Online Appendix:

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

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Review of Economics and Statistics

In this appendix, we include some details omitted in the main text. Appendix A presents the formal test of the quality of randomization referenced in Footnote 36; Appendix B presents the regression based formal test of the parallel trend between the control and treatment villages referenced in Footnote 38. Appendix C collects the omitted details of our analysis using the unexpected snow storm, which we referred to in Section 5.4 in the main text. Appendix D contains the details of Table 7 in our NBER Working Paper No. 15396 which we referenced in Section 5.4.[1]

A 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 A 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 A overwhelmingly shows that none of the included variables predict the experimental group assignment. In the whole Table A, which reports 72 coefficient estimates, only one is 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

[1]The robust standard errors clustered at the township level are reported in the tables in this appendix. This differs from the tables in the main text where robust standard errors without clustering are reported (following a referee's suggestion). All the results remain qualitatively similar if we use robust standard errors and they are available from the authors upon request.

Variables	Linear Probability		Probit		Logit	
	LIG	HIG	LIG	HIG	LIG	HIG
No. of Sows in Dec. 2006	-.0017*	.0003	-.0110	-.0028	-.0150*	-.0040
	(.001)	(.0016)	(.0069)	(.0054)	(.0091)	(.0064)
No. of Pigs in Dec. 2006	-9.9e-06	.0001	.0001	.0002	.0002	.0002
	(.0001)	(.0001)	(.0009)	(.0007)	(.0012)	(.0009)
No. of Villagers in New Medical Cooperative Scheme (NMCS)	-.0001	.0002	-.0001	.0007	-.0001	.0009
	(.0001)	(.0002)	(.0005)	(.0006)	(.0007)	(.0008)
No. of Households Receiving Gov. Subsidy	.0006	-.0008	.0014	-.0019	.0016	-.0024
	(.0004)	(.0005)	(.0020)	(.0020)	(.0027)	(.0026)
Ave. Villager Age	.005	.0008	.0305	.017	.042	.023
	(.0108)	(.0107)	(.0472)	(.0340)	(.0630)	(.0506)
Ave. Villager Education (Years)	.042	-.052	.091	-.128	.129	-.147
	(.0369)	(.0325)	(.1909)	(.1520)	(.2649)	(.1979)
Fraction Male in Village	1.13	-.35	5.29	1.42	7.84	2.48
	(1.011)	(1.075)	(4.142)	(3.556)	(5.836)	(4.443)
Village Population/1,000	.009	-.010	.019	-.014	.034	-.019
	(.0285)	(.0432)	(.1485)	(.1746)	(.1927)	(.2266)
No. of Villagers as Migrant Workers	1.7e-06	.0001	.0003	.0005	.0006	.0008
	(.0002)	(.0002)	(.001)	(.0007)	(.0014)	(.0010)
No. of Surnames in the Village	-.009	.006	-.026	.009	-.044	-.007
	(.0095)	(.0133)	(.0558)	(.0563)	(.0770)	(.0704)
Whether Village is the Township Government Location?	-.031	.159	.355	.724	.479	.910
	(.0887)	(.1046)	(.4784)	(.4468)	(.6675)	(.6025)
Land per Household (Mu)	-.0019	.0143	.0352	.0613	.0457	.080
	(.0137)	(.0095)	(.0813)	(.0557)	(.1113)	(.0727)
(Pseudo-) R^2	.0167	.0165	...		.0164	

Table A: Test for the Quality of Randomization for the Field Experiment.
NOTES: (1) Robust standard errors clustered at the township level are reported; (2) All regressions include an unreported constant term; (3) *, **, *** denote significance at 10%, 5% and 1%, respectively.

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.

B 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.[2] Table B 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 × September 2007", "LIG × December 2007", "HIG × September 2007" and "HIG × 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 the 5% level. These suggest that there is a parallel trend between the control and treatment group villages before our experiment, but there is a different trend between the control and the HIG groups after our experiment.[3]

C The Effect of An Unexpected Snowstorm: Details

Here we provide the details of the omitted results referred to in Section 5.4 in the main text.

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

[3]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.

Variables	Number of Sows
September 2007	8.26***
	(2.18)
December 2007	14.22***
	(2.48)
March 2008	19.84***
	(2.50)
June 2008	23.97***
	(2.50)
LIG × September 2007	4.25
	(3.11)
LIG × December 2007	1.20
	(3.58)
LIG × March 2008	4.09
	(3.54)
LIG × June 2008	4.07
	(3.54)
HIG × September 2007	1.90
	(2.71)
HIG × December 2007	3.21
	(3.01)
HIG × March 2008	6.39**
	(3.00)
HIG × June 2008	7.06**
	(3.00)
Constant	15.85***
	(.76)
R^2	.341

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

NOTES: (1) Robust standard errors clustered at the township level are reported; (2) The omitted time period is December 2006; (3) *, **, *** denote significance at 10%, 5% and 1%, respectively.

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 lasted for a month, 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.[4] News report indicated that there were a total of 5,973 sows that died during the storm in Guizhou province.[5] 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 the amounts of snowfall in the villages in our sample were uncorrelated with the village characteristics. 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. Tables C1 and C2 report the IV estimates of the effect of the insured sows and the sow deaths during the snow storm on subsequent sow productions as of March and June 2008 respectively. In both tables, we use our experimental group assignments as the instruments for

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

[5] See the newsreport on Xinhuanet (Feburary 20, 2008), "Guizhou Fully Made the Compensation for the Insured Sows Died during the Snowstorm", at www.gz.xinhuanet.com/xwpd/2008-02/20/content. While this snowstorm caused a lot of sow deaths in Guizhou province, the damage on sows in our experimental area was somehow modest due to the uneven distribution of snowstorm. As shown in Table 2, the number of sow deaths per village is 0.19, which accounts for 0.7 percent of the number of sows per village in September 2007.

"No. of Insured Sows," and instrument for the interaction terms involving "No. of Insured Sows" by the corresponding interaction terms with our experimental group assignments.

The specifications in Column (1) of the two tables are similar to those in Table 6 except that we now add "No. of Sow Deaths in Snow Storm." The estimates in Column (1) show 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 of the sow deaths during the snow storm on the number of sows, this strongly suggests some large positive offset.

In Column (2) of Tables C1 and C2, we include the interaction of "No. of Insured Sows" and "No. of Sow Deaths in Snow Storm." Interestingly, the coefficients on the interaction terms are positive and significant at least at 15% significance level. As we argued before, this result suggests that even though there may be some pathways for our incentive assignment to the AHWs to affect sow production directly, they are likely unimportant or at least not dominant.

How should we interpret the positive and significant effect on the interaction between the sow death in the storm and insurance reported in Column (2) of Tables C1 and C2? We would like to interpret this positive interaction effect between the sow death in the storm and insurance as the effect of 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. 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.[6] It is useful to emphasize that, for the trust effect of sow insurance to operate, the death and subsequently time payment of

[6]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*).*

loss for even one insured sow would be powerful.

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.[7] 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.[8] Thus indeed, Table C1 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.

Robustness Checks.[9] While the snowstorm was unexpected and the snowfall amount was random, its impact may be correlated with the physical village characteristics. For example, the snowstorm may have caused more damages in villages with lower income and wealth. If it is true, the positive interaction effects with the snowstorm variable shown in Column (2) of Tables C1 and C2 may simply reflect some other differential treatment effect across villages. In Column (3) of

[7]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.

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

[9]We are grateful to an anonymous referee for suggesting these robustness checks.

Variables	(1)	(2)	(3)	(4)	(5)
No. of Insured Sows	.771**	.716**	-2.730	-.792	-1.903
	(.304)	(.318)	(4.120)	(2.366)	(2.662)
No. of Sows in Dec. 2006	.507**	.520**	.507**	.499**	.648***
	(.227)	(.229)	(.229)	(.229)	(.237)
No. of Sow Deaths in Snowstorm	5.13**	2.46***	-52.81	-67.39	-57.05*
	(2.35)	(.57)	(49.90)	(57.75)	(28.86)
No. of Insured Sows ×		.074*	.094**	.832	1.068††
No. of Sow Deaths in Snowstorm		(.041)	(.043)	(.815)	(.655)
Log Housing Value			-3.429	-.833	
			(6.097)	(4.833)	
No. of Insured Sows × Log Housing Value			.346	.147	
			(.420)	(.238)	
No. of Sow Death in Snowstorm			5.273	6.673	
× Log Housing Value			(4.805)	(5.538)	
No. of Insured Sows × No. of Sow Death				-.071	
in Snowstorm × Log Housing Value				(.076)	
Education					-6.184
					(8.143)
No. of Insured Sows × Education					.396
					(.445)
No. of Sow Death in Snowstorm × Education					9.206*
					(4.684)
No. of Insured Sows × No. of Sow Death					-.152
in Snowstorm × Education					(.103)
Constant	-.976	9.646	62.82	33.96	40.35
	(3.38)	(9.714)	(63.80)	(51.17)	(48.79)
Township Dummies	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	.789	.807	.823	.838	.801

Table C1: IV Estimates of the Effect of Sow Deaths During the Snow Storm on Sow Production in March 2008.

NOTES: See notes for Table C2.

Variables	(1)	(2)	(3)	(4)	(5)
No. of Insured Sows	.823**	.773*	-2.623	.267	-2.799
	(.368)	(.389)	(5.107)	(3.090)	(3.278)
No. of Sows in Dec. 2006	.536*	.548*	.545*	.529*	.655**
	(.265)	(.269)	(.279)	(.277)	(.255)
No. of Sow Deaths in Snowstorm	5.230*	2.768***	-54.67	-78.88	-65.843*
	(2.671)	(.857)	(58.70)	(71.65)	(34.842)
No. of Insured Sows ×		.069††	.092††	1.299*	1.436***
No. of Sow Deaths in Snowstorm		(.045)	(.060)	(.735)	(.518)
Log Housing Values			-2.924	.827	
			(7.587)	(6.347)	
No. of Insured Sows × Log Housing Value			.339	.043	
			(.524)	(.3156)	
No. of Sow Death in Snowstorm			5.475	7.778	
× Log Housing Value			(5.657)	(6.865)	
No. of Insured Sows × No. of Sow Death				-.116*	
in Snowstorm × Log Housing Value				(.065)	
Education					-8.740
					(9.578)
No. of Insured Sows × Education					.555
					(.546)
No. of Sow Death in Snowstorm × Education					10.615*
					(5.395)
No. of Insured Sows × No. of Sow Death					-.211**
in Snowstorm × Education					(.082)
Constant	-1.22	13.63	70.53	28.50	55.65
	(3.49)	(12.02)	(77.88)	(65.71)	(57.26)
Township Dummies	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	.773	.788	.804	.823	.788

Table C2: IV Estimates of the Effect of Sow Deaths During the Snow Storm on Sow Production in June 2008.

NOTES: (1) Robust standard errors clustered at the township level are in parentheses; (2) The instruments for "No. of Insured Sows" are the group assignments, and the instruments for the interaction terms involving "No. of Insured Sows" are the corresponding interaction terms with the group assignments; (3) ***, **, *, ††, † denote significance at 1%, 5%, 10%, 15% and 20% respectively.

Tables C1 and C2, we include the log of the village housing values, and its interactions with the number of insured sows as well as the number of sow deaths in the snow storm. Note that the inclusion of these additional controls largely does not change either the magnitude or the statistical significance of the coefficient estimates on the interaction term of "No. of Insured Sows" and "No. of Sow Deaths in Snow Storm."

However, our interpretation of the results in Column (2) of Tables C1 and C2 as evidence for the effect of trust still encounter several potential challenges. The first concern is that, adding log house values in levels as we did in Column (2) may not adequately control for the wealth effect. To more adequately disentangle the trust effect from the wealth effect, in Column (4) of Tables C1 and C2, we include a triple interaction term between log of the village housing value, the number of sow deaths in the snow storm and the number of insured sows, while at the same time controlling for a set of double interaction terms between these three variables. The coefficient on the interaction term between sow deaths in the snowstorm and the number of insured sows still has the expected positive effect on the number of sows in both March and June 2008, and it is significant for June 2008.

Second, because as we described in Section 2 the insurance payment is less than the market value of a sow, it is possible that the more likely confounding explanation is a liquidity effect as supposed to a wealth effect. Unfortunately we do not have direct observations on either total village savings account balances or income to control for the liquidity effect. In Column (5) of Tables C1 and C2, we proxy income or liquid assets using average level of education in the village since it is plausible to believe that higher education is correlated to higher income. Specifically, we include an interaction term between average village education, the number of sow deaths in the snow storm and insured sows, while at the same time controlling for a set of double interaction terms between these three variables. Results in Column (4) indicate that the triple interaction is negative (and

also statistically significant for June 2008), which implies that fewer new sows in response to sow deaths when the village has more liquidity. This suggests that liquidity, or something correlated with liquidity, is indeed part of the explanation for our findings. However, the coefficient estimates of the interaction term between the number of sow deaths in the snow storm and the number of insured sows still have a positive sign in both cases and statistically significant at 12% level for March 2008 and 1% level for June 2008. This suggests that something about the insurance payouts other than liquidity is having an effect.

D Table 7 in Cai et al (NBER Working Paper 15396)

While we have argued that the snowstorm effect reported in Tables C1 and C2 is consistent with the trust story, we do not have direct evidence on the effect of trust on subsequent insurance take-up. As a partial remedy for the lack of direct evidence on the effect of trust on the insurance take-up, we would like to discuss some additional evidence presented in an earlier version of our paper (Cai et al. (2009)). 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 schemes in the village as well as the coverage of government subsidy in the village. The new rural cooperative medical scheme was launched by Chinese government in 2003 and both central and local governments provided subsidies to the premium. Even with heavy subsidies and reasonable price, the take-up of this scheme surprisingly varied across villages. In our village sample, the average take-up rate was only 50%. One of the obstacles was the trust on the ability of government to deliver the promise. Against this background, we hypothesize that if trust is also an important determinant for villagers' purchase of the sow insurance, we would expect to see that there would be a positive correlation between a village's take-up of the medical scheme and its take-up of the sow insurance. The positive correlation between government subsidy coverage and sow insurance can be interpreted as evidence

for the hypothesis that villagers who have been receiving government subsidies tend to have a higher trust for government sponsored program in general, and thus the subsidized sow insurance in particular. While this additional evidence is still subject to other interpretations and thus is not perfect in supporting the role of trust, it may help rule out wealth effects in picking up the whole story. In particular, those people who received government subsidy tended to be poorer than those who didn't, but they may have higher trust in government-sponsored program.

D.1 The Relationship Between Participation in the New Cooperative Medical Scheme and Insurance Purchase

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

New Cooperative Medical Scheme.[10] The original Cooperative Medical Scheme (CMS), introduced in rural China in the 1950s, was dismantled with the collapse of the collective economy in the early 1980s. As a result, by 1985 only about 5% of rural counties in China had any form of health care insurance. Chinese government launched the New Cooperative Medical Scheme in 2003, aiming to provide health coverage for the nation's entire rural population by 2010. Both the central and provincial governments provide subsidies to the premium. For Guizhou Province, in 2003 when NCMS was initiated, the central and provincial government respectively subsidized the program at the rate of 10 Yuan and 40 Yuan per enrollee annually; and the subsidy amounts were increased to 20 Yuan and 50 Yuan respectively in 2006. Each enrollee is only required to contribute

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

24 Yuan annually. Somewhat surprisingly, the heavily subsidized and reasonably priced NCMS was not an immediate success; at the national level, the percentage of rural residents covered under the NCMS increased from 3% in 2004 to 40.57% in 2006. In our sample of villages, as described in Table 2, about slightly over 50% of the villagers signed up the NCMS, with quite sizeable variations in the coverage rates across villages.

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

Columns (1)-(4) in Table D reports the relationship between the number of insured sows and the number of villagers who participated in the NCMS. Results from the preferred specification, which controls for the number of sows in December 2006, as well as the experimental group assignment and Township dummies, are reported in Column (4). The estimated coefficient on the number of villagers in NCMS is positive and significant at the 10% level, thus suggesting that there is some underlying common unobservable factor that leads the villagers to more likely join the NCMS and purchase the sow insurance. At the point estimate, a one standard deviation increase in the number of villagers in the NCMS, which is about 300 from Table 2, will lead to an increase of 4.3 additional insured sows, which represents about 20% of the mean number of insured sows (about 22.7 from Table 2).

While we do not have direct evidence of what the common unobservable factors are, there are several candidates. One candidate common unobservable factor is trust, or lack thereof, for

government sponsored programs. Villages that have a higher trust for government programs in general will both have a higher participation rate in the NCMS, and are more likely to purchase the subsidized sow insurance. In villages assigned to the high incentive group, the AHWs can increase sow insurance coverage by spending more time to dispel the doubts that farmers may harbor toward the government sponsored insurance product. The fact that AHWs are the residents of villages and their personal reputation is held as a "hostage" if they cheat their fellow villagers helps AHWs do their work well.

Another candidate for the unobservable factor is that villages may differ in how easy the information about the government sponsored programs spread. For example, some villages may have more close-knit social networks that words about the government sponsored programs, whether it is the NCMS or the sow insurance, will quickly spread, leading to higher participation rates in both the NCMS and the sow insurance. This channel, however, does not seem to be very plausible because the NCMS has been in place for almost four years, and it is unlikely that any villager has not heard about the NCMS. Yet, the participation rate for the NCMS is still low in the villages with low participation rate for the sow insurance.[11]

D.2 The Relationship Between the Number of Households Receiving Government Subsidies and the Purchase of Sow Insurance

Our second piece of suggestive evidence that trust for government sponsored programs may be the main reason for the seemingly low take-up rate of the sow insurance is the systematic relationship between the number of households receiving government subsidies and the purchase of sow insurance, reported in Columns (5)-(8) in Table D. Here we hypothesize that villagers who have been receiving government subsidies tend to have a higher trust for government sponsored

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

Variables	No. of Insured Sows							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No. of Villagers in New	.0124*	.0121*	.0149*	.0144*				
Coop. Medical Scheme	(.0069)	(.0066)	(.008)	(.0079)				
No. of Households Receiving					.0565***	.0559***	.0610***	.0603***
Gov. Subsidy					(.0194)	(.0188)	(.0220)	(.211)
No. of Sows in Dec. 2006	.676**	.685***	.6005**	.612**	.651**	.659***	.586**	.595**
	(.250)	(.238)	(.263)	(.252)	(.249)	(.237)	(.261)	(.250)
Low Incentive Group		9.850***		9.397***		9.723***		9.257***
		(2.491)		(2.718)		(2.540)		(2.754)
High Incentive Group		12.215***		11.548***		12.278***		11.694***
		(3.103)		(2.959)		(3.134)		(3.022)
Constant	5.135*	-3.419	-.258	-8.265**	2.102	-6.511	-3.001	-11.118***
	(2.924)	(3.964)	(2.653)	(3.750)	(2.940)	(4.219)	(2.586)	(3.857)
Township Dummies	No	No	Yes	Yes	No	No	Yes	Yes
Adjusted-R^2	.355	.390	.449	.478	.371	.406	.461	.492

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

NOTES: (1) Robust standard errors clustered at the township level are reported; (2) *, **, *** denote significance at 10%, 5% and 1%, respectively.

programs in general, and thus the subsidized sow insurance product in particular. The preferred specification with all the controls is reported in Column (8) and the coefficient estimate on the number of households receiving government subsidy is positive and significant at 1%. At the point estimate of the coefficient, a one standard-deviation increase in the number of households receiving government subsidy, which is 92 from Table 2, is associated with an increase of 5.5 insured sows, representing about 25% of the mean number of insured sows (about 22.7 from Table 2).

Potential lack of information about the government sponsored programs can not the common unobservable factor explaining the positive association between households receiving government subsidies and the number of insured sows reported above. The reason is that any eligible households with per capita income below a government specified threshold will automatically receive government subsidies; there is no need for filling out an application and thus there can not be an effect of the knowledge about the existence of the subsidy programs.

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