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MICROINSURANCE, TRUST AND ECONOMIC DEVELOPMENT: EVIDENCE FROM A RANDOMIZED NATURAL FIELD EXPERIMENT

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ABSTRACT

We report results from a large randomized natural field experiment conducted in southwestern China in the context of insurance for sows. Our study sheds light on two important questions about microinsurance. First, how does access to formal insurance affect farmers' production decisions? Second, what explains the low takeup rate of formal insurance, despite substantial premium subsidy from the government? We find that providing access to formal insurance significantly increases farmers' tendency to raise sows. We argue that this finding also suggests that farmers are not previously insured efficiently through informal mechanisms. We also provide several pieces of evidence suggesting that trust, or lack thereof, for government-sponsored insurance products is a significant barrier for farmers' willingness to participate in the insurance program.

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1 Introduction

Farmers in less developed economies face significant barriers in access to credit, insurance and other financial products taken for granted in developed countries. At the same time, the risks they face are typically far more significant relative to their income than their counterparts in developed economies. Lack of access to credit prevents potential entrepreneurs among farmers from obtaining the necessary capital to start or expand their businesses, forcing them to either stay in traditional farming or take other less profitable paths. Lack of access to formal insurance market can similarly prevent farmers from pursuing production activities that may be risky but have potentially large returns.

International aid agencies, non-governmental organizations, and profit or non-profit private banks have devoted a large amount of resources to provide credits to residents in low income regions. The best story of microfinance is that of Muhammad Yunus and Bangladesh's Grameen Bank which he founded in 1976, and was replicated in more than thirty countries from East Timor, Bosnia and even many poor neighborhoods in the United States.[1] Academically, a large empirical literature has documented the success of microfinance programs and a theoretical literature is also developed to understand its success (see de Aghion and Morduch, 2005 for a comprehensive review).

Surprisingly, there has been much less effort, both practically and academically, devoted to provide microinsurance to farmers in low income economies. As Morduch (2006) observed: "The prospects (of microinsurance) are exciting, but much remains unknown. The expanding gaggle of microinsurance advocates are ahead of the available evidence on insurance impacts. ... The advocates may be right, at least in the long-term, but it is impossible to point to a broad range of great evidence on which to base that prejudice."

Studying the causal effect of insurance on agricultural production using observational data is a challenging task because of the problem of unobserved heterogeneity. Certain risk types (unknown to econometricians) may self select into some specific insurance scheme, and these risk types may also affect the choice of production technology, effort level and thus output. For instance, more risk averse people may prefer insurance and at the same time devote more efforts in choosing effective technology to protect against animal diseases and epidemics. The presence of self-selection will cause a spurious correlation between insurance coverage and agricultural output.

To overcome the above challenge, we adopt in this paper an experimental approach to study the effect of insurance access on farmers' subsequent production decisions. Our experimental design,

[1] See Yunus (2001) for a documentation of the origins and development of the Grameen Bank, and Robinson (2001) for an account of its replications around the world.

which we explain in details in Section 5, creates exogenous variations in insurance coverage across villages that are arguably orthogonal to agricultural output, and we then use this exogenous variation to identify the causal effect of insurance on production.

Specifically, we report results from a large randomized natural field experiment conducted in southwestern China in the context of insurance for sows. Our study sheds light on two important questions about microinsurance. First, how does access to formal insurance affect farmers' production decisions? Second, what explains the low takeup rate of formal insurance, despite the heavy premium subsidy from governments? We find that providing access to formal insurance significantly increases farmers' tendency to raise sows. We argue that this finding also suggests that farmers are not previously efficiently insured through informal insurance mechanisms. We also provide several pieces of evidence that trust, or lack thereof, for government-sponsored insurance products acts a significant barrier for farmers' willingness to pay for the nominal insurance premium.

The remainder of the paper is structured as follows. Section 2 reviews the related literature; Section 3 provides a simple theoretical framework to illustrate how expanded insurance options could potentially affect economic activity; Section 4 provides the institutional background for hog production, and the insurance program for sows introduced in China in 2007; Section 5 describes our experimental design; Section 6 describes the data sets and provides summary statistics; Section 7 presents our experimental result that sow insurance significantly affects farmers' decision to raise sows in subsequent periods; Section 8 tackles the puzzle of low take-up rate of heavily subsidized sow insurance, linking it to trust both theoretically and empirically; and finally Section 9 concludes.

2 Related Literature

Two recent papers are most related to our study. Gine et al. (2008) studied the determinants of purchasing an innovative rainfall insurance policy offered to small farmers in rural India. They find that insurance takeup is decreasing in the basis risk between insurance payouts and income fluctuations, increasing in household wealth and decreasing in the extent to which credit constraints bind. These results match the predictions of a simple neoclassical model augmented with borrowing constraints. However, they also found that risk averse household are less likely to purchase insurance, and participation in village networks and familiarity with the insurance vendor are strongly correlated with insurance takeup decisions. Closely related, Cole et al. (2008) documented low levels of rainfall insurance take up, and then conducted field experiments to understand why adoption is so low. Their experimental results demonstrate that high price of the insurance and credit constraints of the farmers are important determinants of insurance adoption, but they also find evidence that endorsement from a trusted third party about the insurance policy significantly increase the insurance take up. Our finding of the importance of trust in our setting is consistent with their findings. These two studies do not examine the causal effect of rainfall insurance on agricultural production.

Our paper is also related to the large and important literature in development economics on how poor villagers rely on informal insurance to cope with risks. In a seminal paper, Townsend (1994) asks whether community-based informal insurance arrangements in the three south India villages might in fact be so effective that the poor can do a very good job of protecting their consumption levels against unusual swings in income. His test is simple: under full insurance, individuals' income, sickness and other idiosyncratic shocks should not influence consumption at all once *aggregate* consumption is controlled for. The intuition for this test is that if preferences are time separable and display weak risk aversion, and if all individuals discount the future at the same rate, and if all information is held in common, then an optimal allocation of risk bearing in a stochastic environment implies that all individuals' consumptions are determined by the aggregate consumption, regardless of the date and history of shocks, thus individuals' consumptions must move together. When applying the test using a data set of roughly 120 households in three villages in south India (the ICRISAT data), he found that although the full insurance is rejected statistically, it does provide a surprisingly good benchmark: household consumptions indeed co-move with village average consumptions, and household consumptions are not much influenced by contemporaneous own income, sickness, unemployment or other idiosyncratic shocks once controlling for village consumption.

Townsend's 1994 study has important implications. It suggests that community-based informal insurance effectively shields villagers from their idiosyncratic shocks, thus policymakers should only provide insurance against more aggregate shocks.[2] Townsend (1994), however, does not shed light on the mechanisms in which the almost full insurance is achieved by the India villagers, whether it is informal community-based insurance including gifts and transfers from family networks, and/or borrowing from village lenders; or via individuals building up grain reserves, or through purchase and sales of assets such as bullocks and land to self-insure. Indeed, studies by Rosenzweig (1988), Rosenzweig and Stark (1989) and Rosenzweig and Wolpin (1993) suggest that indeed households in low-income countries employ a variety of channels to diversify their risk exposure, including purchases/sales of durable production assets such as bullocks, diversifying the marriage location of their daughters, and borrowing from family networks. Lim and Townsend (1998) found that in the

[2]Kazianga and Udry (2006) examined the extent of consumption smoothing between 1981 and 1985 in rural Burkina Faso in response to a period of severe drought. They, however, found evidence of little consumption smoothing and almost no risk sharing.

three south India villages the most important mechanism is self insurance through building and drawing down of grain reserves.[3] Udry (1994) found that credit contracts with state-contingent repayments plays an important role in pooling risks across households in northern Nigeria, even though a fully efficient risk-pooling equilibrium is not achieved.

Our paper contributes to a further understanding of the *efficiency* of the informal insurance mechanisms that farmers may have adopted due to the lack of access to formal insurance market. In Section 3, we make a distinction between observing a full insured outcome and having efficient insurance. We illustrate that unless the farmers are previously efficiently insured through informal mechanisms, more efficient insurance institutions will still have an effect on the farmers' production decisions even if we find that their consumption does not fluctuate with their income shocks. Thus, our study on the effect of formal insurance on farmer behavior also provides a test of whether informal insurance mechanisms achieved efficiency: if farmers' behavior does not change with the introduction of formal insurance, it will be evidence that they are efficiently insured to start with; otherwise, it is evidence that informal insurance was not efficient despite the low sensitivity of consumption to individual income shocks.

To the best of our knowledge, our paper is the first to examine the causal effect of microinsurance on production behavior using randomized field experiments. However, there is a small existing literature in agricultural economics that examined the effect of federal crop insurance on farmers' decisions. For example, Horowitz and Lichtenberg (1993) examined how crop insurance affects corn farmers' fertilizer and pesticide use in the U.S. Midwest and found that farmers purchasing insurance applied significantly more nitrogen per acre, spent more on pesticides, and treated more acreage with both herbicides and insecticides than those who did not purchase insurance. Goodwin, Vandeveer and Deal (2004) examined the extent to which crop insurance programs have resulted in additional land being brought into production and found that increased participation in insurance programs led to statistically significant, but very modest, acreage responses. O'Donoghue, Key and Roberts (2007) use a large increase in Federal crop insurance subsidies as a natural experiment to examine how harvested acreage and diversification changed in response to the policy-induced change in insurance coverage. They found that changes in the risk environment do not seem to have large overall effects.

[3]See Besley (1995) for a comprehensive survey about the theories about credit, saving and insurance in developping economy settings.

3 A Theoretical Framework

In this section, we sketch a simple theoretical framework to illustrate the difference between full insurance and efficient insurance and to derive several predictions regarding how insurance may affect production.

Full Insurance vs. Efficient Insurance. Townsend's seminal research suggests that farmers in India villages are rather insulated from idiosyncratic income shocks as a result of informal insurance. Here we would like to point out that, to the extent that the risks that farmers expose themselves to are endogenous, a finding that the farmers' consumption is not sensitive to their individual income realizations after controlling for the village level average consumption does not necessary indicate that making formal insurance available to the farmers would not affect production. That is, a finding of full insurance does not necessarily imply efficient insurance.

Figure 1 illustrates this basic observation. Suppose that a farmer can choose whether to engage in a risky project. If she undertakes the risky project, her consumption in the two states (loss, or no loss) will be at point A. If she does not undertake the activity, her consumption will be at point B, where she will be considered to be fully insured in Townsend's analysis. However, if she had access to an efficient insurance market, her consumption bundle might have been at point C. As depicted, the farmer will choose B over A, i.e., she would not undertake the risky project if she does not have access to the efficient insurance market; but she will choose C over A, i.e., she would undertake the risky project if efficient insurance market is available.

Of course, if the informal insurance available to the farmer actually allows her to achieve C when she undertakes the risky project, then offering the efficient formal insurance would not change her behavior. This suggests that it is not sufficient from the knowledge of whether farmers' consumption are fully insured to infer about whether formal insurance will lead to change in behavior.

This simple observation also suggests a simple *test of the efficiency of informal insurance*. The informal insurance farmers have access to is efficient only if farmers' behavior, measured by their production choices, is not affected when formal efficient insurance is made available. The empirical finding in our paper shows that farmers' sow production is significantly affected when they have access to formal insurance; this suggests that the informal insurance that villagers in our sample might have access to is not efficient.

Insurance and Production: A Simple Model. Formally, consider a farmer with wealth w, who is contemplating whether to raise a sow. Raising a sow is a risky business. If the sow is healthy and productive, the farmer obtains a profit of $K > 0$, but if the sow dies, she suffers a loss

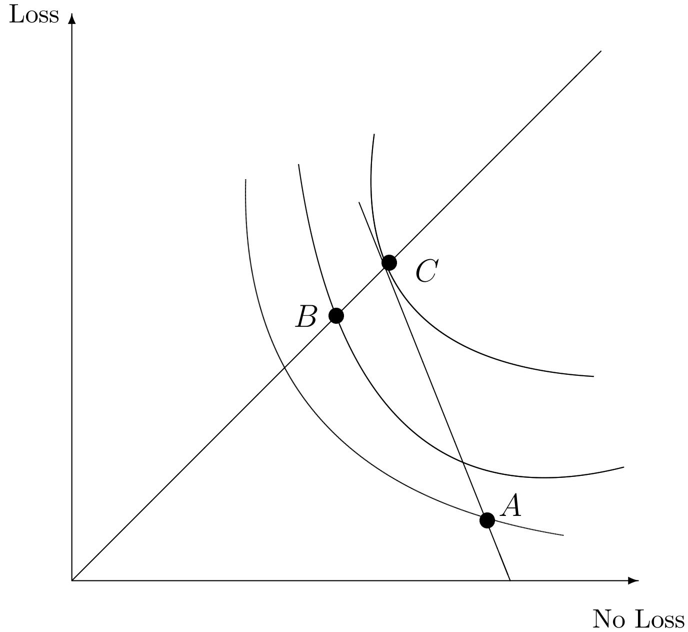

Figure 1: Full Insurance vs. Efficient Insurance: An Illustration

of $L > 0$. The perceived death risk is given by $p \in [0, 1]$, and suppose that p is distributed in the population according to PDF $f(\cdot)$ with mean $\bar{p}$. For simplicity, suppose that the farmer makes only a discrete choice of whether to raise one sow.[4] Let $u(\cdot; \theta)$ denote the farmer's utility function where θ is a vector of parameters that may measure, for instance, the farmer's risk aversion.[5] Her choice problem is:

$$\max \{u(w; \theta), (1-p) u(w+K; \theta) + pu(w-L; \theta)\}. \tag{1}$$

To the extent that there may be heterogeneity among farmers in (w, p, θ), we can denote the set of the farmers who choose $n = 1$ as:

$$S_0 = \{(w, p, \theta) : (1-p) u(w+K; \theta) + pu(w-L; \theta) \geq u(w; \theta)\}. \tag{2}$$

Now suppose that insurance contracts which reimburse the sow farmers of the loss of L in the event of sow death at a premium of $\Delta = \bar{p}L$ become available. The farmer's decision now becomes a two-stage problem: first, she needs to decide whether to raise a sow and then she decides whether

[4]Indeed in China, family-based sow farmers typically raise up to two sows only.

[5]We should interpret the utility function $u(\cdot; \theta)$ as a reduced form for how the farmer approaches risks *given whatever informal insurance she has access to.* Thus, for example, if a farmer is already efficiently insured from the informal insurance mechanisms, her utility function $u(\cdot; \theta)$ will take the linear form.

to purchase the insurance. Formally, her problem is:

$$\max\left\{u\left(w;\theta\right),\max\left\langle\begin{array}{c}\left(1-p\right)u\left(w-\Delta+K;\theta\right)+pu\left(w-\Delta;\theta\right),\\ \left(1-p\right)u\left(w+K;\theta\right)+pu\left(w-L;\theta\right)\end{array}\right\rangle\right\}. \tag{3}$$

Denote the set of farmers who choose $n=1$ (raising one sow) as

$$S_1=\left\{\left(w,p,\theta\right):\max\left\langle\begin{array}{c}\left(1-p\right)u\left(w-\Delta+K;\theta\right)+pu\left(w-\Delta;\theta\right),\\ \left(1-p\right)u\left(w+K;\theta\right)+pu\left(w-L;\theta\right)\end{array}\right\rangle\geq u\left(w;\theta\right)\right\}. \tag{4}$$

The set of new farmers who choose to raise sows with insurance is:

$$S_{1N}=\left\{\begin{array}{c}\left(w,p,\theta\right):\left(1-p\right)u\left(w+K;\theta\right)+pu\left(w-L;\theta\right)<u\left(w;\theta\right)\\ \leq\left(1-p\right)u\left(w-\Delta+K;\theta\right)+pu\left(w-\Delta;\theta\right).\end{array}\right\}$$

The set of farmers who choose to purchase insurance is:

$$S_{1B}=\left\{\begin{array}{c}\left(w,p,\theta\right):\left(1-p\right)u\left(w-\Delta+K;\theta\right)+pu\left(w-\Delta;\theta\right)\\ \geq\max\left\langle u\left(w;\theta\right),\left(1-p\right)u\left(w+K;\theta\right)+pu\left(w-L;\theta\right)\right\rangle.\end{array}\right\}$$

Obviously, $S_{1N}\subseteq S_{1B}\subseteq S_1$. That is, all new sow farmers purchase insurance; but not every sow farmer will buy insurance.

Our first result is that if the insurance premium Δ is actuarially fair or favorable for some risk type p, then access to the insurance contract will increase the level of risky production.

Proposition 1. ***(Insurance and Production)*** *If the farmers are risk averse, then the set of farmers who choose $n=1$ increases when insurance is offered, i.e., $S_0\subset S_1$.*

Proof. A farmer's wealth in the "no loss" and "loss" states if they raise a sow are respectively $(w-\Delta+K,w-\Delta)$ with insurance, and $(w+K,w-L)$ without insurance. For any farmer with $p\geq\bar{p}$, the wealths without insurance represent either a mean-preserving or a mean-decreasing spread. Thus, if the farmers are risk averse, we have:

$$\left(1-p\right)u\left(w-\Delta+K;\theta\right)+pu\left(w-\Delta;\theta\right)>\left(1-p\right)u\left(w+K;\theta\right)+pu\left(w-L;\theta\right).$$

The result follows from inspecting the expressions for S_0 and S_1 respectively in (2) and (4). □

In the above proposition, we assumed that the insurance premium Δ is set to be equal to $\bar{p}L$. However, the same logic implies that the conclusion also holds if, for each risk type p, the insurance contract is offered at the actuarially fair premium of pL. We test for the prediction of Proposition 1 in Section 7.

In the next proposition, we derive some implications on who should purchase the sow insurance, which will have implications for our discussion in Section 8.

Proposition 2. ***(Demand for Insurance)***

1. *If farmers do not have private information regarding p, then all farmers who choose to raise sows should purchase the insurance if they are risk averse.*

2. *However, if farmers have private information regarding p, then at least for some (w, θ), farmers with sufficiently low p will raise sows but do not purchase insurance even if they are risk averse.*

3. *If farmers are risk neutral, then the insurance will be purchased only when farmers have private information about p and only by those with $p > \bar{p}$.*

Proof. If farmers do not have private information regarding p, then each will perceive their risk to be $\bar{p}$. Since $\Delta = \bar{p}L$, the wealths without insurance is a mean preserving spread. Thus all farmers who choose to raise sows will purchase the insurance if they are risk averse.

If p is private information, then for those with sufficiently low p, the wealths with insurance at premium $\Delta = \bar{p}L$ is mean-reducing, even though it reduces variance of the wealth. If farmers are sufficiently close to risk neutral, then such insurance will lead to lower payoff. Thus they will not purchase insurance.

When farmers are risk neutral, only those with $p > \bar{p}$ will find that an insurance at premium $\Delta = \bar{p}L$ is actuarially favorable. Thus these and only these farmers will purchase the insurance. □

4 Institutional Background

Pork is an important part of Chinese daily diet. In 2006, about 52 million tons of pork was produced in China, accounting for 46.9% of world pork production and 64.6% of China's total meat production, and the hog industry was valued at 644.25 billion Yuan, accounting for 48.4% of the total livestock industry.[6]

In China pigs are mainly raised by rural households in their backyard as a sideline business; large-scale hog farms are unusual especially in mountainous regions in southwestern China. The scattered and small scale nature of hog raising not only exposes farmers to market risks, but also to high incidence of pig diseases. Mortality rates for pigs and sows are quite high due to backward breeding technology, weak swine farm infrastructure, poor vaccination and veterinary drug abuse. Natural disasters, such as wind storm, blizzard, thunder, flooding and earthquake also frequently affect pig production.

[6]See Wang and Watanabe (2007) for a comprehensive account of China's hog production.

Since early 2000s China's hog production has suffered from increasingly varied and complex species of pig plagues with the four most common diseases being swine fever, swine erysipelas, hyopneumoniae, and piglets paratyphoid. The frequency and severity of the diseases exceeded the capacity of Chinese veterinary system whose grass-root level animal epidemic prevention teams are poorly paid and inadequately trained. The annual mortality rate for lactating sows was estimated to be around 6% per year.

Infectious animal diseases led to large pork production fluctuations. For example, the bird flu epidemic of 2003 across China caused a sharp decline in the production of live pigs; and in the second half of 2006, the deadly blue ear disease, which spreaded very fast and caused high mortality rate, brought about another shortage in the pork market. Pork price was more than 60% higher in June 2007 than in June 2006. Due to the importance of pork in Chinese diet, the dramatic pork price increase led to intensified public complaint and concerns about food-prices-driven inflation. As a result, the Chinese government decided to intervene and offer government subsidy to increase pork supply. One of these government measures was to offer government-subsidized insurance on sow deaths. In July 2007, the Ministry of Finance initiated a plan specifically subsidizing the insurance of sows raised in the middle and western parts of China. Under the plan, insurance policies for sows at a coverage of 1,000 Yuan in the event of death are offered at a total annual premium of 60 Yuan. However, the central and local governments combined pay for 80 percent of the premium – specifically, the central government contributes 30 Yuan and local governments 18 Yuan – and the farmer only needs to pay 12 Yuan premium for the insurance. The policy will cover deaths of sows caused by major diseases, natural distress, and accidents.[7]

The Property and Casualty Company (PCC thereafter) of the People's Insurance Group of China was designated by the central government as the sole insurance company to deal with the insurance underwriting and claim settlement related to the subsidized sow insurance. Local branches of PCC subsequently cooperated with the Bureau of Animal Husbandry (BAH) at local levels to collect premium payments from pig farmers.

Because the central and local governments pay for 80% of the insurance premium, county and township governments have strong incentives to encourage pig farmers to participate in the insurance plan. To make farmers in remote villages be aware of and access the insurance, BAH at county and township levels mobilized various resources to promote the insurance policy, such as propagandizing the insurance policy and its benefits in local radio and television. But the

[7]In the insurance coverage, major diseases include septicemia, blue tongue, scrapie, swine fever, hyopneumoniae, swine erysipelas, porcine reproductive and respiratory syndrome virus (PRRSV), porcine epidemic diarrhea, streptococcus suis, and foot and mouth disease. Natural diasters include typhoon, tornado, rainstorm, lightning stroke, earthquake, flooding, hailstone, debris-flow and mountain landslide. Accidents include fire, explosion, building collapse, and falling parts or articles from aircraft and other flying objects.

most important channel is the in-person marketing and promotion of the insurance policy in villages through so-called Animal Husbandry Workers (*Xu Mu Yuan* in Chinese, AHW hereafter). Every village has one AHW who is a village resident and works for the BAH on the part-time contractor basis. AHWs serve as the bridge between formal institutions (specifically BAH) and rural villages for matters involving animal husbandry. AHWs are especially important in our experimental area where mountainous land and poor transportation infrastructure make it highly costly for outsiders to access the villages. Typical obligations of AHWs include making immune injection for animals, providing farmers technical assistance on animal-raising and monitoring animal diseases and epidemics.

PCC, in cooperation with the BAH, mobilized the AHWs for the insurance program because the AHWs' expertise in pig-raising, their local knowledge about and connections with villages are crucial for the farmers' participation in the sow insurance. The AHWs act as the messenger to spread the word about the insurance policies, explain to and convince farmers about the policy's benefits, and act as a coordinator between PCC and farmers. For example, the insurance policy requires that each sow be earmarked, literally, with a unique identification number to be eligible for insurance and to be verified in the case of a claim; AHWs thus need to count and check all potentially eligible sows in the village and make earmarks. Local BAHs held special training programs for the AHWs to understand the details of the sow insurance, as well as basic skills of effective promotion and persuasion.

The regular pay for AHWs usually involves some fixed wage (often a tiny amount) and fees for services they provide, and varies across different parts of China. In Jinsha county of Bijie prefecture in Guizhou province, where our field experiment was conducted, AHWs' regular monthly pay is 15 Yuan lump sum and fees for services such as immunization injections. For their involvement in the sow insurance campaign, local branches of PCC paid the AHWs a small *lump sum* to "cover their food and transportation costs." As we will describe in detail below, in our field experiment, we randomly assign the AHWs into different additional incentives for their performance in terms of the number of sow insurance purchases in their villages.

5 Experimental Design

In order to obtain a consistent estimate of the effect of insurance on farmers' production behavior, the key is to isolate an exogenous source of variation in insurance coverage. Within the context of China's sow insurance as described in Section 4, our idea is to randomize the assignment of AHWs into different incentive schemes for their performance in terms of the number of sow insurance purchases in their villages. Different incentive schemes are expected to generate different insurance coverages across villages. Given the randomization, the difference in incentive schemes across villages should be unrelated to the sow output except for the indirect effect on production through insurance coverage. In our main empirical analysis, we will indeed use the random incentive assignment as the instrumental variable for village-level insurance coverage and identify the causal relationship between insurance and production.

Our field experiment was conducted in Jinsha County of Bijie Prefecture in Guizhou province. Located in southwestern China, Guizhou province is a low-income region which relies heavily on natural resources and agriculture. In 2007 the annual per capita net income of farmers was 2,458 Yuan in Bijie prefecture, and was 2,853 Yuan in Jinsha county. Bijie prefecture has a population of 7.38 million and over 93 percent of its area is either highland or mountains. Because of poor road conditions in the highland and mountainous areas, AHWs' effort is crucial in determining the success of the sow insurance program.

The local government of Jinsha County allowed us to run the experiment in 480 villages out of a total 580 villages within its jurisdiction. Based on information from the China Agricultural Census of 2006 (described below in Section 6), there is no systematic difference in all economic indicators, including pig raising, between the villages in our experimental sample and the 100 left-out villages.

In our experiment, we randomly assigned the AHWs of the 480 villages into three incentive schemes.[8] The incentives are summarized in Table 1. In the first group of 120 villages, the AHWs were offered a fixed reward of 50 Yuan to participate in our study with *no* additional incentives. We refer to this group as the *control group* villages. The AHWs in the second group of 120 villages are offered a 20 Yuan fixed reward, and an additional payment of *2 Yuan* for each insured sow. We refer to this group as the *low incentive group (LIG)* villages. In the remaining 240 villages, the AHWs are offered a 20 Yuan fixed reward and an additional payment of *4 Yuan* for each insured sow. We refer to this group as the *high incentive group (HIG)* villages.[9]

We believe that our choices of the fixed payment and the incentives are very attractive to the AHWs. As we mentioned in Section 4, PCC offers only a small lump sum payment to AHWs for their involvement in the sow insurance program; moreover, the regular monthly pay from the BAH for the AHWs is only 15 Yuan. As a result, we expect that our incentive scheme will have a significant effect on the sow insurance purchases.

Each AHW in our experimental village was informed about the assigned reward plan on November 20, 2007 with cooperation from local BAH. They were also told that their performance

[8] See Section 6.4 for formal tests of the quality of randomization.

[9] See Bandiera, Barankay and Rasul (2009), e.g., for a natural field experiment that examines the channels through which team incentives affect productivity.

	Control Group	Treatment Groups	
		Low Incentive Group (LIG)	High Incentive Group (HIG)
Fixed Reward	50 Yuan	20 Yuan	20 Yuan
Incentives	None	2 Yuan/Insured Sow	4 Yuan/Insured Sow
Number of Villages	120	120	240

Table 1: Experimental Design

would be determined by the insurance polices collected and confirmed by PCC. The experiment took five weeks from November 21 to December 25.[10] The data on insured sows in each sampled village were collected in the week just after the experiment ended.

5.1 Discussions

Why do we randomize at the village level? An alternative would be to conduct an experiment where the randomization is at the household level. That is, we may randomly select a set of households and make available to them the formal insurance option, while withholding such options to the non-selected households. However, under such an experimental design, it is inevitable that some households in the same village have access to formal insurance while others do not. It is impractical to refuse to cover those households who were not offered the insurance option, but learned about it from the neighbors and would like to be insured. Such self-selecting households would contaminate the randomization in the experimental data.

Furthermore, there is a more serious shortcoming of household level randomization. As we mentioned in the Introduction, there is substantial evidence that villagers in the same village are likely engaged in informal risk sharing (see Townsend, 1994 for example), randomizing insurance access at the household level may actually lead to an under-estimate of the true effect of insurance on production, due to the potential risk shifting from those households without access to formal insurance to those with access.

Randomizing at the village level has the added benefit that we do not have to collect detailed information about each household, given that we have the fortunate access to the detailed pre-experiment village level information from the China Agricultural Census (CAC) conducted in early 2007.[11]

[10] December 25 was the cut-off date for insurance purchase to be effective from January 1, 2008. Only new sows (that were not officially registered by the AHWs by December 25, 2007) would be accepted for insurance coverage after this cut-off date.

[11] See Section 6.2 below for details about the CAC.

At the village level, a most obvious alternative research design is randomized phase-in.[12] We initially pursued this idea, but the Bijie Prefecture government insisted that preventing some randomly selected villages from accessing the heavily subsidized sow insurance was impractical. We then debated alternative ways to randomly generate differential access to insurance. We believe that, by randomly allocating incentives to the Animal Husbandry Workers, we can generate *de facto* differential access to the insurance product in different villages. Indeed the first-stage result reported in Table 5 below confirmed that incentives we provided to the AHWs led to substantial differences in the number of insured sows.

However, for our experimental design to work, AHWs' efforts must lead to differences in the farmers' insurance purchase decisions. Given that the heavily subsidized sow insurance was available to all villages, AHWs' effort differential could make differences in the farmers' insurance purchase decisions only when they would not have necessarily purchased the insurance, despite the heavy subsidy, without the AHWs' promoting efforts (e.g., explaining and convincing farmers about the benefits of the insurance, etc). This is an important part of how our experimental variations in the AHWs' incentives can generate the desired variations in *de facto* access to the insurance products. We return to examine this issue in Section 8.

Finally, to the extent that our experimental variations in the AHWs' incentives can generate the desired variations in *de facto* access to the insurance products, it has one additional advantage over the random phase-in design. With a random phase-in, villages will either have or not have access to the insurance option, the experimental variation in access to insurance option is restricted to a 0/1 dichotomous variation. In our experimental design, we can in principle generate a much richer variation in insurance access because we can potentially provide a large variety of incentives to AHWs.

6 Data

6.1 Data from the Experiment

The data collected from our experiment is at the village level. For each village, we record the total number of insured sows, including the identification number of the insured sows, as well as a list of the AHW characteristics, including his/her name, age, gender, education etc. We also record the total payment received by the AHW in each village.

[12]See Miguel and Kremer (2004) for an example of randomized phase-in design, and see Duflo, Glennerster and Kremer (2007) for a general discussion about different field experiment designs.

6.2 Other Data Sets

We match the data collected during the experiment with two other data sources: the China Agricultural Census (CAC) of 2006, and the detailed sow death records and sow productions in 2008 from the local Bureau of Animal Husbandry.

China Agricultural Census of 2006. The China Agricultural Census (CAC) was conducted by the National Bureau of Statistics of China between January and February of 2007. It was followed by another two-month of data double-check to ensure the census quality. The CAC covers 250 million rural households in 640 thousand villages and 35 thousand townships in China, and it collects detailed information about agricultural production and services in farming, forestry, husbandry and fishery as of the fourth quarter of 2006.[13] We obtained the detailed CAC data for all villages in our study area, Bijie Prefecture in Guizhou Province.

The CAC has several components, including one that is filled out by village leaders regarding village characteristics such as registered population, villagers working as migrant workers elsewhere, total farm land area, basic infrastructure (such as paved road, water treatment facility, schools, etc.) and village government financial information, etc.

The main component of the CAC data, however, was collected at the household level. Household heads were asked to enter information for every member of their households. We observe from the household component detailed household information including how many individuals reside in the household, their relationship to the household head, their age and gender composition, the amount of contract land, the amount of land in use, ownership of housing, the self-estimated value of house(s), ownership of durable goods, the availability of electricity, water and other amenities, the number of household members that receive government subsidies, and engagement in various agricultural activities including the number of sows and number of pigs raised in the household.

We aggregate up the relevant household data to the village level and then match it, together with the village component of the CAC, to our experimental data using the unique village identification number common to CAC and our experimental data.

Data from the Local Bureau of Animal Husbandry (BAH). We obtained data from the agricultural statistics collected by the local Bureau of Animal Husbandry (BAH). In particular,

[13] The National Bureau of Statistics of China also conducted an earlier round of China Agricultural Census in 1996. The aim of the CAC is to produce reliable statistics for rural population and activities; and it is designed to cover every individual that resided or had registered residence in a rural village at the time of interview. For more detailed information about this census, see "The Action Plan of the Second National Agricultural Census" at http://www.stats.gov.cn/zgnypc/

we obtained the counts of the number of sows in each village tabulated by the BAH on the third and fourth quarter of 2007, as well as the tabulations at the end of the first two quarters of 2008.

We also obtained the sow death records from the BAH. When a sow dies, the village AHW records the death, and collects claim evidence, in particular, the number on the ear of the sow that uniquely identifies the sow. The AHW then submits the list of identification ear numbers of the dead sows to the BAH at the township level and then up to the local BAH, and finally to the local branch (county-level) of the insurance company, PICC Property and Casualty Company. The insurance company then sends its claim staff to check and confirm the death and its reasons. If the death is confirmed to be covered by the policy, the company makes compensation payment to the farmer.

6.3 Descriptive Statistics

Table 2 presents the basic summary statistics of the key variables used in our analysis, both for the whole sample and separately by experimental groups. An observation is a village that participated in our experiment. The pre-experiment variables are characteristics of the villages collected before our experiment period (November 21 to December 25, 2007), mostly from the Chinese Agricultural Census. The average number of sows in December 2006 was 16.3 across all 480 villages; and it was 17.9, 13.2 and 16.9 respectively among the control, LIG and HIG villages. Even though the means are different, a formal test cannot reject the null that the means of the three groups are equal (with a p-value of 15.6%).[14] It is interesting to note that the average number of sows across all villages increased by almost 80 percent from 16.3 in December 2006 to 29.1 in September 2007, right before we conducted our study. In fact, the average numbers of sows in September 2007 were very close across the three experimental groups, with means being 28.8, 28.1 and 29.8 respectively for the control, LIG and HIG villages. The number of pigs in each village is about 350 in December 2006. The average population is about 1000 with an average age of about 33, and about 20% of the villagers work elsewhere as migrant workers. About 54% of the population in each village is male, and the average years of schooling is about 6, which means that the average person in the villages just about finished the elementary school. Each household has about 4.3 *Mu* of land – equivalent to about .71 acres – that is typical for this part of China.

We also reported in Table 2 the means of two variables that we will use in our analysis in Section 8 below. On average, there are about 551 villagers participating in the government-run

[14]In fact, with the exception of the variable "Fraction Male in Village," for all the other pre-experiment variables in Table 2, formal tests show that the null that the means are equal across the three experimental groups are not rejected. The "Fraction Male in Village" in LIG villages is 0.49% higher than that in the control group villages and it is significant with a p-value of 1.7%. The details are available from the authors upon request.

	Whole Sample		By Group					
			Control		Low Incentives		High Incentives	
Variables	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Pre-Experiment Variables:								
No. of Sows in Dec. 2006	16.3	21.4	17.9	26.5	13.2	14.2	16.9	21.5
No. of Sows in Sept. 2007	29.1	31.8	28.8	43.1	28.1	20.5	29.8	29.4
No. of Pigs in Dec. 2006	356.2	228.4	363.9	248.3	338.3	228.1	361.3	218.4
Village Population	1029.1	677.8	1048.7	654.2	1017.9	672.0	1025.0	694.5
No. of Villagers as Migrant Workers	196.0	116.4	188.8	127.1	193.7	103.1	200.8	117.5
Ave. Villager Age	33.2	2.1	33.1	2.1	33.4	2.3	33.2	1.9
Ave. Villager Education (Years)	5.95	0.75	6.00	0.78	6.00	0.77	5.90	0.73
Fraction Male in Village	0.54	0.03	0.54	0.03	0.55	0.03	0.54	0.02
Land per Household (Mu)	4.31	1.97	4.09	1.91	4.28	1.95	4.43	2.00
No. of Surnames in the Village	5.36	2.68	5.41	3.08	5.19	2.47	5.42	2.57
No. of Villagers in New Medical Coop. Scheme	551.5	300.7	560.3	322.3	532.1	299.2	556.9	291.2
No. of Households Receiving Gov. Subsidy	182.2	92.2	183.9	90.4	178.8	99.6	183.0	89.5
Post-Experiment Variables:								
No. of Insured Sows	22.67	26.88	15.47	10.69	21.51	19.48	26.85	34.03
No. of Sow Deaths in Snow Storm	0.19	0.83	0.08	0.40	0.10	0.44	0.28	1.09
No. of Sows in March 2008	38.4	34.3	32.5	24.2	35.7	31.4	42.1	38.7
No. of Sows in June 2008	42.9	37.7	36.6	26.6	39.8	35.8	46.9	42.1

Table 2: Summary Statistics of Key Village-Level Variables.

NOTE: Each observation is a village. The whole sample consists of 480 villages, with 120, 120 and 240 respectively in the control group, the low and high incentive treatment groups.

New Cooperative Mecical Scheme (NCMS). Among all the villages, the NCMS participation rate has a mean of 57% and a standard deviation of about 16.5%, thus there is substantial variation across villages in the NCMS participation rates. Also, about 17% of the individuals receive some form of government subsidy. We will use these two variables as proxies for trust for government sponsored programs in our investigation of the role of trust in Section 8.

Table 2 also reports the summary statistics of several post-experimental variables. The average number of insured sows across the villages is 22.67. If we use the number of sows in September 2007 as the actual number of sows eligible for insurance, the aggregate takeup rate is about 78%. However, there is substantial variation in insurance sign up rates across the experimental groups. Among the control group villages, an average of 15.47 sows out of an average of 28.8 available sows were insured; in LIG villages, an average of 21.51 sows out of an average of 28.1 available sows were signed up for insurance; and in HIG villages, an average of 26.85 out of 29.8 sows were signed up for insurance.

We also report the number of sow deaths during the deadly snow storm between January 12 and February 25, 2008. On average, 0.19 sows died in one village. In addition, the table shows that the number of sows in March 2008 and June 2008 continues to rise from the levels in September 2007.

6.4 Test of Randomization

In Table 2 we showed that for almost all the pre-experiment variables, their means are equal across the villages assigned in the three experimental groups. Table 3 reports a more formal test of the quality of randomization underlying our experiment. It regresses the probability of being assigned to the three experimental groups on a list of pre-experiment village-level variables. We report the coefficient estimates from the linear probability model, as well as the multinomial Probit and Logit models. Table 3 overwhelmingly shows that none of the included variables predict the experimental group assignment. In the whole Table 3, which reports 72 coefficient estimates, only two are marginally significant at 10% level. Also, note that the adjusted R^2 for the linear probability models and the pseudo-R^2 for the Logit model are both less than 0.017, suggesting that the experimental group assignments are very much random.

7 Results on the Effect of Insurance on Production

In this section, we report the results on the effect of insurance on subsequent sow production. We first report in Section 7.1 OLS results where we regress the number of sows measured in March

Variables	Linear Probability		Probit		Logit	
	LIG	HIG	LIG	HIG	LIG	HIG
No. of Sows in Dec. 2006	-.0017	.0004	-.0110	-.0027	-.0149	-.0038
	(1.69)	(.24)	(1.59)	(.50)	(1.64)	(.60)
No. of Pigs in Dec. 2006	-9.9e-06	.0001	.0001	.0002	.0002	.0003
	(.07)	(.41)	(.12)	(.31)	(.16)	(.33)
No. of Villagers in New Medical	-.0001	.0001	-.0003	.0003	-.0004	.0004
Cooperative Scheme (NMCS)	(1.16)	(.98)	(.62)	(.55)	(.52)	(.57)
No. of Households Receiving	.0006	-.0006	.0018	-.0011	.0022	-.0014
Gov. Subsidy	(1.48)	(1.22)	(.0021)	(.55)	(.80)	(.55)
Ave. Villager Age	.005	.0013	.0319	.0198	.044	.027
	(.46)	(.12)	(.0471)	(.50)	(.71)	(.53)
Ave. Villager Education (Years)	.042	-.056*	.076	-.150	.109	-.176
	(1.13)	(1.76)	(.40)	(1.00)	(.41)	(.90)
Fraction Male in Village	1.13	-.46	5.00	.920	7.38	1.80
	(1.12)	(.43)	(1.20)	(.26)	(1.26)	(.41)
Village Population/1,000	.009	-.008	.024	-.005	.041	-.007
	(.33)	(.18)	(.16)	(.03)	(.21)	(.03)
No. of Villagers as	1.7e-06	.0002	.0004	.0007	.0007	.0009
Migrant Workers	(.01)	(.87)	(.36)	(.90)	(.46)	(.92)
No. of Surnames in the Village	-.009	.005	-.030	.003	-.048	-.0006
	(.90)	(.38)	(.53)	(.05)	(.63)	(.01)
Whether Village is the Township	-.031	.165	.380	.759*	.504	.948
Government Location?	(.35)	(1.60)	(.81)	(1.76)	(.77)	(1.64)
Land per Household (Mu)	-.0019	.0130	.0305	.0546	.0397	.712
	(.14)	(1.37)	(.38)	(.99)	(.36)	(.99)
(Pseudo-) R^2	.0167	.0156	...		.0162	

Table 3: Test for the Quality of Randomization for the Field Experiment.

NOTES: (1) Absolute values of $t-$statistics are reported in parentheses. Robust standard errors clustered at the township level are used in calculating the t statistics; (2) All regressions include an unreported constant term; (3) *, **, *** denote significance at 10%, 5% and 1%, respectively.

2008 and June 2008, about three and six months after our experiment respectively, on the number of sows insured during our experimental period. However, in order to estimate a causal effect of insurance on production, one needs to exploit some exogenously induced variations in insurance coverage. In Section 7.2, we use the random experimental group assignment as instruments for the number of insured sows in order to recover the causal effect of insurance access on subsequent production.

7.1 Results from the OLS Regressions

Table 4 reports results from the following OLS specifications:

$$\text{Y}_i = \alpha_0 + \alpha_1 \text{Insured\_Sows}_i + \text{Sows2006}_i + \text{Township\_Dummies} + \epsilon_i, \tag{5}$$

where Y_i represents the number of sows in village i measured in March 2008 (Panel A) or June 2008 (Panel B), "Insured_Sows" represents the number of insured sows in village i by the end of the fourth quarter of 2007, and "Sows2006" represents the number of sows measured in December 2006, and a set of Township dummies are included in some specifications in order to control for the effects of township-specific characteristics on sow-raising.[15] Robust standard errors clustered at the township level are used to calculate the t-statistics reported in parenthesis in Table 4.

Focusing on the specifications with both controls of Township dummies and Sows2006 reported in Column (3) of Panel A and Column (6) of Panel B, we see that insuring one more sow in the fourth quarter of 2007 is associated with 1.093 more sows raised in March 2008 and 1.158 more sows in June 2008, after controlling for the number of sows in the village at the end of 2006 and the Township dummies. Both coefficient estimates are strongly statistically significant with p-value close to 0.

However, the variation in the number of insured sows used in the above OLS regressions includes not only the exogenous variation induced by the randomly assigned AHW incentives, but also endogenous variations across villages that may result from selection on unobserved heterogeneity across villages. Thus the OLS estimate cannot be interpreted as causal effects of insurance on subsequent production. For example, it could be that a village where more farmers are contemplating raising more sows are more likely to purchase sow insurance when such option is presented. This would lead to an upward bias in the estimated effect of insurance on production.

[15] We choose to include the number of sows measured in December 2006 instead of in September 2007 for two reasons. The pork price spike occurred in early 2007 somewhat unexpectedly, so the sows in December 2006 were raised without the effect from the pork price spike. Second, using sows measured at December 2006 also mitigates the effect of potential behavioral change in anticipation of the possible government subsidized sow insurance.

However, if we were to replace Sows2006 by Sows2007, the estimated coefficient on Insured_Sows barely changes both qualitatively and quantitatively. Results are available from the authors upon request.

Variables	Panel A: No. of Sows in March 2008 (1)	(2)	(3)	Panel B: No. of Sows in June 2008 (4)	(5)	(6)
No. of Insured Sows	1.215***	1.239***	1.093***	1.323***	1.314***	1.158***
	(11.03)	(10.98)	(8.78)	(10.26)	(10.04)	(8.32)
No. of Sows in			.2975***			.3178***
Dec. 2006			(3.69)			(3.28)
Constant	11.14***	16.79***	15.91***	13.21***	17.91***	16.96***
	(4.02)	(15.52)	(12.34)	(4.21)	(14.28)	(11.22)
Township Dummies	No	Yes	Yes	No	Yes	Yes
Adjusted-R^2	.6680	.7793	.7915	.6557	.7637	.7752

Table 4: OLS Regression Results on the Effect of Sow Insurance on Subsequent Sow Production. NOTES: (1) Absolute values of $t-$statistics are reported in parentheses. Robust standard errors clustered at the township level are used in calculating the t statistics; (2) *, **, *** denote significance at 10%, 5% and 1%, respectively.

7.2 Results from IV Regressions

In order to identify the causal effect of insurance coverage on sow production, we need to isolate the exogenous variation in insurance access induced by the randomly assigned incentives we provide to the Animal Husbandry Workers. In this section, we use the experimental group assignment as instruments for insured sows in estimating regression equation (5).

First-Stage Results A valid instrument variable for INSURED_SOWS in Equation (5) requires that it be orthogonal to the error term ϵ and that it be significantly correlated with INSURED_SOWS when all other relevant independent variables are controlled. Since the assignments of experimental group to villages were random and should be unrelated to the sow production at the village level, as demonstrated in Table 3, the first requirement for experimental group assignment as an IV for INSURED_SOWS is automatically satisfied. Indeed Hansen's J-statistics from the IV regression reported in Table 6 is only 0.068; thus the over-identification test does not reject the null that all instruments are valid (with a p-value of 0.7938). This formally confirms that instruments satisfy the assumption that both are orthogonal to the error term ϵ in Equation (5).

Now we report the first-stage result that shows that the second requirement for the IV to be valid is also satisfied. Table 5 reports the result from regressing the number of insured sows on the experimental groups (Low Incentive Group and High Incentive Group, with the Control group as the default category), controlling for the number of sows measured in December of 2006 and a

Variables	No. of Insured Sows (1)	(2)	(3)
Low Incentive Group	6.042**	6.630**	9.658***
	(2.73)	(2.61)	(3.71)
High Incentive Group	11.383***	11.403***	12.093***
	(3.32)	(3.38)	(3.76)
No. of Sows in Dec. 2006			.670***
			(2.84)
Constant	15.467***	18.499***	2.081
	(9.97)	(8.65)	(.34)
Township Dummies	No	Yes	Yes
Adjusted-R^2	.0306	.1991	.4550

Table 5: The Effect of Group Assignments on the Number of Insured Sows: First-Stage Results.
NOTES: (1) Absolute values of $t-$statistics are reported in parentheses. Robust standard errors clustered at the township level are used in calculating the t statistics; (2) *, **, *** denote significance at 10%, 5% and 1%, respectively.

set of Township dummies.[16] Overall we find a very strong and significant incentive effect on the insurance coverage. According to the estimates in the preferred specification (Column 3), moving from the control group with fixed compensation to the low incentive treatment group results in nearly 9.7 additional insured sows. Since the sample mean of the insurance coverage is 22.6, the increase of 9.7 sows represents about 43% of the sample mean, an economically significant effect. Moreover, as expected, we found that this incentive effect is stronger for the high incentive group. When Township dummies are included, the whole set of independent variables can explain more than 45% of the total variation in the number of insured sows. Importantly, the partial R^2 from the first-stage is about 0.0625, and the first-stage F-statistics for the significance of the excluded instrument is 13.49, well above the conventional threshold of 10 for weak instrument for the case of a single endogenous regressor (see Staiger and Stock, 1997, p557; and Baum, Schaffer and Stillma, 2003).

Second-Stage Results. Table 6 reports the second-stage regression results. It shows that when we only use the exogenous variation in insurance coverage induced from the variations in

[16]We have also run speficications with the number of sows measured in September 2007 as additional controls. The coefficient estimates on the group assignments do not change, both qualitatively and quantitatively. These results are available from the authors upon request.

the random assignment of incentives to AHWs, the estimated effects of insurance on subsequent production are smaller than those from the OLS regression reported in Table 4; but nonetheless, the effects of the number of insured sows in the fourth quarter of 2007 on the number of sows measured in March and June 2008 are both statistically and economically significant. In the preferred specifications, Columns (3) and (6), one additional insured sow in the fourth quarter of 2007 increased the number of sows by 0.76 by March 2008 and by 0.819 by June 2008. Both of the coefficient estimates are significant at 5% level.

These effects are very large. From the first-stage result reported in Table 5, we know that the low and high incentive group villages insured about 9.6 and 12.0 more sows, respectively, than the control group villages. These increases in insured sows, according to the estimates in Table 6, led to about 7.3 and 9.1 more sows being raised by March 2008 in the low and high incentive group villages respectively than in the control group villages. Note from Table 2, however, the actual difference in the number of sows in March 2008 between the low incentive group villages and the control group villages is only 3.2, suggesting that if the extra incentives to the AHW workers were not provided in the low incentive villages, there would have been 4.1 fewer sows in these villages than in the control villages because, after all, the control villages had more sows in both December 2006 and September 2007. The difference in the number of sows between the high incentive group villages and control group villages should be understood analogously.

Finally, it is worth mentioning that there seems to some suggestive evidence that the effect of insurance on the number of sows is larger when the sows are measured in June 2008 (6 months after the insurance was provided) than in March 2008 (only 3 months after the insurance was provided). This is true both for the OLS result in Table 4 and the IV results in Table 6. This most likely reflects the natural constraint that turning young female pigs into sows takes time; typically, a farmer makes a decision about whether to keep a female pig as a sow or to slaughter it and sell its pork when the pig is about 9 months old.

8 The Role of Trust in the Demand for Microinsurance Policies in Less Developed Regions

As we discussed in Section 1, for our experimental design to work, it is crucial that not all farmers will purchase the heavily subsidized insurance regardless of what incentives we give to the AHWs. Fortunately for us, this is indeed not the case. As one can see from Table 2, for control group villages, there were about 28.8 sows on average in the end of September 2007 (and likely even more in November 2007 when our experiment was conducted), but on average only 15.5 sows

Variables	No. of Sows in March 2008 (1)	(2)	(3)	No. of Sows in June 2008 (4)	(5)	(6)
No. of Insured Sows	.828***	.839***	.760**	.886***	.906**	.819**
	(3.24)	(2.90)	(2.49)	(2.89)	(2.60)	(2.22)
No. of Sows in Dec. 2006			.549**			.574**
			(2.38)			(2.14)
Constant	19.84**	41.93**	2.97	23.03***	54.29***	2.78
	(2.86)	(2.67)	(1.72)	(2.94)	(2.88)	(1.33)
Township Dummies	No	Yes	Yes	No	Yes	Yes
Adjusted-R^2	.6000	.7254	.7680	.5839	.7173	.7550

Table 6: IV Regression Results on the Effect of Sow Insurance on Subsequent Sow Production.
NOTES: (1) Absolute values of $t-$statistics are reported in parentheses. Robust standard errors clustered at the township level are used in calculating the t statistics; (2) The Instruments for the No. of Insured Sows are the group assignments; (3) *, **, *** denote significance at 10%, 5% and 1%, respectively.

were insured. The take up rate for this heavily subsidized insurance option is just over 50% in the control group villages.

In this section, we explore the question of the apparently low takeup rate of the heavily subsidized sow insurance, and in particular, we provide some suggestive evidence for the role played by the villagers' "trust" in these insurance products in their purchase decisions.

It is useful to note that, from a theoretical perspective, there are several reasons that a farmer may refuse to purchase a heavily subsidized insurance. First, even if a farmer is risk averse, she may find the insurance not worthwhile if she has private information that her risk is sufficiently lower than the average sow death probability used in setting the insurance premium (see Part 2 of Proposition 2). Second, if farmers do not have private information, they may refuse to buy the subsidized insurance if they are already fully and efficiently insured by informal mechanisms and thus behave as if they are risk neutral (see Part 3 in Proposition 2). These two possible explanations, however, are inconsistent with our finding reported in Table 5 that sow insurance coverage rates are significantly affected by the randomly assigned incentives to the AHWs. After all, neither private information about the actual risks of sow death risk, nor the farmers' risk aversion could be affected by what the AHW does.

A third potential explanation is that not all farmers are aware of the sow insurance program being introduced in the villages, and AHWs can affect the takeup rates if they work harder to spread the information to more farmers. This explanation is consistent with the first-stage results on the effect of AHW incentives on the insurance takeup rate. But we discuss how our additional evidence below is less supportive to this explanation, especially, the third evidence on the effect of sow deaths during a severe snow storm shortly after our experiment on subsequent sow production.

A fourth potential explanation, which we provide several pieces of collaborating evidence for, is that farmers have different degrees of *trust* for whether the insurance product being offered is credible.[17] It is important to note that, different from any other subsidized program (such as micro-credit) in which farmers *receive* money from the government or financial institutions up front; in the microinsurance setting, farmers are required to *pay* their insurance premium up front, despite the discount, before securing any potential benefit from this policy in the event of a sow death. As a result, interested farmers can be seriously concerned about whether they are able to get the payment as promised in the insurance contract if some unfavorable contingencies occur. The issue of trustworthiness of government policies is particularly relevant in China since governments at all levels often renege on their policy promises, and from the viewpoint of Chinese farmers, local bureaucrats at townships are always searching for "legitimate" reasons to ask them for money and sometimes even cheat them into paying unnecessary fees. More importantly, if local governments fail to deliver their promises in the contract, there is virtually no way for farmers to sue the government in the court.[18]

When farmers do not have complete trust on whether the insurance product is genuine, the insurance policy itself becomes a risk, as seen clearly in the simple model presented below in Section 8.1. Importantly, under the trust explanation, AHWs can dispel farmers' potential doubt about the insurance policies by spending more time explaining to them how the policy works.

As we mentioned in the introduction, our examination of the role of trust for the villagers' demand of insurance echoes that of Cole et al. (2008) in their study of rainfall insurance. We should emphasize that in their setting the rainfall insurance was offered by a for-profit insurance company without premium subsidy. Thus, the trust examined in their setting is the trust for insurance products offered commercially, while in our setting the trust is for government-sponsored, heavily subsidized insurance products. We should also note that we did not randomize trust in our experimental design, while Cole et al. (2008) did in their study. It is interesting to note that our evidence strongly collaborates their findings.

[17] The importance of trust has also been recognized in Guiso, Sapienza and Zingales (2008) , which finds that trust is an important determinant of stock market participation.

[18] In China, the court is controlled by the government and it is very unlikely for the court to make ruling decisions unfavorable to the government.

8.1 A Simple Model of Trust and Insurance Takeup

First, Proposition 2 showed that there is no reason to expect an universal insurance take up even if the premium is heavily subsidized. In particular, when risk type p is private information, those with sufficiently small p will not purchase the subsidized insurance if they are close to risk neutral. However, such an explanation for the low takeup rate of the sow insurance would not have predicted a systematic effect of the incentives we offered to the AHWs on the insurance takeup rates.

We now provide a simple model where the farmers are uncertain of whether the insurance policy will be honored in the event of a sow death. When a farmer pays a premium of Δ for the insurance, she faces uncertainty as to whether the insurance will be honored in the event of a loss realization. Let $t \in [0, 1]$ denote the farmer's probability belief that the insurance product is trustworthy. Now the choice problem for the farmer is as follows:

$$\max \left\{ u(w;\theta), \max \left\langle \begin{array}{c} (1-p)\,u(w+K-\Delta;\theta) + p\left[tu(w-\Delta;\theta) + (1-t)\,u(w-\Delta-L;\theta)\right], \\ (1-p)\,u(w+K;\theta) + pu(w-L;\theta) \end{array} \right\rangle \right\}. \tag{6}$$

The difference from (3) is that now in the event of a loss, the farmer only expects a payment of L with probability t; and with probability $1-t$, she expects that she simply forfeits the premium payment of Δ without receiving any compensation for her loss of L.

Not surprisingly, the set of farmers whose behavior will be impacted by the access of insurance now depends on t. The lower the trust, i.e., the smaller t is, the less the impact of the insurance on farmers' production decisions. In the extreme when t is zero, it is clear from (6) that the insurance option is always dominated by the no insurance option.

8.2 Evidence 1: The Relationship Between Participation in the New Cooperative Medical Scheme and Sow Insurance Purchase

As our first piece of evidence that trust, or lack therefor, for government sponsored programs may prevent farmers from purchasing the heavily subsidized sow insurance, we show that villages where the farmers have previously demonstrated a higher level of participation in another government sponsored voluntary insurance program – the New Rural Cooperative Medical Scheme (NCMS) – are also more likely to purchase the sow insurance.

New Cooperative Medical Scheme.[19] The original Cooperative Medical Scheme (CMS), introduced in rural China in the 1950s, was dismantled with the collapse of the collective economy

[19]See Lei and Lin (2008) and references cited therein for more background information about the NCMS.

in the early 1980s. As a result, by 1985 only about 5% of rural counties in China had any form of health care insurance. Chinese government launched the New Cooperative Medical Scheme in 2003, aiming to provide health coverage for the nation's entire rural population by 2010. Both the central and local governments provide subsidies to the premium. For Guizhou Province, in 2003 when NCMS was initiated, the central and local governments respectively subsidized the program at the rate of 10 Yuan and 40 Yuan per enrollee annually; and the subsidy amounts were increased to 20 Yuan and 50 Yuan respectively in 2006. Each enrollee is only required to contribute 24 Yuan annually. Somewhat surprisingly, the heavily subsidized and reasonably priced NCMS was not an immediate success; at the national level, the percentage of rural residents covered under the NCMS increased from 3% in 2004 to 40.57% in 2006. In our sample of villages, as described in Table 2, about slightly over 50% of the villagers signed up the NCMS, with quite sizeable variations in the coverage rates across villages.

Empirical Result. While there might be a multitude of reasons for the variation in the NCMS coverage across villages, in this section we entertain the hypothesis that one of these sources is the trust for government sponsored programs. Under this hypothesis, if trust is also an important determinant for villagers' purchase of the sow insurance, we would then expect to see that there would be positive correlation between a village's takeup of the NCMS and its takeup of the sow insurance.

Columns (1)-(4) in Table 7 reports the relationship between the number of insured sows and the number of villagers who participated in the NCMS. Results from the preferred specification, which controls for the number of sows in December 2006, as well as the experimental group assignment and Township dummies, are reported in Column (4). The estimated coefficient on the number of villagers in NCMS is positive and significant at the 10% level, thus suggesting that there is some underlying common unobservable factor that leads the villagers to more likely join the NCMS and purchase the sow insurance. At the point estimate, a one standard deviation increase in the number of villagers in the NCMS, which is about 300 from Table 2, will lead to an increase of 4.5 additional insured sows, which represents about 20% of the mean number of insured sows (about 22.7 from Table 2).

While we do not have direct evidence of what the common unobservable factors are, there are several candidates. One is trust, or lack thereof, for government sponsored programs. Villages that have a higher trust for government programs in general will both have a higher participation rate in the NCMS, and are more likely to purchase the subsidized sow insurance. In villages assigned to the high incentive group, the AHWs can increase sow insurance coverage by spending more time to dispel the doubts that farmers may harbor toward the government sponsored insurance product. The fact that AHWs are the residents of villages and their personal reputation is held as a "hostage" if they cheat their fellow villagers helps AHWs do their work well.

Another candidate for the unobservable factor is that villages may differ in how easy the information about the government sponsored programs spread. For example, some villages may have more close-knit social networks that words about the government sponsored programs, whether it is the NCMS or the sow insurance, will quickly spread, leading to higher participation rates in both the NCMS and the sow insurance. This channel, however, does not seem to be very plausible because the NCMS has been in place for almost four years, and it is unlikely that any villager has not heard about the NCMS. Yet, the participation rate for the NCMS is still low in the villages with low participation rate for the sow insurance.[20]

A third candidate is that the villages differ in risk aversion. But as we argued in the beginning of this Section, the incentives we assigned to the AHWs would not have a big effect on the insured sows if villages differ only in risk aversion.

8.3 Evidence 2: The Relationship Between the Number of Households Receiving Government Subsidies and the Sow Insurance Purchase

Our second piece of suggestive evidence that trust for government sponsored programs may be the main reason for the seemingly low takeup rate of the sow insurance is the systematic relationship between the number of households receiving government subsidies and the purchase of sow insurance, reported in Columns (5)-(8) in Table 7. Here we hypothesize that villagers who have been receiving government subsidies tend to have a higher trust for government sponsored programs in general, and thus the subsidized sow insurance product in particular. The preferred specification with all the controls is reported in Column (8) and the coefficient estimate on the number of households receiving government subsidy is positive and significant at 1%. At the point estimate of the coefficient, a one standard-deviation increase in the number of households receiving government subsidy, which is 92 from Table 2, is associated with an increase of 5.4 insured sows, representing about 25% of the mean number of insured sows (about 22.7 from Table 2).

Potential lack of information about the government sponsored programs cannot be the common unobservable factor explaining the positive association between households receiving government subsidies and the number of insured sows. The reason is that eligible households with per capita income below a government specified threshold automatically receive government subsidies; there

[20] Moreover, if closer knit social network is the reason for better information spreading for the government sponsored programs, it would at the same time also lead to less, not more, need for formal insurance.

Variables	No. of Insured Sows (1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No. of Villagers in New	.0124*	.0124*	.0149*	.0146*				
Coop. Medical Scheme	(1.80)	(1.86)	(1.81)	(1.85)				
No. of Households Receiving					.0565***	.0563***	.0610***	.0605***
Gov. Subsidy					(2.92)	(2.99)	(2.77)	(2.86)
No. of Sows in Dec. 2006	.6762**	.6836***	.6004**	.6109**	.6510**	.6584***	.5856**	.5954**
	(2.71)	(2.87)	(2.29)	(2.43)	(2.62)	(2.78)	(2.24)	(2.38)
Low Incentive Group		9.937***		9.526***		9.754***		9.318***
		(3.96)		(3.49)		(3.83)		(3.38)
High Incentive Group		12.340***		11.773***		12.325***		11.819***
		(3.92)		(3.93)		(3.90)		(3.89)
Constant	5.135*	-3.669	-.258	-8.497**	2.102	-6.611	-3.001	-11.218***
	(1.76)	(.91)	(.10)	(2.23)	(.72)	(1.56)	(1.16)	(2.89)
Township Dummies	No	No	Yes	Yes	No	No	Yes	Yes
Adjusted-R^2	.3549	.3903	.4485	.4794	.3710	.4062	.4613	.4923

Table 7: The Relations Villagers Participating in the New Rural Health Coop (Columns 1-4) and the Number of Households Receiving Government Subsidies (Columns 5-8) and the Purchase of the Sow Insurance: Some Preliminary Evidence for the Role of "Trust" for Government.

NOTES: (1) Absolute values of $t-$statistics are reported in parentheses. Robust standard errors clustered at the township level are used in calculating the t statistics; (2) *, **, *** denote significance at 10%, 5% and 1%, respectively.

is no need for filling out an application and thus there cannot be an effect of knowledge about the existence of the subsidy programs.

It is also worth pointing out that villages with more households receiving government subsidies tend to be poorer, and as a result farmers in such villages may be more risk averse. However, this channel would not have explained why the different incentives we provide to AHWs had a large positive effect on the sow insurance purchase. After all, the villages are randomly assigned to the experimental groups, and as Tables 2 and 3 showed clearly, villages assigned to different experimental groups do not seem to exhibit any difference in the numbers of households receiving government subsidies.

8.4 Evidence 3: Sow Death in a Snow Storm and the Effect of Insurance on Subsequent Sow Production

Finally, we report evidence from an unanticipated severe ice and snow storm that occurred shortly after our field experiment to show that lack of trust for government sponsored programs might have played a crucial role in the low takeup rate of sow insurance.

Ice and Snow Storm in Early 2008. In early 2008, just a month and a half after our field experiment, a severe ice and snow storm hit southern and southwestern China and Guizhou was one of the most affected provinces. This storm began in mid-Janurary and ended until mid-Feburary, and its scope and severity were unprecedented in at least the last fifty years. Since snow storms in general are rare in this part of China, let alone one with such severity, many sows and pigs died during the snow storm especially for those sows raised in the backyard of village households which lacked necessary facilities. News report indicated that there were a total of 5,973 sows that died during the storm in Guizhou province. This unusual event, right after our experiment, offers us a rare opportunity to test the role of trust in the purchase of sow insurances.[21]

The idea is simple. Nothing is more convincing to the villagers that the government subsidized sow insurance is for real than actually paying out the promised damage compensation in this unusual event. Indeed, as reported by Xinghua News Agency and Financial Times (Chinese), following government directives, the insurance company quickly dispatched work teams to remote villages to deal with claim evaluations and settlements.[22]

[21] According to Wang and Watanabe (2007), summer months are the most deadly months for pigs in general.

[22] See "Guizhou Province Made Full Compensations on Lactating Sows Which Were Insuranced and Died of the Ice and Freeze Storm," *Xinghua News Agency*, Feburary 20, 2008; and "Insurance Industry Meets with the Ice and Freeze Disaster In the Special Way" (*Financial Times, Chinese,* Feburary 27, 2008).

Variables	Panel A: No. of Sows in March 2008				Panel B: No. of Sows in June 2008			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
No. of Insured Sows	1.01***	.893***	.542**	.507*	1.108***	.979*	.577*	.538
	(3.49)	(12.20)	(2.08)	(1.78)	(10.64)	(1.90)	(1.76)	(1.51)
No. of Sows in Dec. 2006	.357***	.404***	.754***	.707***	.380***	.432***	.830***	.778***
	(4.19)	(3.56)	(3.45)	(3.24)	(3.59)	(3.08)	(3.22)	(3.02)
No. of Sow Deaths in Snow Storm	4.241**	.297	5.485*	-.629	4.113*	-.254	5.525	-1.311
	(2.16)	(.22)	(1.77)	(.39)	(1.83)	(.15)	(1.59)	(.65)
No. of Insured Sows × No. of Sow Deaths in Snow Storm		.109**		.159**		.121*		.178**
		(2.25)		(2.38)		(1.90)		(2.18)
Constant	9.45***	11.27***	13.83***	15.21***	11.44***	13.45***	16.41***	17.95***
	(3.49)	(4.79)	(3.38)	(3.34)	(3.66)	(5.14)	(3.44)	(3.41)
Adjusted-R^2	.7008	.7229	.6440	.6896	.6842	.7066	.6236	.6707

Table 8: The Effect of Sow Deaths During the Snow Storm on Subsequent Sow Production: Further Evidence on the Role of "Trust" for the Demand of the Insurance.

NOTES: (1) Absolute values of $t-$statistics are reported in parentheses. Robust standard errors clustered at the township level are used in calculating the t statistics; (2) The Instruments for the No. of Insured Sows are the group assignments; (3) *, **, *** denote significance at 10%, 5% and 1%, respectively.

Empirical Results. In fact, the ice and snow storm and the subsequent compensation from the insurance company for insured sow deaths has two possible effects. On the one hand, it probably led farmers to have a higher awareness of the riskiness of the environment; on the other hand, the insurance company's prompt claim processing provided farmers a unique opportunity to learn about the credibility of the insurance product. The first effect, in the absence of insurance options, may lead to fewer sows in the future.[23] The second effect implies that, in villages with more sow deaths and more insured sows, there would be more positive cases that the sow insurance contracts are honored. Such positive cases of the insurance contracts being honored would raise the villagers' trust for the sow insurance program. Thus this mechanism will predict that the effect of sow insurance for subsequent sow production should be stronger in such villages.

Table 8 reports results from regressions, both OLS and IV, that examines the effect of insured sows on the sow production measured in March 2008 (Panel A) and June 2008 (Panel B). The specifications are similar to those in Table 6 except that we add "No. of Sow Deaths in Snow Storm" in specifications in odd-numbered columns and additionally we include the interaction of "No. of Insured Sows" and "No. of Sow Deaths in Snow Storm" in even-numbered columns.

In all specifications, the number of insured sows in the fourth quarter of 2007 continue to have a significantly positive effect on subsequent sow production measured in March and June of 2008. The estimated magnitudes of the effect do differ across specifications. In the most preferred specifications, reported in Column (4) and (8) respectively, the estimated coefficients on the interaction term of "No. of Insured Sows" and "No. of Sow Deaths in Snow Storm" are positive and significant at 5% level. Thus indeed, Table 8 provides support for our hypothesis that villages where gains in trust for the government sponsored sow insurance programs are greater do experience a larger production response to the access to insurance. Also note that in specifications (4) and (8), we found that the coefficient on "No. of Sow Deaths in Snow Storm" is negative but insignificant.

9 Conclusion

In this paper, we report results from a large randomized natural field experiment that evaluates the effect of microinsurance on subsequent production. The randomized incentive schemes we offered to Animal Husbandry Workers generate plausible exogenous variations in the effective insurance access across 480 villages in our experimental sample. This allows us to use the random incentive scheme assignment as the instrumental variable for insurance access to recover the causal

[23]However, when access to insurance that does not adjust premium for possible changes in perceived death risk, the effect on future sow production is ambiguous.

effect of insurance access on production.

Our results indicate that having access to formal insurance significantly increases farmers' tendency to raise sows. To the best of our knowledge this is the first large-scale randomized experimental evidence of the effect of microinsurance on farmer production behavior. Our finding also provides a useful test of whether the informal insurance farmers might have access to was efficient. Our finding suggests that microinsurance may be as important as microfinance in poverty alleviation, and microinsurance can supplement and strengthen the effects of microfinance by protection the farmers from the inherent risk of entrepreneurial activities.

We also provide a set of collaborating evidence to suggest that trust, or lack thereof, for government-sponsored insurance products acts a significant barrier for farmers' willingness to participate in the insurance program. This finding is consistent with those of Cole et al. (2008). We believe that overcoming the issue of the lack of trust should be a crucial consideration in the next wave of microinsurance revolution.

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