

THE EFFECT OF MICROINSURANCE ON ECONOMIC ACTIVITIES: EVIDENCE FROM A RANDOMIZED FIELD EXPERIMENT

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Abstract—We report results from a large, randomized field to study how access to formal microinsurance affects production and economic development. We induce exogenous variation in insurance coverage at the village level by randomly assigning performance incentives to the village animal husbandry worker who is responsible for signing farmers up for the insurance. We find that promoting greater adoption of insurance significantly increases farmers' sow production, and this effect seems to persist in the longer run; moreover, the increase in sow production in response to the sow insurance does not seem to be the result of the substitution of other livestock.

I. 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 is one of the binding constraints that prevent potential entrepreneurs among farmers from obtaining the necessary capital to start or expand their business, forcing them to 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.¹

International aid agencies, nongovernmental organizations, and profit or nonprofit private banks have devoted large amounts 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, including East Timor, Bosnia, and even many poor neighborhoods in the United States.² Academically, a large

empirical literature has documented the success of microfinance programs, and a theoretical literature has also been developed to explain its success.³

Surprisingly, there has been much less effort, both practically and academically, devoted to providing 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 may cause a spurious correlation between insurance coverage and agricultural output.

To overcome this 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 detail in section III, creates an exogenous source of 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.⁴

Specifically, we report results from a large, randomized field experiment conducted in southwestern China in the context of a subsidized 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 promoting greater adoption of insurance significantly increases farmers' tendency to raise sows, and the short-run effect of sow insurance on sow production seems to persist in the longer run. Moreover, we find that the increase in sow production in response to the sow insurance does not seem to be the result of the substitution of other livestock, such as pigs, sheep, or cows.

³ De Aghion, Armendariz, and Morduch (2005) provide a comprehensive review. Banerjee et al. (2010) report results from a large-scale randomized evaluation of the impact of introducing microcredit on various measures of economic activities in a new market.

⁴ 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 methods especially in development economics.

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¹ See Morduch (1995) for a discussion about how farm households in developing countries may seek to smooth their consumption by altering their methods of production.

² Yunus (2001) documents the origins and development of the Grameen Bank. Robinson (2001) provides an account of its replications around the world.

A small literature in agricultural economics has examined the effect of federal crop insurance on farmers' decisions using nonexperimental data.⁵ However, 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.⁶ Related to our study, Mobarak and Rosenzweig (2012) studied the demand for, and the effects of, offering formal index-based rainfall insurance through a randomized experiment in an environment with existing informal risk sharing in rural India. Karlan et al. (2013) also conducted randomized field experiments in northern Ghana in which farmers were randomly assigned to receive cash grants or opportunities to purchase rainfall index insurance, or a combination of the two. They found that insurance access leads to significantly larger agricultural investment and riskier production choices. Similarly, Cole, Gine, and Vickery (2013) randomly assigned farmers in Indian villages to rainfall insurance or cash payments and studied differences in subsequent production decisions during the monsoon between these two groups. They found that insurance provision has little effect on total agricultural investments but causes significant shifts in the composition of those investments, particularly among more educated farmers. The randomizations of treatment and control in all of the above studies are at the household level; in contrast, we randomize at the village level (see section III).

Also related to our study, several papers have studied the determinants of the demand for microinsurance. Gine, Townsend, and Vickery (2008) found that for a rainfall insurance policy offered to small farmers in rural India, the 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 households 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, Gine, Tobacman, et al. (2013) documented low levels of rainfall insurance take-up and then conducted field experiments to understand why adoption is so low. Their experimental results demonstrated that the high price of the insurance and credit constraints of the farmers are important determinants of insurance adoption, but they also found evidence that the endorsement from a trusted third party about the insurance policy significantly increases the insurance take-up. In this paper, we also find corroborative evidence suggesting the importance of trust for insurance take-up. Cai (2013) studied the role of social networks on the decisions to purchase crop insurance in rural China for rice farmers. These 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) tested 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.⁷ Our evidence 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 with 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 II, we provide the institutional background for hog production and the insurance program for sows introduced in China in 2007. In section III, we describe and discuss our experimental design. In section IV, we describe the data sets and provide summary statistics. In section V, we present and discuss our experimental result that sow insurance significantly affects farmers' decision to raise sows in subsequent periods. Finally, in section VI, we conclude. Supplemental tables and omitted details are collected in an online appendix.

II. Background

Pork is an important part of the Chinese diet; about 52 million tons of pork (valued at 644.25 billion yuan or 101.46 billion U.S. dollars) were produced in China in 2006.⁸ In China, pigs are mainly raised in rural households' backyard as a sideline business; large-scale hog farms are unusual, especially in its mountainous southwestern regions. The small-scale and scattered nature of hog raising exposes farmers to a high incidence of pig diseases, and as a result, mortality rates for pigs and sows are quite high.⁹

Though there are 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 2009.¹⁰ In Yunnan Province, which neighbors our study area of Guizhou Province, the mortality rate for insured sows was estimated to be about 2.04% to

⁷ Rosenzweig (1988), Rosenzweig and Stark (1989), Rosenzweig and Wolpin (1993), Udry (1994), and Lim and Townsend (1998), among others, studied the mechanisms through which villagers achieve the informal risk sharing.

⁸ The hog industry in China was valued at 644 billion yuan in 2006, accounting for 48.4% of the total livestock industry (Wang & Watanabe, 2007).

⁹ 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.

¹⁰ See <http://www.chinabreed.com/PIG/develop/2009/03/20090326255187.shtml>.

⁵ See, for example, Horowitz and Lichtenberg (1993), Goodwin, Vanderveer, and Deal (2004), and O'Donoghue, Key, and Roberts (2007).

⁶ Innovations for Poverty Action (2009) reviews the existing and ongoing microinsurance field experiments. Most of the experiments focus on the low take-up rate of insurance products.

2.6%.¹¹ In our own data set described below, the sow mortality rate is 1.92%.

A. *The Study Area*

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 (about US\$387) in Bijie Prefecture and 2,853 yuan (US\$449) in Jinsha county. Bijie prefecture has a population of 7.38 million, and over 93% of its terrain is either highlands or mountains; roads are in condition.

Pig raising is an important source of income for farmers in our experimental area. In 2007, the market price of a sow was about 1,500 yuan, while the annual per capita income in the experimental area was 2,853 yuan. A sow's market value accounts for over 10% of a household's income (since the average household has about five people). According to an industry analysis in 2009, the return from raising a porker pig (one raised for its meat) was about 17% around 2007, which was relatively high.¹²

B. *The Sow Insurance Program*

Infectious diseases have led to large fluctuations in pork production and 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, causing pork prices to be more than 60% higher in June 2007 than in June 2006. Due to the importance of pork in the Chinese diet, the dramatic hike in pork prices led to intense public complaints and concerns about food-price-driven inflation. As a result, the Chinese government decided to intervene and offer a government subsidy to increase pork supply. One of these government measures was to offer government-subsidized insurance on sow deaths.¹³ In July 2007, the Ministry of Finance initiated a plan specifically to subsidize 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 (about US\$157) in the event of death are offered at an annual premium of 60 yuan, including the estimated administrative cost of 40 yuan per policy.¹⁴ However, the central and local governments combined would pay 80% (48 yuan) of the premium, so the farmer pays only 12 yuan premium for the insurance.

¹¹ These statistics can be found at <http://www.chinabreed.com/PIG/develop/2009/03/20090326255187.shtml>, <http://www.cangyuan.gov.cn/xxgk/jgzx/xzfzbm/2009-05-25/3188.shtml>, and http://pe.xxgk.yn.gov.cn/canton_model2/newsview.aspx?id=1152247.

¹² See Finance and Industry Institute of Northeast Securities (2009).

¹³ Other government measures include direct subsidy and low-interest loans for pig farmers.

¹⁴ Recall that the market price for a sow in our study area was around 1,500 yuan at the time of our study.

The policy covers deaths of sows caused by major diseases, natural disasters, and accidents.¹⁵

The Property and Casualty Company (PCC) of the PICC was designated by the central government as the sole insurance company to underwrite the subsidized sow insurance and to settle claims. Local branches of PCC subsequently cooperated with the local Bureau of Animal Husbandry (BAH) to collect premium payments from pig farmers who decided to insure their sows.

C. *Animal Husbandry Workers*

BAH at county and township levels mobilized various resources to increase the awareness of the subsidized insurance policy and its benefits through local radio and television broadcasting, but the more important channel for the marketing campaign was through the so-called animal husbandry workers (AHW). 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.¹⁶ AHWs are especially important in our study area because the poor transportation infrastructure makes it highly costly for outsiders to access the villages.

PCC, in cooperation with the BAH, mobilized the AHWs 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.¹⁷ 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 an insured 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.

The regular income for AHWs usually includes a small fixed wage from the local BAH (15 yuan per month in our study area) and fees for services they provide to farmers, such as immunization, spaying and neutering, and other

¹⁵ Major diseases covered by the insurance are septicemia, blue tongue, scrapie, swine fever, hyopneumonias, swine erysipelas, porcine reproductive and respiratory syndrome virus, porcine epidemic diarrhea, streptococcus suis, and foot and mouth disease. Natural disasters covered by the insurance are typhoon, tornado, rainstorm, lightning stroke, earthquake, flooding, hailstorm/snowstorm, debris flow, and mountain landslide. Accidents include fire, explosion, building collapse, and falling parts or articles from aircraft and other flying objects.

¹⁶ Regular obligations of the AHWs include immunization for animals, technical assistance for farmers, and the monitoring of animal diseases and epidemics.

¹⁷ The local BAHs held training sessions for the AHWs to understand the details of the sow insurance and provide basic skills for effective promotion and persuasion, such as highlighting the convenience of the insurance purchase and claim settlement.

veterinarian treatments.¹⁸ 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 in section III, we randomly assign the AHWs into different additional incentives for their performance in terms of the number of sow insurance purchases in their villages. We should emphasize that when we introduced the experiment in our meetings with the AHWs, they were simply told that we would offer them additional incentive packages. We left no hint that we would examine the effect of the insurance or that we would run the experiment again in future.

D. Farmers' Decision Regarding Raising Sows

Around a month after a female piglet is born, farmers have to decide whether it is to be raised for breeding purposes. Female piglets not for breeding purposes are spayed at that time; otherwise, the piglet becomes a sow at around 6 months of age, when she becomes sexually active and can start breeding.

Not spaying a female piglet (and thus keeping it fertile) involves significant costs and benefits. Spaying a piglet enables it to grow faster and produces, to most Chinese consumers, better-tasting pork.¹⁹ Thus, on the cost side, not spaying a female piglet (thus turning it into a sow) leads to slower growth, less tasty and thus less valuable meat, and higher feeding costs. The benefit for raising sows is to breed piglets, which can be sold in the market. A sow's pregnancy takes about 4 months (114 days).²⁰ The number of piglets born in each pregnancy varies, and it typically rises in the first three pregnancies, reaching a peak in the fourth to sixth pregnancy. A sow is typically kept for about four to six years and then slaughtered when its fertility rate drops to a low level. It is rare for farmers to keep a sow for its natural life span. The insurance does not cover a sow's natural death or if it is slaughtered by the farmer.

Recall that farmers need to contribute a 12 yuan premium toward an insurance policy that pays out 1,000 yuan in the event of sow death due to the covered risks (estimated to occur at about 2% probability). Thus, due to the government subsidy, the insurance is actuarially favorable for the farmers for the risks covered by the insurance. However, it should be emphasized that even with the insurance, farmers are still exposed to significant residual risks when they decide on whether to spay a female piglet or to turn it into a sow.

First, the risks covered by the insurance, as described in note 15, are only a subset of risks that may cause sow deaths. For example, the risk that a sow will die in the birth process is not covered. Second, farmers also face the market risks.

¹⁸ The average income of the AHWs from the service fees is about 3,000 yuan per year.

¹⁹ The meat from a sow has virtually no market value; government regulation prohibits it from being sold in the market.

²⁰ Any unsprayed female pigs older than 3 or 4 months are eligible for the coverage of the insurance policy.

The price for piglets—the output of a sow—is volatile. For instance, a piglet in our study area, Guizhou Province, was priced at 22.50 yuan per kilogram in November 2007 when our experiment was conducted, and then increased to 33.49 yuan per kilogram in May 2008; it dropped to 22.75 yuan per kilogram in November 2008.²¹ That is, within a year, we saw an increase of 49% in the piglet price in the first half of the year and then a drop of 32% in the second half. Such market risks are not covered by the insurance.

Third, even ignoring the volatility of the piglet prices, it will not be worthwhile for farmers to seek the gain of the 8 yuan subsidy per sow by purchasing the insurance. Turning a female piglet into a sow in order to be eligible for the insurance leads to a slower growth, less tasty meat, and more feeding costs. The pork price was about 28 yuan per kilogram in Guizhou from November 2007 to June 2008. The 8 yuan subsidy can be offset by a mere 0.3 kilogram difference in the quantity of meat production between a spayed female pig and a sow. A typical female pig weighs about 100 to 150 kilograms before being sold in the market.

Finally, we should point out the 8 yuan subsidy is received only in expectation. Farmers have to pay a 12 yuan premium upfront, and the insurance is valuable only if the sow dies for the covered causes. But the market value of the sow was about 1,500 yuan during the period of our experiment, and farmers still suffer significant residual financial loss if an insured sow dies.

III. Experimental Design

A. 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 II, 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 schemes are expected to generate different insurance coverage 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 (IV) for village-level insurance coverage and identify the causal relationship between insurance and production.

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

²¹ The price information is obtained from China Information Network on Husbandry at <http://www.caaa.cn>.

TABLE 1.—EXPERIMENTAL DESIGN

	Control Group	Treatment Groups	
		Low-Incentive Group	High-Incentive Group
Fixed reward	50 yuan	20 yuan	20 yuan
Incentives	None	2 yuan/insured sow	4 yuan/insured sow
Number of villages	120	120	240
Number of covered townships	27	27	27

580 villages within its jurisdiction.²² These 480 villages are located in 27 townships. We randomly assigned the AHWs of the 480 villages into three incentive schemes. The incentives are summarized in table 1. In the first group of 120 villages, the AHWs were offered a fixed reward of 50 yuan (US\$7.87) 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 were 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 were offered a 20 yuan fixed reward and an additional payment of 4 yuan for each insured sow.²³ We refer to this group as the high-incentive group (HIG) villages.²⁴ As shown in the last row in table 1, in each of the 27 townships, there are villages assigned to all three experimental groups.

Our choices of the fixed payment and the incentives are very attractive to the AHWs. As we mentioned in section II, 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 government-sponsored insurance program was initiated in July 2007 but was not implemented in our study area until the beginning of November 2007. Our incentive experiment ran from November 21 to December 25, 2007.²⁵ Each AHW in our experimental village was informed about the assigned incentive plan on November 20, 2007, with the cooperation of the local BAHs. The data on insured sows in each sampled village were collected in the week immediately after our experiment. After our experiment, we also obtained data on all sows in each of the villages from the local BAHs collected at two different times—one

²² Based on information from the China Agricultural Census of 2006 (described in section IV), 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.

²³ 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 offered to the AHWs would lead to more insured sows in these villages.

²⁴ Our experimental design is related to the “encouragement design” as described in Duflo et al. (2007). The difference is that in our experiment, the incentives are provided to the AHWs, not to the farmers.

²⁵ December 25, 2007, was the cut-off date for the 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 date.

as of the end of March 2008 and the other as of the end of June 2008.

B. Discussions

Randomization at the village versus household level. 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 unselected 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 households that were not offered the insurance option but learned about it the neighbors and would like to be insured as well. Self-selection by households would contaminate the randomization in the experimental data.

Another serious shortcoming of household-level randomization is that there is substantial evidence that villagers in the same village are likely engaged in informal risk sharing (Townsend, 1994), so randomizing insurance access at the household level may actually lead to an underestimate of the true effect of insurance on production due to the potential risk shifting from households without access to formal insurance to those with access.²⁶

For us, randomizing at the village level had the added benefit that we do not have to collect detailed information about each household because we fortunately had access to the detailed preexperiment village-level information from the China Agricultural Census (CAC) conducted in early 2007 (see section IVB).

Randomized incentives to the AHWs versus randomized phase-in. At the village level, a most obvious alternative research design is randomized phase-in (Miguel & Kremer, 2004).²⁷ We initially pursued this idea, but the Bijie Prefecture government insisted that preventing some randomly selected villages from accessing the partially subsidized sow

²⁶ Angelucci and De Giorgi (2009) and Angelucci et al. (2009) made similar observations in their study of the indirect effects of PROGRESA cash transfers to family members in Mexico. They show that these transfers to households eligible for them indirectly increase the consumption of ineligible households living in the same villages because ineligible households receive more gifts and loans from eligible households.

²⁷ Duflo et al. (2007) provide a general discussion of different field experiment designs.

insurance was impractical. We then debated alternative ways to randomly generate differential access to insurance. We believe that by randomly allocating incentives to the AHWs, we can generate de facto differential access to the insurance product in different villages. Indeed the first-stage result (reported in table 5) confirmed that the incentives we provided to the AHWs led to substantial differences in the number of insured sows.²⁸

C. AHWs' Performance Incentives and Farmers' Take-Up of Sow Insurance

We now describe several channels through which our performance incentives to the AHWs may affect farmers' take-up of sow insurance, and discuss several potential threats to the validity of our incentive assignment as instruments for sow insurance.

Information channel. Insurance was new to the farmers in this area and they were unlikely to know how it works. Performance incentives to the AHWs could 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 might be led by the performance incentives to exaggerate the benefits of the sow insurance scheme in order to persuade the farmers to enroll. However, this type of exaggeration should not directly affect farmers' subsequent sow production decisions.

Sharing the performance incentives with farmers. The AHWs might offer to share part of the performance 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 shared a fraction of the 4 yuan incentive payments with the farmers, more farmers might 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, except possibly through an income effect, which we assume to be small.²⁹

Building trust. In the microinsurance setting, farmers are required to pay their insurance premium up front (although just a small fraction of the entire premium in our case) before receiving any potential benefit from this policy in the event of a sow death.³⁰ As a result, farmers may be seriously concerned about whether they will be able to get the payment as

promised in the insurance contract if covered contingencies occur and whether the government would continue to offer and subsidize similar sow insurance in the future.³¹ Importantly, if a local government fails to deliver its promises in the contract, there is virtually no way for farmers to sue the government in court. This lack of trust can lead to low take-up of the subsidized sow insurance. Our performance incentives may lead the AHWs to exert more effort to improve the farmers' trust in the insurance scheme and change their subjective beliefs that their claims will be honored in the event of a loss. Our evidence in section VD can be interpreted as evidence for the importance of trust. This channel should not directly affect the farmers' subsequent sow production decisions.

We now briefly discuss several prominent mechanisms that may potentially threaten the instrument validity. We argue that they are unlikely to cause major problems in the interpretation of our estimated effect of insurance on subsequent sow production reported in section V.

The first threat is that higher performance incentives to the AHWs may lead them to promise the farmers that they will deliver their on-site extension services such as immunization, neutering or spaying, and veterinary care 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, they also directly affect farmers' subsequent sow production decisions, thus invalidating the incentive assignments as the IV for insured sows.³² We discuss in section VD evidence from a rare unexpected snowstorm that occurred right after our experiment, which suggests that this is unlikely an important channel for our performance incentives to affect insurance enrollment.

A second concern is that the random incentive assignments we gave to the AHWs in our experiment may directly affect how the sows in March and June 2008, which we use as the measure for the postexperiment level of production, would be counted. This concern is unwarranted, however, because the AHW and the local BAH staff counting the number of sows and pigs in each village are typically not the same person. The local BAH has its own staff to conduct regular agricultural statistics (including counting the sow population).³³ Moreover, the counting of the numbers of sows in March and

about the government or financial institution is not important for farmers' participation in microcredit settings.

²⁸ 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 reasons to ask them for money and sometimes even cheat them into paying unnecessary fees. Indeed, though we did not anticipate it, the government decided not to offer such sow insurance to farmers in 2008 due to a substantial drop in pork prices during May and November 2008.

²⁹ Similarly, our IV strategy will be threatened if the villagers perceive the more aggressively marketed insurance as a sign that the government would offer other types of incentives related to sow production.

³⁰ Our experimental design did not allow us to distinguish the income effect from the effect of insurance.

³¹ See articles 5 and 20 in "Regulations for Chinese Agricultural Statistical Data Collection," http://www.gov.cn/zwgk/2006-08/31/content_374576.htm) for reference. These articles state that the statistical staff member is typically the head or the accountant of the village.

²⁸ With a random phase-in, villages either have or do 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.

²⁹ In contrast, in microcredit programs farmers receive money from the government or financial institutions up front. Trust of the farmers

June 2008 occurred after our incentives ended on December 25, 2007.

A third concern is that the AHWs and the farmers may collude to either fleece the insurance company or extract more bonus payments from us. For example, an AHW and a farmer may fake a spayed pig as a sow and enroll her in the insurance and then slaughter the pig but at the same time try to claim death benefits; or an AHW and a farmer may insure a sow and then fake its death to obtain payment from the insurance company. These are difficult to implement, however, because the insurance company always sends 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. Another worry is 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 in section IV, the actual number of insured sows we used to decide how much bonus an AHW would receive from us were 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 when an actual insurance policy is issued. Because the 12 yuan premium exceeds even our high bonus amount, this collusion scheme is not profitable. Importantly, however, any of the potential collusions we have mentioned, if they occur at all, would weaken our instruments and make it harder for us to find the effects of insurance on subsequent sow production.

IV. Data

A. Data from the Experiment

The data collected from our experiment are at the village level. For each village, we obtained from 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 name, age, gender, and education. We recorded the total payment received by the AHW in each village.

B. Other Data Sets

We matched 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 BAH.

China Agricultural Census of 2006. The CAC was conducted by the National Bureau of Statistics of China between January and February 2007. It covered 250 million rural households in 640,000 villages and 35,000 townships in China, and it collected detailed information about agricultural production and services in farming, forestry, husbandry, and fishery as of the fourth quarter of 2006.³⁴ We obtained

³⁴For more detailed information about this census, see “The Action Plan of the Second National Agricultural Census,” <http://www.stats.gov.cn/zgnypc/>.

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 (e.g., paved roads, water treatment facility, schools), and village government financial information. The main component of the CAC data, however, is collected at the household level. Household heads are asked to enter information for every member of their household. We observed from the household component detailed household information, including how many individuals resided in the household, their relationship to the household head, and their age and gender; 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 who received government subsidies; and engagement in various agricultural activities, including the number of sows and number of pigs raised in the household. We aggregated up the relevant household data to the village level and then matched it, 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 BAH. We obtained data from the agricultural statistics collected by the local 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 animal. The insurance company, PCC, then sends its claim staff to check and confirm the death and its causes. If the death is confirmed to be covered by the policy, the company makes compensation payment to the farmer. 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.

C. Descriptive Statistics

Table 2 presents the basic summary statistics of the key variables used in our analysis for the whole sample and separately by experimental groups. An observation is a village that participated in our experiment. In the last column of table 2, we report the p -value for the hypothesis that the means of the three groups (control, LIG, and HIG) are equal. It shows that for all the preexperimental variables, the hypotheses that the means are equal across the villages assigned in the three experimental groups cannot be rejected.³⁵ The preexperiment

³⁵We also conducted more formal tests of the quality of randomization underlying our experiment. We regress the probability of being assigned to

TABLE 2.—SUMMARY STATISTICS OF KEY VILLAGE-LEVEL VARIABLES

Variables	Whole Sample		By Group						<i>p</i> -value
	Mean	SD	Control		LIG		HIG		
			Mean	SD	Mean	SD	Mean	SD	
Preexperiment variables									
Number of sows in Dec. 2006	16.3	21.4	18.0	26.5	13.2	14.2	16.9	21.5	0.18
Number of sows in Sept. 2007	29.1	31.8	28.8	43.1	28.1	20.5	29.8	29.4	0.91
Number of sows in Dec. 2007	31.2	34.5	32.3	46.5	26.4	23.7	32.6	31.7	0.43
Number of pigs in Dec. 2006	356.2	228.4	363.3	248.3	338.3	228.1	361.6	218.4	0.61
Village population	1,029.1	677.8	1,043.7	653.8	1,017.9	672.0	1,027.4	694.8	0.96
Number of villagers as migrant workers	196.0	116.4	189.3	126.9	193.7	103.1	200.5	117.6	0.68
Average villager age	33.2	2.1	33.1	2.1	33.4	2.3	33.2	1.9	0.49
Average villager education (years)	5.95	0.75	6.00	0.77	6.00	0.77	5.90	0.74	0.48
Fraction male in village	0.54	0.03	0.54	0.03	0.55	0.03	0.54	0.02	0.33
Land per household (mu)	4.31	1.97	4.10	1.91	4.28	1.95	4.43	2.00	0.33
Log house value	9.83	.63	9.87	.59	9.75	.63	9.84	.63	0.32
Number of surnames in the village	5.36	2.68	5.38	3.08	5.19	2.47	5.44	2.57	0.71
Number of villagers in new medical cooperative scheme	551.5	300.7	555.1	307.1	532.1	299.2	561.4	299.1	0.69
Number of households receiving government subsidy	182.2	92.2	182.9	89.6	178.8	99.6	183.5	89.9	0.90
Postexperiment variables									
Number of insured sows	22.67	26.88	15.43	10.71	21.51	19.48	26.87	34.02	0.00
Number of sow deaths in snowstorm	0.19	0.83	0.17	1.15	0.10	0.44	0.24	0.79	0.32
Number of sows in March 2008	38.4	34.3	31.8	23.2	35.7	31.4	42.4	38.9	0.06
Number of sows in June 2008	42.9	37.7	35.9	25.8	39.8	35.8	47.2	42.2	0.07

variables are characteristics of the villages collected before our experiment period (November 21–December 25, 2007), mostly from the CAC. The average number of sows in December 2006 was 16.3 across all 480 villages³⁶—18.0, 13.2, and 16.9, respectively, among the control, LIG, and HIG villages. Although 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 18%). It is interesting to note that the average number of sows across all villages increased by almost 80% 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 was very close across the three experimental groups: the means were 28.8, 28.1, and 29.8, respectively, for the control, LIG, and HIG villages. The number of pigs in each village was about 350 in December 2006. The average population was about 1,000 with an average age of about 33, and about 20% of the villagers worked elsewhere as migrant workers.

Table 2 also reports the summary statistics of several post-experimental variables. The average number of insured sows across the villages was 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 was about 78%. However, there was substantial variation in insurance take-up rates

across the experimental groups. Among the control group villages, an average of 15.43 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 26.87 out of 29.8 sows were signed up for insurance.

We also report the number of sow deaths during the deadly snowstorm between January 12 and February 25, 2008 (see section VD for details). On average, 0.19 sows died in one village. However, the average number of sow deaths in the control villages was 0.17, higher than that of 0.10 in LIG villages and lower than that of 0.24 in HIG villages. In addition, the last two rows of table 2 show that the number of sows in March 2008 and June 2008 continued to rise from the September 2007 levels.

D. Test for Parallel Trend between the Control and Treatment Villages

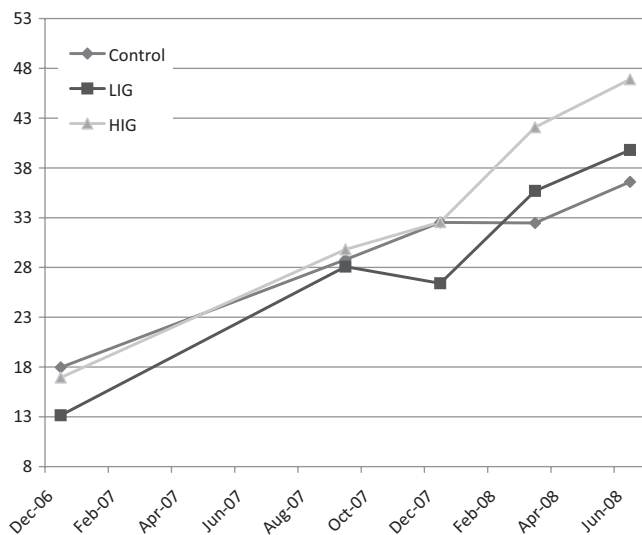
Our IV estimator of the effect of insurance access on production also relies on the assumption that there is no systematic difference in the trend of sow production between control and treatment villages. In figure 1, we plot the average number of sows by the three experimental groups for the entire period for which we have data. It shows that prior to our experimental intervention, the number of sows grew similarly in the three groups, especially between the control and HIG, but afterward the HIG grew more quickly.³⁷

³⁷ We also conducted a more formal test of the parallel trend assumption where we regressed 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 formal test results suggest a parallel trend between the control and treatment group villages before our experiment, but there

one of the three experimental groups on a list of preexperiment village-level variables using linear probability, multinomial probit, and multinomial logit specifications. These tests overwhelmingly show that none of the included variables predict the experimental group assignment. The details are reported in the online appendix.

³⁶ The ratio of the numbers of pigs and sows for a typical village is about 20. Given the fact that a sow typically gives birth to about eight to twelve piglets per litter and can have two litters each year, such a ratio is not too surprising.

FIGURE 1.—TIME TREND OF THE NUMBER OF SOWS FOR DIFFERENT INCENTIVE GROUPS



V. 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 VA the ordinary least square (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 VC, we use the random experimental group assignment as instruments for the number of insured sows in order to recover the overall causal effect of insurance access on subsequent sow production at the village level.³⁸

A. Results from the OLS Regressions

Table 3 reports results from the following OLS specifications:

$$Y_i = \alpha_0 + \alpha_1 \text{Insured_Sows}_i + \alpha_2 \text{Sows2006}_i + \text{Township_Dummies} + \epsilon_i, \quad (1)$$

was a different trend between the control and the HIG groups after our experiment. The details are reported in the online appendix.

³⁸ Since the goal of the subsidized sow insurance was to increase pork production, it is useful to mention that there is a strong positive correlation between the number of sows and the number of slaughtered porker pigs. Using data from *China Husbandry Yearbook* in 2008–2011, we find that a 1 percentage point increase in the number of sows is associated with a 1.22 percentage point increase in the number of the slaughtered porker pig. The overall effect we estimate is the sum of both the extensive and the intensive margins, where the intensive margin refers to the previous sow farmers increasing the number of sows and the extensive margin refers to the production from new sow farmers. We do not have the necessary household level data to distinguish the two margins.

where Y_i represents the number of sows in village i measured in March 2008 or June 2008. *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 is included in some specifications in order to control for the effects of township-specific characteristics on sow-raising.³⁹ Robust standard errors are reported in parentheses in table 3.⁴⁰

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 was unavailable in our data set. As a result, we used the number of insured sows in each village as a proxy for accessibility.⁴¹ The number of insured sows contains information on availability, but it may be contaminated by other factors. In the next section, we report the reduced-form effects of the treatments on both insured sow ownership and total sow ownership

Focusing on the specifications with controls of both township dummies and *Sows2006* reported in columns 3 and 6, 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 a p -value close to 0.

However, the variation in the number of insured sows used in the 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 is more likely to purchase sow insurance when such an option is presented. This would lead to an upward bias in the estimated effect of insurance on production.

³⁹ We chose to include the number of sows measured in December 2006 instead of 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 on request.

⁴⁰ In earlier versions of this paper, we reported standard errors clustered at the township level. None of the results reported in tables 3 to 7 is qualitatively affected by whether we cluster the standard errors at the township level.

⁴¹ 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 for the number of sows at 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 subsequent number of sows raised at the village. For a robustness check, we also used the percentage of sows insured over the sows in 2006 as the measure for the access, the basic results remain qualitatively similar.

TABLE 3.—OLS REGRESSION RESULTS ON THE RELATIONSHIP BETWEEN SOW INSURANCE AND SUBSEQUENT SOW PRODUCTION

Variables	Number of Sows in March 2008			Number of Sows in June 2008		
	(1)	(2)	(3)	(4)	(5)	(6)
Number of insured sows	1.215*** (.078)	1.239*** (.078)	1.093*** (.150)	1.323*** (.088)	1.314*** (.090)	1.158*** (.169)
Number of sows in Dec. 2006			.298* (.164)			.318* (.190)
Constant	11.14*** (1.710)	16.79*** (3.906)	15.91*** (3.604)	13.21*** (1.911)	17.91*** (4.276)	16.96*** (3.99)
Township dummies	No	Yes	Yes	No	Yes	Yes
Adjusted R^2	.67	.78	.79	.66	.76	.78

Robust standard errors are in parentheses. Significance at *10%, **5%, and ***1%.

TABLE 4.—EFFECT OF INCENTIVE GROUP ASSIGNMENTS ON SUBSEQUENT SOW PRODUCTION: REDUCED-FORM RESULTS

Variables	Number of Sows in March 2008		Number of Sows in June 2008	
	(1)	(2)	(3)	(4)
Low-incentive group	4.895 (4.478)	7.110** (3.477)	5.146 (5.055)	7.497* (4.043)
High-incentive group	9.196** (3.894)	7.491** (3.182)	9.876** (4.283)	8.066** (3.538)
Number of sows in Dec. 2006		1.120*** (.152)		1.189*** (0.166)
Constant	20.954*** (5.262)	13.280*** (4.175)	22.236*** (6.270)	14.089*** (5.041)
Township dummies	Yes	Yes	Yes	Yes
Adjusted R^2	.274	.546	.293	.547

Robust standard errors are in parentheses. Significance at *10%, **5%, and ***1%.

B. Results from Reduced-Form Regressions

Table 4 reports the reduced-form regression results, examining how the treatment dummies affect the subsequent sow productions. We see that HIG villages had higher levels of sows in both March and June 2008 than LIGs, and two incentive groups generated more sows than the control group villages did. The coefficients on the two incentive groups, reported in columns 2 and 4, are both statistically significant at least at the 10% level.

C. 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 AHWs. 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 2, the first requirement for experimental group assignment as valid IVs for *Insured_Sows* is automatically satisfied.⁴²

⁴² Indeed Hansen's J -statistics from the IV regression reported in table 6 is only 0.068; thus, the overidentification test does not reject the null that all instruments are valid (with a p -value of 0.7938).

TABLE 5.—EFFECT OF GROUP ASSIGNMENTS ON THE NUMBER OF INSURED SOWS: FIRST-STAGE RESULTS

Variables	Number of Insured Sows		
	(1)	(2)	(3)
Low-incentive group	6.075*** (2.027)	6.606*** (2.072)	9.622*** (2.064)
High-incentive group	11.433*** (2.405)	11.336*** (2.687)	12.007*** (2.418)
Number of sows in Dec. 2006			.670*** (.189)
Constant	15.433*** (.977)	18.538*** (4.268)	2.134 (5.778)
Township dummies	No	Yes	Yes
Adjusted R^2	.031	.20	.46

Robust standard errors are in parentheses. Significance at *10%, **5%, and ***1%.

Now we report the first-stage result that shows that the second requirement for valid IVs 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 2006 and a set of township dummies.⁴³ Overall we find a strong and significant effect of AHW performance incentive assignment 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 9.6 additional insured sows.

⁴³ We have also run specifications with the number of sows measured in September 2007 as additional controls. The coefficient estimates on the group assignments do not change qualitatively or quantitatively. These results are available from the authors on request.

TABLE 6.—IV REGRESSION RESULTS ON THE EFFECT OF SOW INSURANCE ON SUBSEQUENT SOW PRODUCTION

Variables	Number of Sows in March 2008			Number of Sows in June 2008		
	(1)	(2)	(3)	(4)	(5)	(6)
Number of insured sows	.904*** (.241)	.854*** (.252)	.775*** (.244)	.962*** (.266)	.916*** (.281)	.829*** (.279)
Number of sows in Dec. 2006			.537*** (.206)			.566** (.230)
Constant	18.116*** (5.530)	41.106*** (14.595)	2.933 (3.534)	21.316*** (6.090)	53.740*** (16.180)	2.760 (3.728)
Township dummies	No	Yes	Yes	No	Yes	Yes
Adjusted R ²	.624	.73	.77	.61	.72	.76

Robust standard errors are in parentheses. The instruments for the number of insured sows are the group assignments. Significance at *10%, **5%, and ***1%.

Since the sample mean of the insurance coverage is 22.6, the increase of 9.6 sows represents about 43% of the sample mean, an economically significant effect. Moreover, as expected, we find that this incentive effect is stronger for the high-incentive group. When township dummies are included, the set of independent variables can explain more than 45% of the total variation in the number of insured sows.⁴⁴

Second-stage results. Table 6 reports the second-stage regression results. It shows that when we used the exogenous variation only in insurance coverage induced by the variations in the random assignment of incentives to AHWs, the estimated effects of insurance on subsequent production were smaller than those from the OLS regression reported in table 3; 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 were 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 0.78 by March 2008 and by 0.83 by June 2008. Both coefficient estimates are significant at the 1% 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 7.5 and 9.4 more sows being raised by March 2008 in the low- and high-incentive group villages, respectively, than in the control group villages.⁴⁵ 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 4.3 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.

It is worth mentioning that there seems to be some suggestive evidence that the effect of insurance on the number of sows is larger when the number of sows was measured in June 2008 (six months after the insurance was provided) than in March 2008. This is true for both the OLS result in table 3 and the IV results in table 6. This most likely reflects the fact that farmers had longer by June 2008 to react to the sow insurance by turning young female piglets into sows.

Making sense of the effect of insurance on sow production. The effect of sow insurance on the number of sows we estimated in table 6 is likely the combination of two types of actions by the farmers: first, the farmers may increase the total number of sows by purchasing additional fertile female pigs; second, the farmers keep as sows some of their own female piglets that would otherwise be spayed.⁴⁶ Our experiment took place in December 2007, and the first postexperiment measurement of the number of sows occurred in March 2008, only three months later. Our results show a large increase in reported sow numbers by this time. However, according to the time line of sow production described in section IID, it takes about six months to raise a sow. There was not enough time to have such a large natural increase in the number of sows due to the insurance. The only way to rationalize such a large increase would be that there were a lot of piglets close to one month old in December 2007 available for farmers to turn into sows. Although we do not have data on the number of young female piglets per village at the end of 2007, a back-of-the-envelope calculation indicates that it is plausible to see about a ten-sow difference between control and treatment groups reported in table 5. As shown in table 2, there were about 356 pigs and 16 sows per village in December 2006 and 29 sows per village in September 2007. Suppose the sow-to-pig ratio is constant over time; then there would be 645 pigs in September 2007. Even without any increase in pigs from September to December 2007, there should be around 322 female pigs per village. Farmers typically raise meat pigs to be around 6 to 8 months old (12 months at most) and then sell them to the market. If the distribution of female

⁴⁴ The partial R² from the first stage is about 0.0625, and the first-stage F-statistics for the significance of the excluded instrument is 13.49.

⁴⁵ 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 2008, as reported in table 4.

⁴⁶ Unfortunately, we are unable to disentangle them because we do not have data on the total number of female pigs.

pigs at different ages is uniform, the number of young female piglets around 1 month old in December 2007 was at least 26.

D. *The Effect of an Unexpected Ice and Snow Storm*

In this section, we briefly describe some additional evidence from an unexpected, and severe, ice and snow storm, that occurred in early 2008, just a month and a half after our field experiment. The storm hit southern and southwestern China, and Guizhou was one of the most affected provinces. The storm began in mid-January and lasted for a month, and its scope and severity were unprecedented in at least the last fifty years. Since snowstorms in general are rare in this part of China, let alone one with such severity, many sows and pigs died during the storm, especially sows raised in the backyard of village households that lacked necessary facilities.⁴⁷ News reports indicated that 5,973 sows died during the storm in Guizhou Province.⁴⁸ The ice and snow storm of such a scale was totally unexpected, and the amounts of snowfall in the villages in our sample were uncorrelated with the village characteristics.

In the online appendix, we report results from IV regressions similar to those in table 6 except that we now add the interaction of “Number of Insured Sows” and “Number of Sow Deaths in Snowstorm,” using our experimental group assignments as the instruments for “Number of Insured Sows,” and instrument for the interaction terms involving “Number of Insured Sows” by the corresponding interaction terms with our experimental group assignments. Interestingly, we find that the coefficients on the interaction terms are positive and significant at least at the 15% significance level.

We consider this positive interaction effect between the sow deaths in the storm and insurance as supportive evidence that trust plays an important role in the farmers’ insurance take-up decisions. As we mentioned in section IIIC, when farmers do not have complete trust on whether the insurance product is genuine, the insurance policy itself becomes a risk. Nothing is more convincing to the villagers than that the government-subsidized sow insurance is for real than actually paying out the promised damage compensation in this unusual storm.⁴⁹ In villages with more sow deaths and more

insured sows, there would be more positive cases that the sow insurance contracts were 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 on subsequent sow production should be stronger in such villages.⁵⁰

The positive interaction effect between sow deaths in the storm and insurance also casts 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 (see section IIIB). Under such a mechanism, we would see the positive effect of insurance coverage on sow production, but we should not see any significant effect of the interaction between the severity of the storm and insured sows.

E. *Longer-Run Effects and Spillover on Other Livestock*

In this section, we present some additional results regarding the longer-run effects of the sow insurance on sow production and the spillover effects of sow insurance on other livestock. For this purpose, we collected additional village-level data on the number of sows and pigs measured in December 2009 and December 2010, as well as the number of sheep and cows. Because the number of sheep and cows was collected less frequently than that of sows and pigs, we have their information only for December 2008.

Table 7 presents IV regression results of the number of sows, pigs, sheep, and cows on the number of insured sows during our experimental period (the end of 2007), after controlling for the corresponding stocks at the end of December 2006 from the China Agricultural Census (see section IV). In columns 1 and 2, we find that the number of insured sows by December 2007 had a positive effect on the number of sows in 2009 and 2010, though the effect was significant only at the 20% level. However, if we take the average number of sows measured at the end of the three-year period, 2008 to 2010, we find in column 3 that the number of insured sows in 2007 had a positive effect that is statistically significant at the 1% level. We similarly find in columns 4 and 5 that the variation in the number of insured sows induced by our experimental group assignment was positively related to the number of pigs (for pork production) in 2009 and 2010, as well as the average number of pigs in the three-year period 2008 to

⁴⁷ According to Wang and Watanabe (2007), summer months are the most deadly period for pigs in general.

⁴⁸ See the news report on Xinhuanet (February 20, 2008), “Guizhou Fully Made the Compensation for the Insured Sows Died during the Snowstorm,” www.gz.xinhuanet.com/xwpd/2008-02/20/content. While this snowstorm caused a lot of sow deaths in Guizhou Province, the damage to sows in our experimental area was somehow modest. As shown in table 2, the number of sow deaths per village was 0.19, which accounts for 0.7% of the number of sows per village in September 2007.

⁴⁹ Indeed, as reported by Xinghua News Agency and the *Financial Times* (Chinese), following government directives, the insurance company quickly dispatched work teams to remote villages to deal with claim evaluations and settlements. See “Guizhou Province Made Full Compensations on Lactating Sows Which Were Insured and Died of the Ice and Freeze Storm,” Xinghua News Agency, February 20, 2008; and “Insurance Industry Meets with the Ice and Freeze Disaster in the Special Way” (*Financial Times, Chinese*, February 27, 2008).

⁵⁰ In table 7 of Cai et al. (2009), we document that the number of insured sows is significantly associated with the coverage of new rural cooperative medical plans in the village as well as the coverage of government subsidies in the village, further suggesting the importance of trust for the government in insurance take-up. Details are in the online appendix.

Our examination of the role of trust for the villagers’ demand of insurance echoes that of Cole et al. (2013) 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. (2013) did in their study. It is interesting to note that our evidence strongly corroborates their findings.

TABLE 7.—IV ESTIMATES OF THE LONGER-RUN EFFECTS OF SOW INSURANCE ON SOW AND PIG PRODUCTION AND THE SPILLOVER EFFECTS ON OTHER LIVESTOCK (SHEEP AND COWS)

Variables	Sows			Pigs			Sheep	Cows
	2009	2010	Average, 2008–2010	2009	2010	Average, 2008–2010	2008	2008
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of insured sows	.291 [†] (.206)	.291 [†] (.207)	.452 ^{***} (.175)	1.284 [†] (.950)	1.486 ^{††} (1.007)	1.346 [†] (.956)	.592 ^{††} (.373)	.490 (.497)
Number of sows (Dec. 2006)	.164 (.191)	.165 (.190)	.288 [*] (.151)					
Number of pigs (Dec. 2006)				-.079 [†] (.059)	-.010 ^{††} (.061)	-.085 [†] (.059)		
Number of sheep (Dec. 2006)							-.011 (.024)	
Number of cows (Dec. 2006)								-.331 ^{††} (.203)
Constant	14.51 ^{***} (4.18)	13.94 ^{***} (3.81)	10.46 ^{***} (3.17)	540.18 ^{***} (28.75)	545.90 ^{***} (28.96)	538.72 ^{***} (19.60)	33.29 (35.17)	184.89 ^{***} (16.57)
Township dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R ²	.494	.486	.612	.851	.782	.801	.486	.674

Robust standard errors are in parentheses. The instruments for “Number of Insured Sows” are the group assignments, and the instruments for the interaction terms involving “Number of Insured Sows” are the corresponding interaction terms with the group assignments. ***, **, *, †, †† denote significance at 1%, 5%, 10%, 15%, and 20%, respectively.

2010. These effects are significant at the 15% to 20% level. In columns 7 and 8, we also found that the number of insured sows had a positive but generally insignificant effect on the number of sheep and cows in the villages. These results suggest that the positive and significant short-run effect of the number of insured sows on sow production, as reported in table 6, seems to have some persistence in the longer run. Moreover, the increase in sow production in response to the sow insurance did not seem to be the result of the substitution of other livestock such as pigs, sheep, or cows. In fact, if anything, the spillover effects of sow insurance on the production of other livestock seem to be positive, though the effects are only marginally significant.

VI. Conclusion

In this paper, we report results from a large, randomized field experiment that evaluates the effect of microinsurance on subsequent production. Making use of a heavily subsidized insurance program for sows, we randomized the incentive schemes offered to AHWs, which generates plausible exogenous variations in the effective insurance access across 480 villages in our experimental sample. This allowed us to use the random incentive scheme assignment as the instrumental variable for insurance access to recover the causal effect of insurance access on production. Our results indicate that promoting greater adoption of formal insurance significantly leads to a subsequent increase in sow-raising. To the best of our knowledge, this is one of the first large-scale experimental studies on the effect of microinsurance on farmer production behavior. Our findings suggest that microinsurance may be as important as microfinance in increasing production, and microinsurance can supplement and strengthen the effects of microfinance by protecting farmers from the inherent risk of entrepreneurial activities.⁵¹

⁵¹ It is important to note, as we mentioned in section IID, that the sow insurance studied in our paper insures farmers only against loss from

Our experimental design, as well as corroborating evidence from the effect of an unexpected snowstorm, suggests that the documented correlation between the sow insurance coverage and the subsequent sow production may be causal. We also find some suggestive evidence that the positive and significant short-run effect of sow insurance on sow production seems to have some persistence in the longer run; moreover, the increase in sow production in response to the sow insurance does not seem to be the result of the substitution of other livestock, such as pigs, sheep, or cows. Our evidence from the effect of an unexpected snowstorm reported in section VD also suggests that trust, or lack thereof, of 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, Gine, Tobacman et al. (2013). 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 were unable to address several important questions due to data limitations. First, how would providing access to microinsurance complement the effectiveness of micro-credit programs?⁵² Second, how does the increase in sow production affect farmers’ well-being as measured by consumption, for example? There are fruitful avenues for future research.

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 farmers who purchased the sow insurance still faced significant risks. We would expect that the effect of insurance on sow production would be even larger if the farmers also had access to insurance against price fluctuations.

⁵² Gine and Yang (2007) study whether the provision of rainfall insurance induces farmers to borrow to invest in new varieties. Karlan et al. (2013) study 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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