[15.12]***	[6.78]***	[10.16]***
Very steep [0/1]	73.48					51.87	51.87	51.87
	[10.85]***					[4.72]***	[4.54]***	[5.73]***
Terraced [0/1]	56.64					56.77	56.77	56.77
	[10.97]***					[9.12]***	[5.57]***	[5.51]***
Wheat yield in tons per ha		-3.10				-0.95	-0.95	-0.95
		[14.30]***				[4.37]***	[1.68]*	[2.41]**
Distance from home in km			10.68			3.43	3.43	3.43
			[10.70]***			[2.67]***	[1.15]	[2.17]**
Medium soil quality [0/1], Good soil omitted				13.05		2.96	2.96	2.96
				[5.49]***		[1.03]	[0.70]	[0.87]
Poor soil quality [0/1]				46.57		21.34	21.34	21.34
				[19.72]***		[6.68]***	[3.32]***	[4.33]***
Insolation ratio					-2.24	-13.66	-13.66	-13.66
					[0.42]	[2.06]**	[1.20]	[1.51]
Constant	15.15	79.37	32.15	15.34	42.68	35.37	35.37	35.37
	[11.25]***	[28.19]***	[27.50]***	[7.57]***	[7.79]***	[4.39]***	[2.42]**	[3.14]***
Observations	3184.00	1854.00	3236.00	3232.00	3177.00	1811.00	1811.00	1811.00
R-squared	0.20	0.10	0.03	0.15	0.00	0.31	0.31	0.31

** significant at 5%; *** significant at 1%

Robust t statistics in brackets
Absolute value of t statistics in brackets

Table 9. Effects of household characteristics on probability of enrollment at the plot level [%]

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Total household land holdings	0.33 [2.75]***											
Number of household members		0.35 [0.56]										
Education of household head (years)			0.34 [1.43]									
Age of household head				-0.07 [0.93]								
Number of household members working on-farm					-0.61 [0.80]							
Number of household members working off-farm						4.26 [0.89]						
Proportion of household members working on-farm							0.97 [0.85]					
Propotion of household members working off-farm								-1.75 [0.54]				
Land area per household member [*mu*]									0.90 [2.26]**			
House area in m2										0.00 [0.28]		
House area adjusted for household size											-0.01 [0.40]	
Animal units												0.67 [1.25]

Constant	36.72	39.33	38.54	44.40	41.97	40.35	40.37	41.61	38.07	41.09	41.25	40.15
	[21.75]***	[14.08]***	[22.62]***	[10.95]***	[25.82]***	[42.49]***	[42.32]***	[25.35]***	[26.21]***	[26.77]***	[27.06]***	[39.92]***
Observations	3240.0 0	3240.0 0	3204.0 0	3222.0 0	3240.0 0	3240.0 0	3240.0 0	3240.0 0	3240.0 0	3160.0 0	3160.0 0	3225.0 0
R-squared	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.18	0.18	0.19

Absolute value of t statistics in brackets ** significant at 5%; *** significant at 1%

All regressions in this table include a complete set of village dummy variables [coefficients not shown].

Except for the education of the household head, for which lagged values were not asked, all variables that change over time have been lagged to 1998 values

Table 10. Effects of household characteristics on probability of enrollment at the plot level, controlling for plot characteristics [%]

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Total household land holdings	0.23 [2.20]**											
Number of household members		0.94 [1.74]*										
Education of household head [years]			0.35 [1.72]*									
Age of household head				-0.11 [1.51]								
Number of household members working on-farm					-0.82 [1.21]							
Number of household members working off-farm						2.08 [2.10]**						
Proportion of household members working on-farm							-5.58 [1.97]**					
Propotion of household members working off-farm								9.42 [2.27]**				
Land area per household member [*mu*]									0.41 [1.18]			

House area in m2										0.02		
										[1.34]		
House area adjusted for household size											0.01	
											[0.59]	
Animal units												0.48
												[1.03]
Sloped [0/1], Flat omitted	15.33	15.43	16.39	15.71	15.52	15.50	15.51	15.46	15.47	15.65	15.69	15.65
	[7.76]***	[7.82]***	[8.28]***	[7.95]***	[7.87]***	[7.86]***	[7.87]***	[7.84]***	[7.84]***	[7.82]***	[7.84]***	[7.91]***
Steep [0/1]	43.33	43.60	44.40	43.57	43.57	43.66	43.69	43.58	43.43	43.88	43.82	43.65
	[20.77]***	[20.92]***	[21.25]***	[20.85]***	[20.91]***	[20.95]***	[20.96]***	[20.92]***	[20.81]***	[20.67]***	[20.64]***	[20.87]***
Very steep [0/1]	55.20	57.21	59.22	58.92	56.82	56.60	57.25	56.61	55.73	57.05	57.08	56.76
	[8.77]***	[9.15]***	[9.27]***	[9.20]***	[9.09]***	[9.06]***	[9.16]***	[9.06]***	[8.80]***	[9.08]***	[9.08]***	[9.07]***
Terraced [0/1]	39.75	39.77	39.81	39.72	40.04	38.76	40.54	38.42	39.66	39.59	39.70	39.42
	[8.48]***	[8.48]***	[8.51]***	[8.47]***	[8.50]***	[8.24]***	[8.60]***	[8.15]***	[8.46]***	[8.41]***	[8.43]***	[8.39]***
Medium soil quality [0/1], Good soil omitted	9.49	9.36	8.77	9.16	9.39	9.58	9.24	9.68	9.55	9.51	9.48	9.55
	[4.44]***	[4.38]***	[4.08]***	[4.27]***	[4.39]***	[4.48]***	[4.32]***	[4.53]***	[4.47]***	[4.39]***	[4.37]***	[4.45]***
Poor soil quality [0/1]	27.06	26.87	26.39	26.78	26.79	27.01	26.67	27.20	27.00	27.12	27.07	27.00
	[11.63]***	[11.55]***	[11.28]***	[11.46]***	[11.50]***	[11.61]***	[11.45]***	[11.67]***	[11.59]***	[11.45]***	[11.43]***	[11.55]***
Constant	0.44	-0.74	0.76	8.48	4.83	2.16	5.85	2.01	1.97	1.34	2.23	2.61
	[0.19]	[0.25]	[0.32]	[2.13]**	[2.08]**	[1.08]	[2.51]**	[1.01]	[0.89]	[0.58]	[0.98]	[1.30]
Observations	3174.00	3174.00	3138.00	3156.00	3174.00	3174.00	3174.00	3174.00	3174.00	3094.00	3094.00	3159.00
R-squared	0.40	0.40	0.41	0.40	0.40	0.40	0.40	0.40	0.40	0.40	0.40	0.40

Absolute value of t statistics in brackets ** significant at 5%; *** significant at 1%

All regressions in this table include a complete set of village dummy variables [coefficients not shown].

Except for the education of the household head, for which lagged values were not asked, all variables that change over time have been lagged to 1998 values

Table 11. Weight placed on wheat yields in decision-making

	No choice					Choice				
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Household mean	6.25	6.18	6.07	5.43		7.74	7.57	7.58	6.88	
	[9.36]***	[9.28]***	[9.25]***	[8.67]***		[8.84]***	[8.68]***	[8.77]***	[8.18]***	
Village mean	-1.40	-2.27				-1.23	-2.48			
	[1.43]	[3.20]***				[1.11]	[2.83]***			
Directional mean	-1.08		-1.92			-1.68		-2.30		
	[1.26]		[3.13]***			[1.86]*		[3.20]***		
Wheat yield [MT/ha]	-7.10	-7.27	-6.98	-7.29	-3.47	-7.68	-7.93	-7.61	-7.98	-2.57
	[13.25]***	[13.98]***	[13.18]***	[13.96]***	[11.92]***	[10.19]***	[10.65]***	[10.13]***	[10.68]***	[7.10]***
Constant	86.90	87.23	80.93	69.41	89.44	75.11	74.88	68.76	54.25	70.77
	[12.34]***	[12.39]***	[14.29]***	[16.05]***	[23.58]***	[8.60]***	[8.56]***	[10.40]***	[11.21]***	[15.43]***
Observations	986.00	986.00	986.00	986.00	986.00	743.00	743.00	743.00	743.00	743.00
R-squared	0.20	0.20	0.20	0.19	0.13	0.15	0.15	0.15	0.14	0.06

Absolute value of t statistics in brackets

** significant at 5%; *** significant at 1%

Table 12. Weight placed on distance from home in decision-making

	No choice					Choice				
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Household mean	-17.78	-17.30	-20.61	-19.22		-21.11	-20.94	-21.38	-20.37	
	[6.31]***	[6.11]***	[7.54]***	[7.14]***		[6.05]***	[6.01]***	[6.42]***	[6.31]***	
Village mean	-21.24	-10.63				-1.65	2.34			
	[3.90]***	[2.18]**				[0.26]	[0.43]			
Directional mean	13.95		8.14			5.06		4.46		
	[4.27]***		[2.78]***			[1.17]		[1.22]		
Distance from home [km]	17.22	20.40	18.51	20.38	11.74	18.60	19.46	18.72	19.41	8.19
	[8.75]***	[11.14]***	[9.50]***	[11.12]***	[8.40]***	[7.65]***	[8.40]***	[7.86]***	[8.39]***	[5.46]***
Constant	54.23	54.01	42.70	46.78	38.12	36.57	36.59	35.86	38.01	30.62
	[13.95]***	[13.82]***	[16.86]***	[22.60]***	[22.38]***	[9.48]***	[9.49]***	[13.23]***	[18.42]***	[17.78]***
Observations	1624.00	1624.00	1624.00	1624.00	1624.00	1410.00	1410.00	1410.00	1410.00	1410.00
R-squared	0.08	0.07	0.08	0.07	0.04	0.05	0.05	0.05	0.05	0.02

Absolute value of t statistics in brackets

** significant at 5%; *** significant at 1%

Table 13. Weight placed on slope in decision-making

	No choice					Choice				
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Household mean	-20.92	-18.59	-21.62	-15.80		-29.03	-28.62	-28.63	-25.62	
	[7.20]***	[6.53]***	[7.94]***	[6.78]***		[8.39]***	[8.30]***	[9.06]***	[9.23]***	
Village mean	-2.70	5.62				1.44	5.75			
	[0.69]	[1.71]*				[0.29]	[1.46]			
Directional mean	13.29		12.01			5.55		6.27		
	[3.92]***		[4.23]***			[1.38]		[1.99]**		
Sloped	32.83	35.58	33.11	35.50	27.67	25.20	26.22	25.12	26.52	14.50
	[10.35]***	[11.43]***	[10.52]***	[11.40]***	[9.44]***	[7.43]***	[7.91]***	[7.43]***	[8.01]***	[4.62]***
Steep	66.04	70.67	66.42	70.82	55.21	64.80	66.46	64.60	66.56	40.69
	[18.41]***	[20.70]***	[18.74]***	[20.73]***	[21.60]***	[15.41]***	[16.49]***	[15.58]***	[16.51]***	[13.61]***
Very steep	92.33	101.26	93.43	100.45	76.16	74.66	75.81	74.46	75.46	42.71
	[11.01]***	[12.47]***	[11.36]***	[12.39]***	[10.33]***	[4.42]***	[4.49]***	[4.41]***	[4.47]***	[2.51]**
Terraced	36.26	43.27	36.36	43.83	51.13	81.67	82.82	81.52	82.32	65.56
	[4.13]***	[5.21]***	[4.15]***	[5.28]***	[7.68]***	[9.47]***	[9.77]***	[9.47]***	[9.71]***	[7.75]***
Constant	33.16	36.06	31.49	41.77	18.44	49.27	50.08	50.03	55.74	14.44
	[6.44]***	[7.04]***	[6.94]***	[10.74]***	[10.08]***	[7.77]***	[7.93]***	[8.70]***	[11.16]***	[6.31]***
Observations	1578.00	1581.00	1578.00	1581.00	1599.00	1385.00	1386.00	1385.00	1386.00	1387.00
R-squared	0.27	0.27	0.27	0.27	0.25	0.20	0.19	0.20	0.19	0.14

Absolute value of t statistics in brackets
** significant at 5%; *** significant at 1%

Table 14. Weight placed on soil quality in decision-making

	No choice					Choice				
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Household mean	-34.97	-33.30	-31.65	-23.94		-35.84	-35.14	-34.22	-30.70	
	[10.31]***	[9.93]***	[9.48]***	[7.52]***		[10.32]***	[10.18]***	[10.13]***	[9.52]***	
Village mean	27.57	37.35				12.97	19.33			
	[4.76]***	[7.74]***				[1.96]*	[3.51]***			
Directional mean	13.35		24.98			7.96		12.97		
	[3.04]***		[6.80]***			[1.73]*		[3.38]***		
Medium soil	28.99	30.62	28.12	31.71	20.13	22.03	23.07	21.61	23.56	7.08
	[7.83]***	[8.34]***	[7.56]***	[8.49]***	[5.82]***	[5.65]***	[5.98]***	[5.54]***	[6.08]***	[1.98]**
Poor soil	73.88	77.23	71.28	77.85	53.03	70.49	72.53	69.29	72.71	39.77
	[15.61]***	[16.73]***	[15.06]***	[16.57]***	[15.60]***	[14.12]***	[14.95]***	[13.97]***	[14.92]***	[11.24]***
Constant	-9.86	-7.54	19.85	54.71	16.25	31.75	32.45	47.12	66.55	16.74
	[1.00]	[0.76]	[2.56]**	[9.30]***	[5.50]***	[2.78]***	[2.84]***	[5.68]***	[11.09]***	[5.52]***
Observations	1625.00	1625.00	1625.00	1625.00	1625.00	1406.00	1406.00	1406.00	1406.00	1406.00
R-squared	0.22	0.22	0.21	0.19	0.16	0.19	0.19	0.19	0.18	0.13

Absolute value of t statistics in brackets
** significant at 5%; *** significant at 1%

Table 15. Effects of household characteristics on enrollment [%]

	No choice						Choice					
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Proportion on farm	-2.83	-5.24					-0.68	-5.06				
	[0.60]	[1.26]					[0.14]	[1.15]				
Proportion off farm			-0.74	8.26					15.80	12.26		
			[0.11]	[1.38]					[2.18]**	[1.90]*		
Education of household head [years]					-0.34	-0.19					1.00	0.88
					[1.02]	[0.66]					[2.70]***	[2.68]***
Sloped		19.05		18.89		20.35		10.99		11.16		11.03
		[6.90]***		[6.83]***		[7.34]***		[3.55]***		[3.61]***		[3.54]***
Steep		48.13		48.02		49.36		35.94		35.82		36.12
		[17.39]***		[17.36]***		[17.77]***		[10.62]***		[10.60]***		[10.68]***
Very steep		57.34		56.69		60.42		36.82		35.88		36.73
		[8.43]***		[8.33]***		[8.68]***		[2.38]**		[2.33]**		[2.39]**
Terraced		33.23		31.22		33.02		49.68		47.50		47.83
		[5.48]***		[5.18]***		[5.53]***		[6.37]***		[6.06]***		[6.15]***
Medium soil quality		10.10		10.80		9.69		4.62		4.71		3.64
		[3.33]***		[3.61]***		[3.23]***		[1.35]		[1.38]		[1.06]
Poor soil quality		21.92		22.64		21.63		29.90		30.00		29.39
		[6.65]***		[6.89]***		[6.60]***		[8.18]***		[8.21]***		[7.99]***
Constant	49.55	12.39	48.36	8.65	50.02	10.25	37.26	5.05	35.17	1.36	30.80	-2.06
	[20.78]***	[3.63]***	[36.63]***	[3.16]***	[20.68]***	[3.13]***	[14.80]***	[1.38]	[24.37]***	[0.42]	[11.67]***	[0.56]
Observations	1628.00	1594.00	1628.00	1594.00	1608.00	1574.00	1410.00	1383.00	1410.00	1383.00	1397.00	1370.00
R-squared	0.27	0.48	0.27	0.48	0.27	0.49	0.17	0.36	0.17	0.37	0.18	0.37

Absolute value of t statistics in brackets

** significant at 5%; *** significant at 1%

All regressions in Table 8 include village dummy variables [not shown].