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Effects of China's Sloping Land Conversion Program on Non-farm Labor Market Participation: Separating Substitution and Income Effects using the Timing of Participation[12]

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[2] Weights and measures: 15 *mu*=1 hectare; 8.3 *yuan*=$1 (based on the fixed exchange rate during most of the period studied); 2 *jin*=1 kilogram; 9 *jin/mu* ~= 1 bushel/acre

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. Whether participants successfully move into non-farm employment remains uncertain. This paper uses a household dataset collected by the author 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 exploits largely exogenous variations in the exact timing of enrollment, and measures enrollment in several ways to distinguish among competing channels of the effects of enrollment on employment. Enrollment has a small but significant and robust positive effect on non-farm employment. 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 have implemented payments for environmental services (PES) programs, which 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 largest such programs are the Conservation Reserve Program in the United States, designed primarily to set aside highly erodible land, and payment schemes in Europe aimed at the environmental impacts of intensive agriculture (Zilberman et al. 2008).

In recent years, a number of developing countries have begun implementing PES programs. Two of the best known schemes are in Mexico and Costa Rica, where farmers are paid to plant trees to protect watersheds. Both of these programs are relatively small in scale; their environmental and social benefits have necessarily been limited, and evaluations of their impacts may not generalize to larger-scale programs. 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).

The Sloping Land Conversion Program[3]

China's Sloping Land Conversion Program (SLCP), formerly known as Grain for Green, is by far the largest PES program in any developing country, by almost every measure. 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

[3] 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 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).

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.

Most of the enrolled area is in western China, the poorest area of the country and the one facing the most serious erosion hazards. 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 the most recent Chinese government program to replant marginal cropland and barren hillsides. 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.

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. 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 confiscated for public uses or urban development. Under China's legal system, laws and national regulations give executive departments broad authorities to write national and local implementing regulations (the National Forestry Law and SLCP Ordinance, for example, are only approximately 3500 words each), and courts rarely accept cases of farmers attempting to sue a local government. 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. Although some farmers say they are worse off, 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 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. The sample contract is 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 following paragraph summarizes the contract and its typical implementation.

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. 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. 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[4]. There exists no procedure for un-enrolling a plot once it has 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.

However, 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. 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

[4] 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.

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.

The survey used in this paper asked farmers about the alternatives they were offered as well as the choices they made. 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. 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.

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. Farmers, however, tend to prefer to enroll remote and inaccessible land, whereas local governments sometimes prefer to enroll land near roads, either for ease of inspection or for showcasing. In many villages, only land within a set of contiguous designated areas can be enrolled. Such designated contiguous areas are not recognized by national regulations, but rather are created at the local level to reduce administrative costs, and are often drawn to encompass entire steep hillsides. 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.

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. Many farmers said that planting orchard trees had 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.

Farmers generally report that they received their subsidy payments (although they report some problems discussed in the next paragraph). The main form of corruption in

the program seems to arise when local governments enroll state-owned wasteland, and capture the subsidies for reforesting such wasteland. Under national regulations, only farmers' cropland is eligible to be enrolled and receive subsidies, but over-reporting of the amount of farmland enrolled appears to be very widespread at all levels.

Farmers often receive subsidies late or only in part, but tend to be compensated in other ways. 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 early surveys 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* (approximately $300-400 per hectare) 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. However, over-reporting the areas enrolled or applying subsidies to back taxes may result in administrative sanctions. Serious offenses,

such as simply retaining all subsidies without paying any to farmers have been grounds for dismissal and criminal penalties.

Nevertheless, local governments tend to give plots a passing grade in inspections even if tree survival is poor, and usually extend subsidies beyond the length of time specified in the initial contract. 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.

Reforestation and employment

Although the primary goals of SLCP are environmental, promoting non-farm employment is also an official goal of the program, and one that is vital to the program's long-term success. Policymakers hope that after subsidies expire farmers will find non-farm employment or alternative income from high-value crops. If they do not, and the subsidies are not extended, farmers will either become impoverished without their land or will clear the trees and reverse the ecological benefits of the program.

In remote areas of rural China, where reforestation is most important to the economy, the non-farm employment of farmers has clearly increased substantially over the past ten years of program implementation. These 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.

2. Theoretical framework

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.

Each of the hypotheses is based on the assumption that enrollment is mandatory or exogenous from the perspective of the farmer, an assumption discussed further in the empirical section. 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, 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. 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

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

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 work, while where it decreases income it will increase non-farm work

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 of 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,

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

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

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 were the primary factors keeping working-age individuals on the farm.

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 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, the author designed a household survey of 682 households in Shaanxi Province, which 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 corresponding to two natural experiments. (No other survey of reforestation in China has been designed in such a way as to take advantage of either natural experiment.)

The first natural experiment relates to the phase-in of the program and assigned counties to participate or not participate in particular years. 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.

The second natural experiment consists of a discontinuity in per-hectare subsidy payment rates. 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. The division between the northern and southern subsidy rates mostly follows provincial boundaries, but Shaanxi Province contains both types of counties. Within Shaanxi Province, the southern counties in the Yangtze River basin actually have slightly lower grain yields than do the central counties in the Yellow River basin, so the effects of differences in subsidy rates are not confounded by corresponding differences in opportunity costs. Additionally, Shaanxi Province contains time-series as well as cross-sectional variation in its payment rates; in 1999, subsidy rates were not allowed to vary within a province, and all Shaanxi counties received the lower northern subsidy rate. With a view to evaluating incentive and income effects of the subsidies, the sample includes three central Shaanxi counties and three southern Shaanxi counties, all near the watershed boundary where subsidy payment rates change abruptly.

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[5]. Within each village, households were

[5] 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

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 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.[6]

Questionnaire design

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 nonfarm 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.

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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.

[6] 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.

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, 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 nonfarm 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 nonfarm, 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, I also introduced 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.

Survey management

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". For more information on the survey management, see Appendix 2.

4. Descriptive statistics

Household characteristics

Approximately 86% of households in the sample participated in SLCP, while 57% saw at least one member begin a non-farm occupation between 1998 and 2006 (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.

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). Nearly 87% live in families that participated in the SLCP, which is representative of the study region—in many villages, there were few or no households that did not participate.

(Because of the small numbers of non-participants and potential unobserved heterogeneity, this paper does not use non-participants as a primary identification strategy, merely presenting a few summary statistics comparing them to participants.) The data 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 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[7].

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

[7] 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.

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. The robustness section further discusses the possibility that enrollment may be an endogenous choice variable.

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[8], 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 approximately 400,000 new non-farm jobs out of 15 million participating households that

[8] 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.

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 absolute percentages are much higher for young adults, but the ratio is similar across age groups.

The figure suggests 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 **b** in the following equation:

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

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

The probabilities are assumed to follow an underlying normal distribution, and the equation is estimated by maximizing the following weighted pseudo-maximum likelihood probit function:

$$\ln L = \sum w_j \ln \Phi(x_j b) + \sum w_j \ln\left(1 - \Phi\left(x_j b\right)\right)$$

where w_j=households in village/households sampled in village

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.

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. Although household-year enrollment levels are not randomly assigned in the sense of an intentional experiment, they are essentially not within the control of the job seeker and approach random assignment. The process by which variation in the independent

variable was determined can be described in two steps. First, households were administratively assigned land. 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[9]. 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[10] 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, and there was no systematic relationship between household and land 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.

[9] Such re-allocations were discontinued prior to the beginning of SLCP in 1999.
[10] 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.

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[11]. In principle, the steepest land was retired first. However, to facilitate the administration of the program, not every administrative region (county, township, 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 enrollment on non-farm employment would need to involve both nonrandom assignment

[11]The decline was also likely related to the retirement of Premier Zhu Rongji and a decline in enthusiasm for the program from national leaders.

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 of 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, includes only new jobs that were obtained via active means. 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[12]. "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 specifications seek 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

[12] 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.

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[13]. A proxy for labor savings from SLCP is thus calculated as follows:

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

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; $\Sigma A \in [0 \ldots 1]$

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

[13] 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 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.

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 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. The coefficients represent elasticities, which correspond to approximately a 0.7 percentage point increase in the probability of beginning non-farm employment in a year with a typical enrollment level of 40% of the household's land[14]. The coefficients on control variables indicate that males and young adults are most likely to begin non-farm employment, that education has a positive and concave effect on non-farm employment, and that the rate of movement into non-farm employment has increased over the past decade.

Timing of effects

Table 6 shows that only current-year enrollment has a statistically significant effect on new non-farm employment. The positive (albeit insignificant) coefficients in the y-1 row suggest that enrollment may increase non-farm employment in the year after enrollment as well as in the year of enrollment. However, they are not very different

[14] The magnitude and significance of the results is similar using a linear probability model (results not shown).

from coefficients in the y+1 row, which are results of a specification check estimating the (spurious) effect of future enrollment on past employment. The positive coefficients in the y-1 and y+1 rows may be a result of random sampling error (since they are not statistically significantly different from zero), or may be a result of measurement error in the timing of enrollment or employment changes. Although only current-year enrollment has a statistically significant effect on employment changes, the data are consistent with a story in which the current-year coefficients underestimate the magnitude of the effect, as a result of either attenuation bias from measurement error or failing to capture longer-term effects. By focusing on short-term effects, the analysis gains statistical power crucial to distinguishing among different theoretical hypotheses, though this may come at the expense of underestimating the magnitude of the long-term effects.

Distinguishing theoretical hypotheses

Table 7 presents results from a set of regressions intended to differentiate between the labor savings, income, and liquidity hypotheses. Columns 1 and 3 show significantly positive effects of labor savings controlling for the amount of land enrolled. This is consistent with the hypothesis that labor savings are a key channel of the effect of enrollment on employment.

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.

Results from a separate regression designed to isolate the income effect from the liquidity effect also support the idea that there are no income or liquidity effects, except for a small income effect 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 this group, the coefficient of the grain budget change on employment change is negative and significant for poor households (-.0002815, z=-2.65), and positive and insignificant for non-poor households. This suggests an income effect for poor households (a positive effect on income from participation reduces the likelihood of working, i.e. increases the likelihood of ending employment). The magnitude of this coefficient is smaller than that in Column 6, which is not consistent with a story where liquidity effects cancel out income effects among those not already working, and suggests that liquidity effects do not exist.

8. Threats to validity

Theoretical assumptions

This section discusses four 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 8 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 8 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 8). 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.

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% on 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.

Identification assumptions

1. Enrollment is not an endogenous farmer choice. 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. As a check, Table 8 presents separate coefficients for mandatory and voluntary plots. "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 are almost the same. Results are less statistically significant for mandatory plots, but this is a result of somewhat higher standard errors for mandatory plots rather than any meaningful difference in the point estimates.

2. Non-farm employment, or the expectation of non-farm employment, does not cause enrollment in SLCP. This assumption follows logically for mandatory plots, but could in principle be violated for voluntary plots. To directly test this assumption, 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.

3. Enrollment is exogenous and not affected by other types of selection or omitted variable bias. Although it appears unlikely that endogenous choice or reverse causation are driving the results, selection bias on unobserved variables remains a potential threat to validity. As discussed earlier in the section on sources of variation in the independent variable, there are no obvious candidates for such unobserved variables, and 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, the instrumental variable is an excellent predictor of household-year enrollment, with a t statistic of 71 and an R2 of 0.18. In the second stage, measuring the effects of employment, the coefficient on predicted enrollment is positive and about 60% as large as that on actual enrollment (see Table 9). The coefficients are not statistically significant, but this is primarily a result of higher standard errors rather than smaller coefficients than in the baseline regressions. (According to a regression run using a simulated instrumental variable constructed with white noise to replicate the R^2 of the first stage, attenuation bias can explain the smaller coefficients relative to the baseline regression.) 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).

4. Effects of SLCP on employment are related to household, not 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 8 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 coefficient on village enrollment is small 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.

Other potential biases

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)[15]. For no subsample 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

[15] 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.

presents results clustered at other levels of aggregation. Results remain significant at the 1% 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.

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.

8. 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

			Participation		Anyone new job	
	Total	Std dev	Yes	No	Yes	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

Table 2. Individual characteristics

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

			Participation		Employment status past 10 years				
	Total	Std dev	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									
Nonfarm hours per day	9.4	1.8	9.4	**9.2**		9.4	9.3		**8.3**
Nonfarm 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									
nonfarm days per year 1998	211	103	208	**232**		**197**		**256**	201
nonfarm days per year 2002	223	100	221	**237**		**203**	**264**	**251**	**244**
nonfarm 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					

Table 3. Land characteristics

			Enrolled		Anyone new job	
	Total	Std dev	Yes	No	Yes	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

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 got new job	No	13601	1747	15348
	Yes	528	85	613
	Total	14129	1832	15961

Table 5. Baseline regression results

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

Table 6. Timing of Effects

Independent variable: Proportion enrolled in year …	(1)	(2)	(3)	(4)	(5)
y-2			0.006		
			(0.04)		
y-1		0.179	0.163		
		(1.06)	(0.85)		
y	0.373	0.342	0.337	0.322	0.301
	(2.59)***	(2.40)**	(2.22)**	(2.29)**	(2.04)**
y+1 (specification check)				0.129	0.117
				(0.83)	(0.74)
y+2 (specification check)					-0.008
					(0.03)

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

Table 7. Distinguishing Theoretical Hypotheses

	(1)	(2)	(3)	(4)	(5)	(6)^
Any new enrollment				0.362		
				(3.17)***		
Proportion land enrolled	1.143	0.725	1.151			
	(2.64)***	(3.25)***	(2.62)***			
Labor svgs (frac yr)	0.75		0.73			
	(2.18)**		(2.11)**			
Grain budget change		0	0	0	0	-0.001
		(0.97)	(0.24)	(0.58)	(1.64)	(2.55)**

^(6) Poor households only (below median in house area/sqrt(household size) in 1998)
All estimations include a complete set of county and year dummy variables (not shown)

Table 8. Robustness Checks

	(1)	(2)^	(3)^^	(4)	(5)	(6)	(7)
Proportion planted to ecological	0.051						
	(0.15)						
Proportion planted to economic	0.582						
	(2.45)**						
Proportion planted to ambiguous	0.444						
	(1.70)*						
Proportion land enrolled		0.532	0.297				0.519
		(3.12)***	(1.66)*				(3.09)***
Proportion land enrolled: Voluntary				0.479		0.494	
				(2.30)**		(2.35)**	
Proportion land enrolled: Mandatory					0.407	0.421	
					(1.62)	(1.68)*	
Proportion village land enrolled							-0.134
							(0.89)

^(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 county dummy variables (not shown)

Table 9. Instrumental variable regression results

	(1)	(2)	(3)^	(4)^	(5)^^	(6)^^
Proportion land enrolled	0.52		0.458		0.463	
	(3.10)***		(2.84)***		(3.34)***	
Estimated proportion land enrolled		0.303		0.218		0.268
		(0.75)		(0.63)		(0.74)

^(3), (4) no probability weights

^^(5), (6) no control variables

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

Table 10. Alternative weighting and clustering

	(1)^	(2)	(3)^^	(4)^^^
Proportion land enrolled	0.508		0.52	0.52
	(3.10)***		(3.15)***	(3.34)***
Rank of grain budget change		0		
		(0.33)		

^ (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 county and year dummy variables (not shown)

Figure 1. New nonfarm jobs and new enrollment

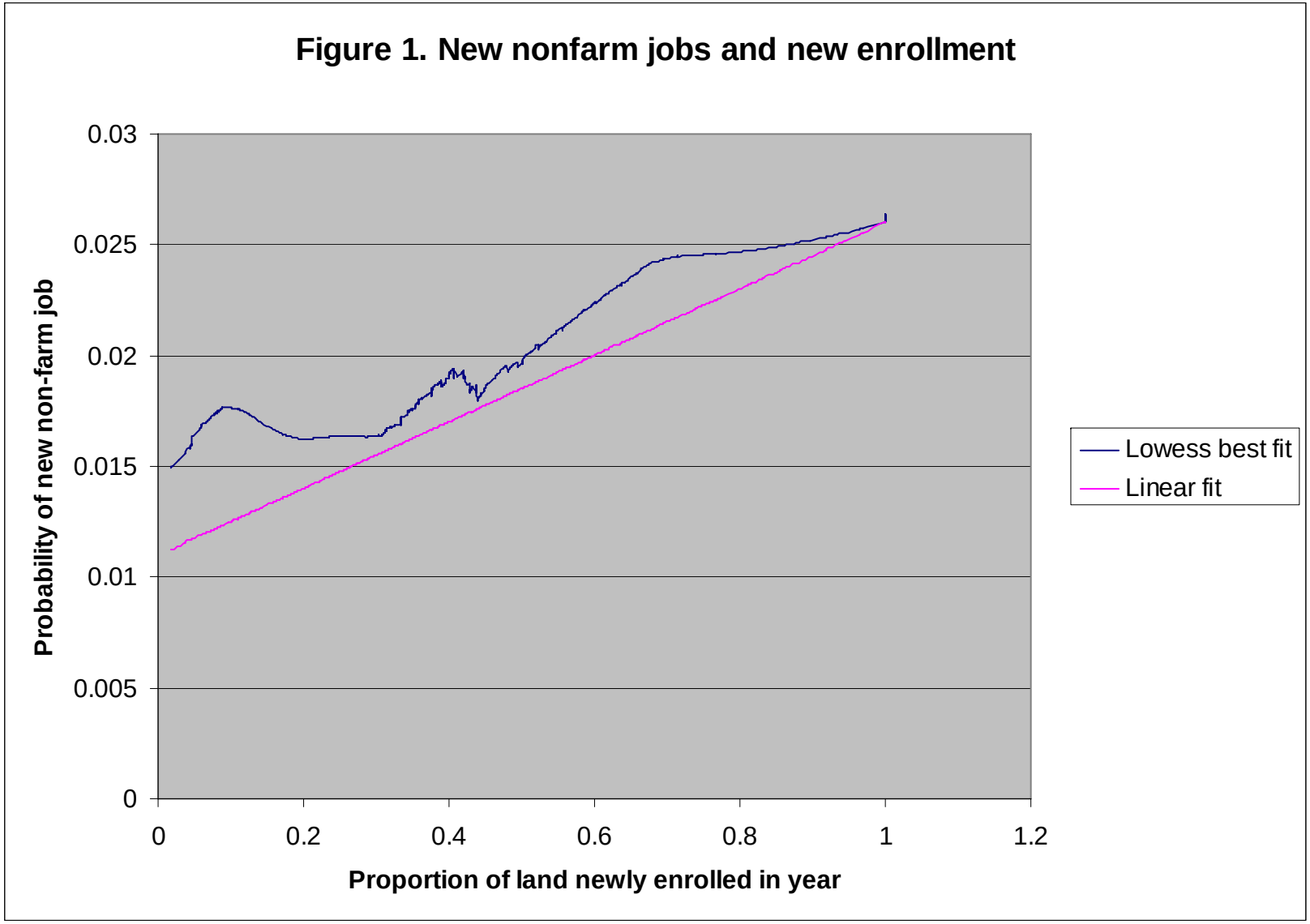
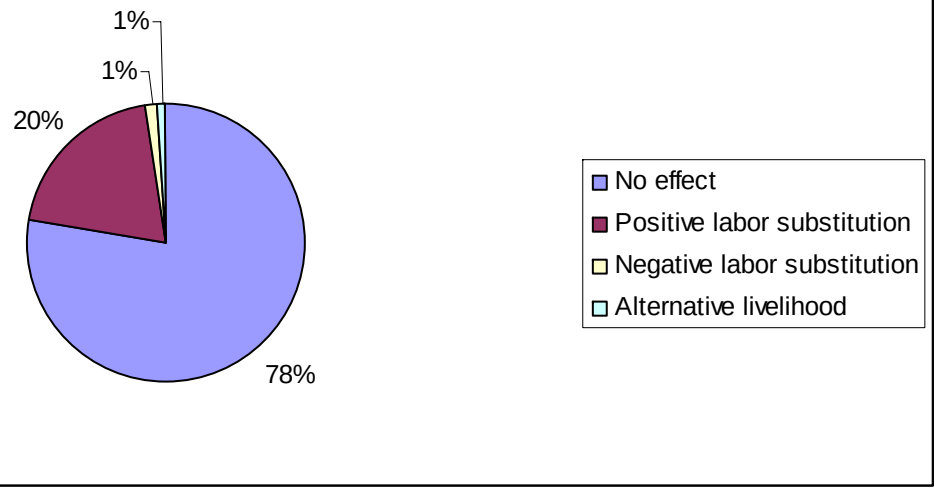


Figure 2. Farmer opinions on the effect of SLCP on non-farm employment



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. Survey management

The author's own role in the survey was probably greater than that of any previous American graduate student in a household survey in rural China. The survey team consisted of 16 Master's students in Economics and Forestry from the Northwest Sci-Tech University of Agriculture and Forestry, and three supervisors, including the author and two mid-career MBA students (Shenglong Wang and Jin Wang). A training manual was developed specially for the survey, and a pre-test conducted, both with the help of several graduate students in Beijing who had worked for numerous rural economic surveys.

During the survey, the survey team split into two sub-teams, and at least one of the three supervisors accompanied each team each day. The supervisors rotated among different teams on different days. 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 data cleaning was performed by the author.

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 corruption 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.