The Effect of Microinsurance on Economic Activities: Evidence from a Randomized Field Experiment*

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Abstract

Lack of access to formal insurance market may prevent farmers from pursuing risky production activities with potentially large returns. How does access to formal microinsurance affect production and economic development? We report results from a large randomized natural field experiment conducted in southwestern China in the context of insurance for sows. We find that providing access to formal insurance significantly increases farmers' tendency to raise sows. We also find some evidence that lack of trust on the government-sponsored insurance policies is an important impediment to farmers' willingness to purchase the formal insurance.

Keywords: Microinsurance; Production; Field Experiment

JEL Classification Codes: C93; O12; O16

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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, they typically face far more significant risks 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 markets can similarly prevent farmers from pursuing risky production activities with 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 credit 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.[2]

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. Individuals with certain traits may self select into some specific insurance scheme, and these unobserved traits may also affect the choice of production technology, effort level and thus output. For instance, more risk averse farmers 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 use in this paper an experimental approach to study the effect of insurance access on farmers' subsequent production decisions. Our experimental design, explained in details in Section 3, creates an exogenous variation in insurance coverage across villages that is arguably orthogonal to agricultural output, and we then use this exogenous variation to identify the causal effect of insurance on production.[3]

[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.

[2] See de Aghion and Morduch (2005) for a comprehensive review; and see Banerjee, Duflo, Glennerster and Kinnan (2010) for a recent, but the first large-scale randomized evaluation of the impact of introducing microcredit on various measures of economic activities in a new market.

[3] See Harrison and List (2004) and List (2006) for surveys and methodological discussions, including categorizations, of the surging literature of field experiments; and see Duflo, Glennerster and Kremer (2007) for the application of the experimental

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 one important question about microinsurance: how does access to formal insurance affect farmers' production decisions? We find that the wider insurance coverage significantly increases farmers' tendency to raise sows. We also find that the villages which had more sow insurance but at the same time were hit by an unexpected ice and snow storm tend to raise more sows in the next period. The ice and snow storm caused many sow deaths, which provides a rare opportunity for farmers to learn about the credibility of the government-sponsored insurance. Therefore we interpret this empirical result as a piece 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.

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 take-up 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 take-up 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.

To the best of our knowledge, our paper is among the first to examine the causal effect of microinsurance on production behavior using randomized field experiments.[4] However, there is a small existing literature in agricultural economics that examined the effect of federal crop insurance on farmers' decisions using non-experimental data. 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

methods especially in development economics. A website `http://www.fieldexperiments.com` maintained by List provides a useful categorization as well as comprehensive and updated list of papers in this literature.

[4]See Innovations for Poverty Action (2009) for a review of existing and ongoing microinsurance field experiments. Most of the experiments focus on the low take-up rate of insurance products.

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.

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) tests whether community-based informal insurance arrangements might effectively protect the poor's consumption levels from unusual swings in income; he found that, among the roughly 120 households in three villages in southern India, full insurance provides a surprisingly good benchmark although it is statistically rejected.[5] The evidence in our paper that access to formal insurance has a significant effect on the farmers' production decisions suggests that formal insurance can still play an important role even in areas where there are informal risk sharing, possibly because formal insurance allows villagers to better insure against aggregate shocks.

The remainder of the paper is structured as follows. In Section 2 we provide the institutional background for hog production, and the insurance program for sows introduced in China in 2007. In Section 3 we describe and discuss our experimental design. In Section 4 describes the data sets and provide summary statistics. In Section 5 we present and discuss our experimental result that sow insurance significantly affects farmers' decision to raise sows in subsequent periods. Finally in Section 6 we conclude.

2 Institutional Background

Pork is an important part of Chinese daily diet. In 2006 about 52 million tons of pork (valued at 644.25 billion Yuan) were produced in China in 2006.[6]

Sow Mortality Risks. 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 small-scale and scattered 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.[7]

Though there is no nationwide census data to estimate the annual mortality rate for sows, People's Insurance Company of China (PICC) estimates that the mortality rate of *insured* sows was about 2% in 2009.[8] In Yunnan province, which neighbors our study area Guizhou province, the mortality rate for insured sows was estimated to be about 2.04-2.6%.[9] In our own data set described below, the sow

[5]Other studies, for example, Rosenzweig (1988), Rosenzweig and Stark (1989), Rosenzweig and Wolpin (1993), Udry (1994) and Lim and Townsend (1998), studied the various mechanisms through which villagers achieve the informal risk sharing, including gifts and transfers from family networks, borrowing from village lenders, building up grain reserves, purchase and sales of assets such as bullocks and land, and credit contracts with state-contingent repayments.

[6]The hog industry in China was valued at 644 billion Yuan in 2006, accounting for 48.4% of the total livestock industry (see Wang and Watanabe (2007) for a comprehensive account of China's hog production).

[7]There are many causes for pig and sow mortality, including backward breeding technology, weak swine farm infrastructure, poor vaccination, veterinary drug abuse and natural disasters, such as wind storm, blizzard, thunder, flooding and earthquakes.

[8]See `http://www.chinabreed.com/PIG/develop/2009/03/20090326255187.shtml`.

[9]These statistics can be found at `http://www.chinabreed.com/PIG/develop/2009/03/20090326255187.shtml`,

mortality rate is a little lower than 2%.

Sow Insurance Program. Infectious diseases led to large fluctuations in pork production and pork prices in China. For example, in 2003 a bird flu epidemic caused a sharp decline in the production of live pigs; and in the second half of 2006, a fast-spreading deadly blue ear disease 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 hike in pork prices led to intense public complaint and concerns about food-price-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.[10] 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.[11]

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 underwrite the subsidized sow insurance and settle claims.[12] Local branches of PCC subsequently cooperated with the Bureau of Animal Husbandry (BAH) at local levels to collect premium payments from pig farmers who wanted to insure their sows.

Animal Husbandry Workers (AHWs). To make farmers in remote villages aware of the insurance, BAH at county and township levels mobilized various resources to spread the news about the subsidized insurance policy and its benefits in local radio and television. Probably a more important channel for the marketing of the insurance program is through the so-called Animal Husbandry Workers (*Xu Mu Yuan* in Chinese, AHW hereafter). In our study area every village has one AHW, who works for the BAH on a part-time contractor basis and is always a village resident. The AHWs serve as the bridge between formal institutions (specifically, the BAH) and rural villages for matters involving animal husbandry. AHWs are especially important in our study area where mountainous land and poor transportation infrastructure make it highly costly for outsiders to access the villages. Regular obligations of the AHWs include immunization for animals, technical assistance for farmers, and the monitoring of animal diseases and

`http://www.cangyuan.gov.cn/xxgk/jgzn/xzfzbm/2009-05-25/3188.shtml`, and
`http://pe.xxgk.yn.gov.cn/canton_model2/newsview.aspx?id=1152247`.

[10]The market price for a sow in our study area was around 1,500 Yuan.

[11]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.

[12]PCC is a majority state-owned company and it is listed in the Shanghai stock exchange.

epidemics. PCC, in cooperation with the BAH, mobilized the AHWs for the insurance program. The AHWs are asked to spread the word about the insurance policies, explain to and convince farmers about the policy's benefits, and act as a *coordinator* between the PCC and the farmers.[13] For example, the insurance policy requires that each sow be earmarked with a unique identification number in order to be eligible for insurance, and the earmark needs to be verified for claim purposes. The AHWs thus need to count and check all potentially eligible sows in the village and make earmarks. The farmers are also asked to contact their village AHW to initiate a claim when a sow dies. The AHW is also responsible for making sure that all relevant evidence of the loss is preserved until the official PCC claim agent arrives to complete the claim process.

Finally, the regular income for AHWs usually involves a small fixed wage from the local BAH (which is 15 Yuan per month in our study area) and fees for services they provide to farmers such as immunization, neutering and veterinarian treatments.[14] For their participation in the sow insurance campaign, local branches of PCC paid the AHWs an additional small *lump sum* to cover their food and transportation costs. In our field experiment, which we describe in detail below, we randomly assign the AHWs into different additional incentives for their performance in terms of the number of sow insurance purchases in their villages.

The Time Line of Raising Sows. The time frame for raising a sow is as follows. At around a month after a piglet is born, farmers have to make a choice of whether it will be raised for breeding purposes. Piglets not for breeding purposes are neutered. Neutering is believed to make the piglets grow faster and at least to some produce better-tasting pork.[15] A female piglet that is not neutered at that time turns into a sow at around 6 months of age, when she becomes sexually active and become pregnant. Pregnancy takes about 4 months (114 days).[16] The number of piglets born in each pregnancy varies, and it typically rises in the first three pregnancies, and reaches the peak in the 4th to 6th pregnancy and then the fertility declines from the 7th pregnancy. A sow is typically kept for about 4 to 6 years.

3 Experimental Design

3.1 Randomization of Incentive Schemes

The key to obtain a consistent estimate of the effect of insurance on farmers' production behavior is to isolate an exogenous source of variation in insurance coverage. In the context of sow insurance as described in Section 2, our idea is to randomize the assignment of AHWs into different incentive schemes for their performance measured by the number of sow insurance purchases in their villages. Different incentive

[13] The local BAHs held special training programs for the AHWs to understand the details of the sow insurance, as well as basic skills for effective promotion and persuasion.

[14] The average income of the AHWs from the service fees is about 3000 Yuan per year.

[15] The meat of a sow has almost no market value, regardless of whether it dies of sickness or is intentionally slaughtered; but some farmers still consume it within their family.

[16] Any unneutered female pigs older than 3-4 months of age are eligible to be covered by the insurance policy.

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 subsequent number of sows 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 is one of the poorest provinces in China and its economy 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.

Control Group, Low Incentive Group and High Incentive Group Villages. The local government of Jinsha County allowed us to run the experiment in 480 villages out of a total 580 villages within its jurisdiction.[17] We randomly assigned the AHWs of the 480 villages into three incentive schemes.[18] 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.[19] We refer to this group as the *high incentive group (HIG)* villages.[20]

Our choices of the fixed payment and the incentives are very attractive to the AHWs. As we mentioned in Section 2, PCC offers only a small lump sum payment to AHWs for their involvement in the sow insurance program; moreover, the regular monthly payment from the BAH to the AHWs is only 15 Yuan.

Time Line of the Study. The time-line of our study is as follows. The government-sponsored insurance program was initiated in July 2007, but was not implemented in our study area until the beginning of

[17]Based on information from the China Agricultural Census of 2006 (described below in Section 4), 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.

[18]See Section 4.4 for formal tests of the quality of randomization.

[19]We have twice as many HIG villages in our experiment as the control and LIG villages at the insistence of the local Bijie Prefecture government officials, who *a priori* believed that the high incentives we offer to the AHWs would lead to more insured sows in these villages.

[20]Our experimental design is related to the "encouragement design" as described in Duflo, Glennerster and Kremer (2007). The difference is that in our experiment, the incentives are provided to the AHWs, instead of the farmers. However, it is possible that some of our incentive payments to the AHWs are shifted to the farmers (see Section 3.2 for the discussion).

	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

November 2007. Our incentive experiment ran from November 21 to December 25.[21] Each AHW in our experimental village was informed about the assigned reward plan on November 20, 2007 with the cooperation from the local BAHs. The data on insured sows in each sampled village were collected in the week just after the experiment ended. After our experiment ended, we also obtained data of total sows in each of the villages from the local BAHs collected at two different time points, one as of the end of March 2008 and the other as of the end of June 2008.

3.2 Discussions

3.2.1 Experimental Design

Randomization at the Village vs. Household Level. As described above we implemented our randomization at the village level. An alternative would be to conduct an experiment where the randomization is at the household level. For example, 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. Self selection by 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, e.g., Townsend, 1994), 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.[22]

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

[21] 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.

[22] Angelucci and De Giorgi (2009) and Angelucci et al. (2009) made similar observations in their study of the indirect effects of Progresa transfers on family members.

village level information from the China Agricultural Census (CAC) conducted in early 2007.[23]

Randomized Incentives to the AHWs vs. Randomized Phase-in. At the village level, a most obvious alternative research design is randomized phase-in.[24] We initially pursued this idea, but the Bijie Prefecture government insisted that preventing some randomly selected villages from accessing the partially 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 7 below confirmed that incentives we provided to the AHWs led to substantial differences in the number of insured sows.[25] We explain below several potential channels through which our incentives to the AHWs may affect farmers' take-up of sow insurance.

3.2.2 Possible Channels for Performance Incentives to Affect Farmers' Take-up of Sow Insurance

We now describe four possible channels through which our performance incentives to the AHWs may affect farmers' take-up of sow insurance. Most of the channels below should not directly affect the farmers' subsequent decisions about sow production and thus will make our incentive assignment valid instruments for sow insurance. The fourth channel, however, may also directly affect sow production, invalidating our incentive assignment as IV for sow insurance; the additional evidence in Section 5.4 suggests that this channel is unlikely to be important.

1. **Information Channel.** Insurance is new to the farmers in this area and they are unlikely to know how it works. Our performance incentives to the AHWs can lead them to work harder to provide the farmers with information about insurance in general and the potential benefits of the sow insurance in particular, thus increasing take-up. It is possible that the AHWs may be led by the performance incentives to exaggerate the benefits of the sow insurance scheme in order to persuade the farmers to enroll. This type of exaggeration should not directly affect the farmers' subsequent sow production decisions.[26]

[23] See Section 4.2 below for details about the CAC.

[24] 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.

[25] 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.

[26] However, the AHWs can also exaggerate the risks of not purchasing insurance, by warning the farmers that there will be some pig disease outbreak, for example. However, this type of exaggeration may directly lead farmers to raise *less* sows subsequently, leading to an underestimate of the causal effect of insurance on production.

2. **Sharing the Performance Incentives with Farmers.** Our performance incentives to the AHWs may lead them to offer to share part of their incentive payments to the farmers in order to induce them to enroll in the insurance program. For example, the AHWs in the high incentive group villages receive 4 Yuan per insured sow. If the AHW shares a fraction of the 4 Yuan incentive payments to the farmers, more farmers may purchase the insurance because the kickback reduces the effective price of the insurance. However, this channel should not directly affect the farmers' subsequent sow production decisions.[27]

3. **Building Trust**. 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.[28] 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.[29] 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. This lack of trust can lead to low take-up of the subsidized sow insurance. Higher performance incentives may lead the AHWs to exert more effort to improve trust in the insurance scheme, changing farmers' subjective beliefs that their claims will be honored in the event of a loss. Our evidence in Section 5.4 can be interpreted as evidence for the importance of trust. This channel should not directly affect the farmers' subsequent sow production decisions.

4. **Promising to Lower Future Cost of Veterinary Services in Exchange for Insurance Enrollment.** Higher performance incentives to the AHWs may lead them to more likely to promise the farmers that they will deliver their on-site extension services such as immunization, neutering, and veterinary care etc. at lower costs, or more expediently, *only if* the farmers enroll in insurance. In this channel, high performance incentives to the AHWs lead to higher enrollment in the sow insurance, but at the same time also *directly* affects farmers' subsequent sow production decisions, thus invalidating the incentive assignments as the IV for insured sows. We argue, however, this is unlikely an important channel for our performance incentives to affect insurance enrollment. Exploiting a rare unexpected snow storm right after our experiment, we show in Section 5.4 that the effect of sow insurance on subsequent sow production is strongly positively related to the number of sow deaths during the snow storm, which is inconsistent with this channel.

[27]The kickback may change the farmers' production decisions through an income effect, which we can not distinguish from the effect of insurance. We assume that the income effect is small.

[28]This is an important difference between microinsurance and micro-credit programs, in which farmers receive money *from* the government or financial institutions up front. Trust of the farmers about the government or financial institution is not important for farmers' participation in micro-credit settings.

[29]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.

3.2.3 Potentials for Cheating, and Collusion Between the AHWs and the Farmers

The AHWs play important roles in our experimental study. They are responsible for enrolling the farmers' sows into the insurance scheme; they are also responsible to report the number of sows and pigs in their villages to the local Bureau of Animal Husbandry whose data we use in estimating the effect of sow insurance on subsequent sow production. One may be concerned that the subsidized insurance scheme, as well as our differential incentive assignments we provide to the AHWs, may lead them to "cheat" or to collude with the farmers for financial gains. We now list all of the prominent potential scenarios for cheating, and collusion between the AHWs and the farmers, and argue that they are unlikely to cause any problems in the interpretation of our estimated effect of insurance on subsequent sow production reported in Section 5.

1. **Collusion by the AHWs and the Farmers to Fleece the Insurance Company.** There are two possibilities under this type of collusion. First, an AHW and a farmer may fake a neutered pig as a sow and enroll her in the insurance, and then slaughter the pig for selling the pork and at the same time try to claim death benefits from the insurance company. This is hard to implement because the insurance company always send claim staff to the scene to verify all evidence, thus for this type of collusion to work, the claim agent from PCC must also be involved. Second, an AHW and a farmer may insure a sow and then fake its death in order to obtain payment from the insurance company. Note, however, that slaughtering a sow for insurance payment is a losing proposition for the farmer because the market value for a sow, which we noted in footnote 10 to be around 1500 Yuan, is higher than the 1000 Yuan insurance coverage amount. Importantly, even if such collusion schemes do occur, their occurrence should not depend on our incentive assignments to the AHWs.

2. **Collusion by the AHWs and the Farmers to Extract More Bonus Payments.** One may worry that the AHWs and the farmers may fake the number of insured sows in order to receive more bonus payments from us. However, as we detail below in Section 4, the actual numbers of insured sows we use to decide how much bonus an AHW will receive from us are obtained from the insurance company. Thus the AHW receives bonus payments from us only if the 12 Yuan premium is paid to the insurance company and an actual insurance policy is issued. Note that the 12 Yuan premium exceeds even our high bonus amount. Thus this collusion scheme is impossible to occur.

3. **Inaccurate Reporting of the Number of Sows by the AHWs.** One may also worry that the AHWs may not have the incentives to count carefully all the sows in their villages when they report to the local BAHs. This is indeed possible; however, note that such inaccurate reporting should be completely independent from our incentive assignments due to our randomization. In particular, the numbers of sows observed in March 2008 and June 2008, which we use as the measure for post-experiment level of production were collected after our incentives ended on December 25, 2007. Thus any mis-reporting, if exists, in March and June 2008 counting of the number of the sows should not be directly affected by our incentive assignments.

4 Data

4.1 Data from the Experiment

The data collected from our experiment is at the village level. For each village, we obtain the insurance company the total number of insured sows, including the identification number of the insured sows; we also collected information about a list of the AHW characteristics, including his/her name, age, gender, education and so on. We also record the total payment received by the AHW in each village.

4.2 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.[30] 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.

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.

[30]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/

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, we obtained the counts of the number of sows in each village tabulated by the BAH at the end of the third and fourth quarters 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 initiates a call to the insurance company to start the claim process, records the death, and collects claim evidence, in particular, the number on the ear of the sow that uniquely identifies the sow. The insurance company, PCC, 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.[31] 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.

4.3 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%).[32] 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.

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 take-up rate is about 78%. However, there is substantial variation in insurance take-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

[31] Thus, PCC's records of dead sows are a subset of the sow deaths recorded by the BAH.

[32] 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
Log House Value	9.83	.63	9.87	.61	9.75	.63	9.84	.63
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.

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 (see Section 5.4 for details). On average, 0.19 sows died in one village. However, the average number of sow deaths in the control villages is 0.08, lower than that of 0.10 in LIG villages and that of 0.28 in HIG villages. This is consistent with possible moral hazard among farmers with insured sows. In addition, the last two rows of Table 2 show that the number of sows in March 2008 and June 2008 continues to rise from the levels in September 2007.

4.4 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. We also run a different regression (without reporting) with each baseline covariate as dependent variable and treatment dummies as explanatory variable (standard errors are clustered at the township level), and find the similar result: the experimental treatments are orthogonal to the pre-test characteristics of villages.

4.5 Test for Parallel Trend Between the Control and Treatment Villages

Our IV estimator below of the effect of insurance access on production also relies on the assumption that there is no systematic difference in the trend of sow production between control and treatment villages. Here we report a formal test of this parallel trend assumption. We have the information about the number of sows measured at three different time points (December 2006, September 2007 and December 2007) before our experimental treatment and at two different time points (March 2008 and June 2008) after our experimental treatment.[33] Table 4 reports the fixed effect regression results when we regress the number of sows measured at the different points in time on time dummies and the interaction of the treatment dummies and the time dummies. The coefficient estimates for "LIG $\times$ September 2007", "LIG $\times$ December 2007", "HIG $\times$ September 2007" and "HIG $\times$ December 2007", namely the interactions between the treatment dummies and the two pre-experiment periods, are not statistically significant, while those for the interaction between HIG treatment dummies and post-experiment period dummies are significant at close to 5% level. These suggest that there is a parallel trend between the

[33]The sow counts for December 2007 were collected right when our experiment was conducted. As such, we treat it as the pre-experiment counts.

Variables	Linear Probability		Probit		Logit	
	LIG	HIG	LIG	HIG	LIG	HIG
No. of Sows in Dec. 2006	-.0017	.0004	-.0110	-.0027	-.0149	-.0038
	(1.685)	(.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.

control and treatment group villages before our experiment, but there is a different trend between the control and the HIG groups after our experiment.[34]

5 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 5.1 OLS results where we regress the number of sows measured in March 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 5.3, 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.

In our paper, we estimate the overall effect of insurance on sow production at the village level. The overall effect we estimate is the sum of both the extensive or the intensive margins, where the extensive margin refers to the previous sow farmers increasing the number of sows, and the intensive margin refers to the production of new sow farmers. We do not have the necessary household level data to distinguish the two margins.

5.1 Results from the OLS Regressions

Table 5 reports results from the following OLS specifications:

$$\mathrm{Y}_i = \alpha_0 + \alpha_1 \textsc{Insured\_Sows}_i + \alpha_2 \textsc{Sows2006}_i + \textsc{Township\_Dummies} + \epsilon_i, \quad (1)$$

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.[35] Robust standard errors clustered at the township level are used to calculate the t-statistics reported in parenthesis in Table 5.

In order to examine the causal effect of the access to the formal insurance on sow ownership, ideally we need a proper measure of insurance access. However, such a measure of insurance access is unavailable

[34]In alternative specifications, we grouped the LIG and HIG villages as a single "Treatment" group. We found that the coefficients for the interaction terms of "Treatment × March 2008" and "Treatment × June 2008" are significant at close to 5% level, while the coefficients for "Treatment × September 2007" and "Treatment × December 2007" are insignificant.

[35]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	Number of Sows
September 2007	8.18***
	(3.74)
December 2007	14.41***
	(5.80)
March 2008	20.52***
	(8.21)
June 2008	24.66***
	(9.87)
LIG × September 2007	4.33
	(1.39)
LIG × December 2007	1.01
	(0.28)
LIG × March 2008	3.41
	(0.96)
LIG × June 2008	3.39
	(0.96)
HIG × September 2007	2.03
	(0.75)
HIG × December 2007	2.93
	(0.97)
HIG × March 2008	5.42*
	(1.80)
HIG × June 2008	6.08**
	(2.03)
Constant	15.85***
	(20.96)
R^2	.341

Table 4: Testing for the Parallel Trend in the Number of Sows Between the Treatment and Control Villages.

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 omitted time period is December 2006; (3) *, **, *** denote significance at 10%, 5% and 1%, respectively.

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 Dec. 2006			.2975***			.3178***
			(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 5: 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.

in our data set. As a result, we use the number of insured sows in each village as a proxy for accessibility.[36]The number of insured sows contains information on availability, but it may be contaminated by other factors. In the next section, we will report the reduced-form effects of the treatments both on insured sow ownership and total sow ownership

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.

5.2 Results from Reduced-Form Regressions

Table 6 report the reduced-form regression results, examining how the treatment dummies affect the subsequent sow productions. We see that high-incentive groups lead to higher level of sows both in March

[36]In order to measure the access or take-up of the insurance plan, an alternative and seemingly natural measure will be the percentage of sows insured. However, we have controlled the number of sows in the end of 2006 for all regressions, and the estimated coefficient on the number of insured sows should be interpreted as the effect of the insurance on the increase of sows raised at the village. For robustness check, we also used percentage of sows insured over the sows in 2006 as the measure for the access, the basic results remain qualitatively similar.

Variables	Number of Sows in March 2008 (1)	(2)	Number of Sows in June 2008 (3)	(4)
Low Incentive Group	4.828	7.030*	5.119	7.457*
	(1.15)	(1.97)	(1.08)	(1.71)
High Incentive Group	9.133***	7.404*	9.874	8.038*
	(3.38)	(1.84)	(1.63)	(1.75)
No. of Sows in Dec. 2006		1.120***		1.189***
		(6.71)		(6.81)
Constant	1.120***	13.358***	22.241***	14.116***
	(6.71)	(3.14)	(4.25)	(2.88)
Township Dummies	Yes	Yes	Yes	Yes
Adjusted-R^2	.274	.546	.293	.547

Table 6: The Effect of Incentive Group Assignments on Subsequent Sow Production: Reduced-Form 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.

and June, 2008 than low-incentive groups, and two incentive groups generate more sows than the control group villages. The coefficients on two incentive groups are both statistically significant at 10 percent level.

5.3 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 (1).

First-Stage Results A valid instrument variable for INSURED_SOWS in Equation (1) 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 8 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).

Now we report the first-stage result that shows that the second requirement for the IV to be valid is also satisfied. Table 7 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),

Variables	Number 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 7: 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.

controlling for the number of sows measured in December of 2006 and a set of Township dummies.[37] 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. 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.

Second-Stage Results. Table 8 reports the second-stage regression results. It shows that when we only use the exogenous variation in insurance coverage induced from the variations in 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 5; 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 7, we know that the low and

[37] 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.

Variables	Number of Sows in March 2008 (1)	(2)	(3)	Number 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			.549**			.574**
Dec. 2006			(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 8: 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.

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 8, 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.[38] 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 5 and the IV results in Table 8. This most likely reflects the natural constraint that turning young female pigs into sows takes some time.

5.4 The Effect of an Unexpected Snow Storm

As we described in Section 3.2.2 (the fourth channel), to the extent that the AHWs responded to our performance incentives by promising to lower future cost of veterinary services in exchange for the farmers' insurance enrollment, our estimated effect of insurance on subsequent sow production will be upward biased. Here we provide additional evidence to argue that this may not be a serious issue.

[38]Note that these magnitudes are remarkably close to the reduced-form impact of the incentive treatment group on the number of sows in March and June of 2008, as reported in Table 6.

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-January and ended until mid-February, 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.[39] News report indicated that there were a total of 5,973 sows that died during the storm in Guizhou province. This unexpected event offers us a rare opportunity to test whether the incentives facing the AHWs might affect farmers' sow production directly, as discussed above, rather than through the insurance.

The ice and snow storm of such a scale was totally unexpected, and its damage on the villages in our sample were also random. If the insurance purchases are mainly driven by the AHWs promising to offer cheaper and/or additional veterinary services to the farmers, we would see the positive effect of insurance coverage on sow production, but we would not see any significant effect of the interaction between the severity of the storm and insured sows. The presence of the significant effect of the interaction will cast doubt on the hypothesis that the effect of insurance coverage is mainly driven by the channel of lowering the future cost of veterinary services in exchange for the farmers' insurance enrollment.

Results. Table 9 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 8 except that we add "No. of Sow Deaths in Snow Storm" in Columns (1) and (4), and additionally the interaction of "No. of Insured Sows" and "No. of Sow Deaths in Snow Storm" in Columns (2) and (5).

Table 9 provides some interesting results. We see that without any other interactions, villages that lost more sows to the storm actually ended up with more sows just a few months later. Since there is a mechanical negative effect, this strongly suggests some large positive offset. Once we include interactions with insurance take-up the coefficients on sow deaths in columns 4 and 8 are indeed negative. And the coefficients on the interaction terms are positive and significant in all specifications, and in some cases significant at 5 percent level. Also note that in specification (5), we found that the coefficient on "No. of Sow Deaths in Snow Storm" is negative but insignificant. This result suggests that even though there may be some pathways for the incentives to affect sow production directly, they are likely unimportant or at least not dominant.

One potential concern is that the positive coefficient estimate for the interaction term of "No. of Insured Sows" and "No. of Sow Deaths in Snow Storm" may be due to a wealth effect, namely, villages with more insured sows are likely to receive more settlements from the insurance company. To address this concern, we add the log of the total housing values in the villages in specifications reported in Columns (3) and (6). Note that this does not change at all the coefficient estimate on the interaction term of "No. of Insured Sows" and "No. of Sow Deaths in Snow Storm."

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

Variables	(1) OLS	(2) OLS	(3) OLS	(4) IV	(5) IV	(6) IV
Panel A: Number of Sows in March 2008						
No. of Insured Sows	1.01***	.893***	.896***	.542**	.507*	.427
	(3.49)	(12.20)	(12.29)	(2.08)	(1.78)	(1.44)
No. of Sows in Dec. 2006	.357***	.404***	.398***	.754***	.707***	.768***
	(4.19)	(3.56)	(3.72)	(3.45)	(3.24)	(3.29)
No. of Sow Deaths in Snow Storm	4.241**	.297	.048	5.485*	-.629	-.989
	(2.16)	(.22)	(.03)	(1.77)	(.39)	(.55)
No. of Insured Sows ×		.109**	.109**		.159**	.169**
No. of Sow Deaths in Snow Storm		(2.25)	(.2.07)		(2.38)	(2.40)
Log Housing Values			2.59			1.381
			(.04)			(.42)
Constant	9.45***	11.27***	-14.0	13.83***	15.21***	2.736
	(3.49)	(4.79)	(.53)	(3.38)	(3.34)	(.08)
Adjusted-R^2	.7008	.7229	.725	.6440	.6896	.6764
Panel B: Number of Sows in June 2008						
No. of Insured Sows	1.108***	.979*	.982***	.577*	.538	.441
	(10.64)	(1.90)	(10.72)	(1.76)	(1.51)	(1.19)
No. of Sows in Dec. 2006	.380***	.432***	.424***	.830***	.778***	.851***
	(3.59)	(3.08)	(3.19)	(3.22)	(3.02)	(3.10)
No. of Sow Deaths in Snow Storm	4.113*	-.254	-.565	5.525	-1.311	-1.762
	(1.83)	(.15)	(.32)	(1.59)	(.65)	(.80)
No. of Insured Sows ×		.121*	.121*		.178**	.190**
No. of Sow Deaths in Snow Storm		(1.90)	(1.93)		(2.18)	(2.22)
Log Housing Values			3.24			1.835
			(1.07)			(.49)
Constant	11.44***	13.45***	-18.03	16.41***	17.95***	1.263
	(3.66)	(5.14)	(.62)	(3.44)	(3.41)	(.03)
Adjusted-R^2	.6842	.7066	.7096	.6236	.6707	.6559

Table 9: The Effect of Sow Deaths During the Snow Storm 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.

A Potential Interpretation of the Results in Table 9. How should we interpret the positive and significant effect on the interaction between the storm and insurance reported in Table 9. We argue that this positive effect can be interpreted by the trust-building behind the insurance purchase and claim settlement (see the third channel in Section 3.2.2). When farmers do not have complete trust on whether the insurance product is genuine, the insurance policy itself becomes a risk.[40] 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.[41]

From this perspective, 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.[42] 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.[43] Thus indeed, Table 9 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.

6 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

[40] See Karlan et al. (2009) for a model and test for the role of network-based trust as social collateral in obtaining informal borrowing.

[41] 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).

[42] However, when farmers have access to formal insurance that does not adjust premium for the changes in perceived death risks, the effect on future sow production is ambiguous.

[43] 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, partially 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.

assignment as the instrumental variable for insurance access to recover the causal 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 suggests that microinsurance may be as important as microfinance in poverty alleviation, and microinsurance can supplement and strengthen the effects of microfinance by protecting the farmers from the inherent risk of entrepreneurial activities.[44] Our experimental design, as well as collaborating evidence from the effect of an unexpected snow storm, leads us to believe that our estimated effect of insurance on subsequent sow production is causal.

Our evidence from the effect of an unexpected snow storm reported in Section 5.4 also suggests 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.

It is important to note that in this study we are unable to address several important questions due to data limitations. First, how does sow insurance affect the *other* production decisions of the farmers? Do they substitute away from non-insured, and thus more risky, produces? Or do their better ability to insure with respect to sow production also enhance their production of other non-insured produces? Second, how would providing access to microinsurance complement the effectiveness of micro-credit programs?[45] Third, how does the increase in sow production affect the farmers' well-beings as measured by consumption, for example? There are fruitful avenues for future research.

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[44]It is important to note that the sow insurance studied in our paper only insures farmers against loss from the death of the sows. To the extent that the value of raising sows also depends on the price of the pork and thus the price of piglets, whose fluctuations are not insured by the sow insurance considered in this study, even the sow the farmers who purchased our insurance still face significant risks. We would expect that the effect of insurance on sow production will be even larger if the farmers also have access to additional insurance against price fluctuations.

[45]See Gine and Yang (2007) for a study of whether the provision of rainfall insurance induces farmers to borrow to invest in new varieties. They found, surprisingly, that the the take-up of farm loans that are bundled with a rainfall insurance is actually 13 percentage points lower than the take-up for a standard agricultural loan without insurance. See also Karlan, Kutsoati, McMillan and Udry (2010) for a study in Ghana to understand how farmer risk aversion affects investment decisions, and whether a loan product with an insurance component encourages farmers to take and benefit from credit.

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