Subsidy Policies and Insurance Demand[1]

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Abstract

Using data from a two-year randomized experiment in rural China, this paper studies the impact of two subsidy policies, cost-sharing and free-distribution, on the long-term adoption of a new weather insurance product. In the first year, we randomize the two subsidy policies on the village level; in the second year, eight different prices are randomized on the household level. The results show that overall, first-year subsidy policy does not affect second-year take-up and price sensitivity substantially. In order to explain why, we compare three effects between the two subsidy policies: effect of having access to insurance, learning effect, and price anchoring. We find that the learning effect under a free distribution policy is much smaller than that under a partial subsidy policy, while the payout rate in the former case is higher, which drives to the similar average take-up rates in the second year under these two policies. In addition, we find no price anchoring effect. As a result, free distribution subsidy policy performs no better than cost-sharing policy, while the cost of the former is much larger than the later.

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1. Introduction

Introducing new technologies and financial products is important for improving economic development and household welfare in developing countries. However, in many cases, the diffusion of new products is extremely slow. One commonly used way to increase adoption is by providing subsidies, with the expectation that this will improve voluntary adoption in the long-run, even if subsidies are gradually removed. Given the importance of improving innovation adoption and the widespread use of subsidy policy, it's crucial to analyze the impact of providing subsidies on long-run adoption, and to study what is the optimal subsidy policy in the short-run.

Providing subsidies may influence long-run adoption because of the following reasons. First, people who took the product when subsidies are provided will be more likely to purchase it again in the future because of habit formation. Second, improving the coverage gives more people the opportunity to learn benefits of the product either through own experience, or friends' experience with the product. Third, there might be a negative effect as people may anchor on the previous price and will thus less likely to buy if price increases in the future. For details, Dupas (2012) provides a good theoretical framework discussing the potential impact of short-run subsidies on long-run adoption of different health products. There are two commonly used subsidy policies, cost-sharing and free-distribution. Given that subsidies can influence long-run adoption because of the above three reasons, it's not clear which policy is better. First, cost-sharing will reduce program coverage by decreasing demand; Second, the intensity of usage might be lower if the product is distributed for free; Third, it will be more difficult to maintain adoption rate under free-distribution because of price anchoring effect.

This paper studies and compares the impact of two subsidy policies (cost-sharing and free-distribution) on long-term adoption in the context of new weather insurance adoption. During the recent years, many developing countries are providing weather insurance products to farmers whose production are usually exposed substantial weather shocks. However, in most cases, participation rate for such programs is sub-optimally low, and subsidies are frequently provided to improve take-up. Using a two-year randomized field experiment including 143 randomly selected natural villages with around 3,500 households in rural China, we answer the following two questions in this paper. First, what is the impact

of different types of short-run subsidies (cost-sharing or free-distribution) on long-run adoption? Second, why is cost-sharing policy better than free-distribution, or vise versa?

The study design is as follows. In the first year, we randomized subsidy policies (cost-sharing or free-distribution) on the village level. In a randomly selected group of villages, there's a 70% government subsidy. While in other villages, the insurance was sold at 30% of the premium first, and then households were surprised by given the insurance for free. The government spent around 150,000 RMB more on providing subsidies under the full subsidy policy compared with that under the partial subsidy policy. In the second year, in order to estimate demand curve for insurance in the second year, we randomly assigned eight prices with subsidies ranging from 30% to 90% on the household level. Everything else remains the same in the contract.

To estimate the impact of different subsidy policies on long-run insurance demand, we compare the second year insurance demand curve between villages under cost-sharing policies and those under free-distribution policies in the first year. We find that the overall second year take-up rate among households under full subsidy policy in the first year is not substantially higher than that of households under partial subsidy policy (5.7 percentage points, about a 10% increase). The price sensitivity (slope) does not look different. The magnitude of this effect is not high compared with the amount of extra subsidies spent under the free-distribution case. In order to explain why, we compare three effects between the two subsidy policies: effect of having access to insurance, learning effect, and price anchoring.

First, households who have been insured for one year might be more likely to purchase it again in the second year because of habit formation. This effect may vary with different subsidy policies because more households have the insurance under full subsidy policy. To study the habit formation effect, we estimate the impact of first-year take-up on second-year purchase, using the randomized subsidy policy and default options as instrumental variables for first-year take-up decisions. We fine that, having access to the insurance does not influence either the level or the slope of the demand curve in the following year. This means simply enlarge the coverage rate is not enough.

Second, we explore whether the learning effect is different between the two subsidy policies. There are two types of learning we consider: learning from own experience, and

learning from friends' experience. We find three main results: First, for households who paid for the insurance in the first year, receiving payouts has a positive effect on second year demand, and households are less sensitive to price increase if they received payout; However, for households who received it for free, although receiving payouts have a positive effect on second year take-up, there's no significant effect on the slope of demand curve, and the level effect of receiving payout is much higher under the partial subsidy policy. Second, for households who paid for insurance, observing friends receiving payouts improves second year take-up and makes people less sensitive to price change for households who did not purchased insurance in the first year, but there's no such effect for households who purchased insurance in the first year, regardless of whether they received payout by themselves or not; while for households under the full subsidy policy, observing friends receiving payouts does not have slope effects, and it only have significant level impact on second year demand curve for households who did not want to pay to purchase insurance in the first year and who did not received payout. The magnitude of learning-from-others effect is smaller than that in the non-free sample. What is driving the effect of receiving or observing payout on take-up? We verified that this is not because of trust or income effect, but is mainly because of the learning effect. To explain why is the learning effect smaller under free subsidy policy, we show that people paid less attention to the payout information if they received it for free last year.

Third, to estimate the price anchoring effect, we restrict the sample to households who were willing to purchase the insurance at a 70% subsidy in the first year and are facing higher subsides in the second year, and estimate whether those people are more likely to buy insurance in the second year. However, we did not find any price anchoring effect, and there's no significant difference regarding the anchoring effect between households under different first year subsidy policies.

The findings in this paper suggest that the learning effect under full subsidy policy is much smaller than that under a partial subsidy policy, but the payout rate in the former case is higher, which drives to the similar average take-up rates in the second year under these two policies because. As a result, free distribution subsidy policy performs no better than cost-sharing policy, while the cost of the former is much larger than the later.

The rest of the paper is organized as follows. Section 2 describes the background for the study and the insurance contract. Section 3 explains the experimental design. Section 4 presents the estimation strategies and results, and section 5 concludes.

2. Background

Rice is the most important food crop in China, with nearly 50% of the country's farmers engaged in its production. In order to maintain food security and shield farmers from negative weather shocks, in 2009 the Chinese government requested the People's Insurance Company of China (PICC) to design and offer the first rice production insurance policy to rural households in 31 pilot counties.[3] The program was expanded to 62 counties in 2010 and to 99 in 2011. The experimental sites for this study were 143 randomly selected rice production villages included in the 2010 expansion of the insurance program, located in Jiangxi province, one of China's major rice bowls. In these villages, rice production is the main source of income for most farmers. Because the product was new, no household had ever heard of or purchased such insurance before, and most of them had never interacted with PICC. As a result, farmers, and even government officials at the village or town level, had a very limited understanding of weather insurance products and were unfamiliar with the insurance company.

The insurance contract is as follows. The actuarially fair price is 12 RMB per mu per season.[4] If a farmer decides to buy the insurance, the premium is deducted from the rice production subsidy deposited annually in each farmer's bank account, with no cash payment needed.[5] The insurance covers natural disasters, including heavy rain, flood, windstorm, extremely high or low temperatures, and drought. If any of these natural disasters occurs and leads to a 30% or more loss in yield, farmers are eligible to receive payouts from the insurance company. The amount of the payout increases linearly with the loss rate in yield,

[3] Although there was no insurance before 2009, if major natural disasters occurred, the government made payments to households whose production had been seriously hurt. However, the level of transfer was usually very low and far from sufficient to help farmers resume production.

[4] 1 RMB = 0.15 US$; 1 mu = 0.067 hectare. In the experimental sites, farmers produce two or three crops of rice each year. The actuarially fair price was calculated based on the average probability of disaster and yield information at the national level. It is lower in this particular county.

[5] Starting in 2004, the Chinese government has provided production subsidies to rice farmers in order to give them more production incentives. Each year, subsidies are deposited directly to the farmers' agricultural cards in the rural credit cooperatives (the main rural bank of China).

from 60 RMB per mu for a 30% loss to a maximum payout of 200 RMB per mu[6]. The loss rate in yield is determined by a committee composed of insurance agents and agricultural experts. Since the average gross income from cultivating rice in the experimental sites is between 700 RMB and 800 RMB per mu, and the production cost is around 300 RMB to 400 RMB per mu, the insurance policy covers 25 to 30% of the gross income or 50 to 70% of the production cost.

3. Experimental Design and Data

3.1. Experimental Design

We use a two-year randomized experiment to study the effect of different subsidy policies on insurance demand. The first year experiment was carried out in Spring 2010, and the second year experiment was implemented in Spring 2011. The experimental sites include 134 randomly selected villages in Jiangxi Province with around 3500 households.

In order to promote voluntary purchase of the insurance, the government provides subsidies on the insurance premium. The main treatment in this experiment involves randomization of the level of subsidy. In the first year, we first sell the insurance with a 70% subsidy on the premium to households in all villages. This means farmers pay 3.6 RMB per mu if they want to buy the insurance. After we get the take-up decisions from all households, we randomly divide the 134 villages into two groups. Specifically, 72 villages were assigned with a cost-sharing subsidy policy, where all households pay 30% of the premium if they agreed to buy the insurance. All households in the other 62 villages were surprised an announcement that the insurance will be offered to all farmers for free, so eventually all rice-farmers in those villages receive the insurance for free. As a result, we know who wants to buy the insurance at the 70% level of subsidy in both types of villages. To randomize the subsidy policy on the village level, the sample villages was stratified according to village size (total number of households). In order to generate exogenous variation in individual insurance purchase, we also randomized the default option within each village. If the default was BUY, then the farmer needed to sign off if he or she did not want to purchase the insurance; if the default was NOT BUY, then the farmer had to sign on if he or she decided

[6] For example, consider a farmer who has 5 mu in rice production. If the normal yield per mu is 500kg and the farmer's yield decreased to 250kg per mu because of a windstorm, then the loss rate is 50% and he will receive 200*50% = 100 RMB per mu from the insurance company.

to buy the insurance. The randomized default option will be used as IV for first year insurance purchase decisions together with the randomized subsidy policy.

In the second year, a follow-up experiment was conducted with all households in the sample villages included in the first-year experiment. We randomize the household level subsidy in all villages. The subsidy level ranged from 90% to 40%, and the corresponding final price faced by households varied from 1.2 RMB to 7.2 RMB. Except for the final price, everything else remained the same in the contract as in the first year. In total, eight different prices were offered. Similar as the design in Dupas (2013), only two or three prices were assigned within each village[7]. For example, if one village was assigned with a price set {1.8, 2.6, 5.4}, all sample households in the village were randomly assigned with one of these three prices. To randomize price sets on the village level, the sample of villages was stratified according to village size (total number of households) and first year village-level payout ratio. For price randomization on the household level, the sample within each village was stratified according to rice production area.

In both years, we offer information session about the insurance policy to farmers. Households make insurance purchase decisions individually right after the meeting. In the second year, we not only repeat items in the contract, but also payout made during the first year. Specifically, we announce the list of people in the village who purchased insurance and have received a payout during the first year, so all households know who in the village received a payout and the amount of the payout[8].

3.2. Data and Summary Statistics

At the end of the visit in both years, a census was collected in all villages included in the experimental sites. The analysis of this paper is based on the household survey and the administrative purchase and payout data from the insurance company.

The household survey contains six parts. The first part asks about household characteristics including household size; age and education of the household head; area of rice production; yields and sales; household income from different sources; borrowing; etc.;

[7] Price sets with either two or three different prices were randomly assigned on the village level. For villages assigned with two prices {P1, P2}, P1 <= 3.6 and P2 > 3.6; for villages with three prices {P1, P2, P3}, P1 < 3.6, P2 = {3.6, 4.5}, and P3 > 4.5.

[8] After the insurance was offered in April 2010, low temperature disaster happened in October 2010, just before the harvest of the late season rice, which lead to yield loss for most farmers.

The second part asks about types of natural disasters experienced, loss rate in rice yield in the past three years, and methods of coping with such losses. The third part covers experience in purchasing any kind of insurance, as well as payouts received in the past three years. The fourth part asks about risk attitudes and perceptions about future disasters[9]. The fifth contains questions which test farmers' knowledge of how insurance works and its potential benefits; households' trust of the insurance company regarding loss checking and the payout issuing process. The six part includes a social network survey, in which we ask each household to name five of their closest friends, with whom they most frequently discuss agricultural production and financially-related questions with.

Summary statistics of selected variables are presented in Table 1. In total, 3476 households in 143 villages have been interviewed. According to Panel A in Table 1, household heads are almost exclusively male, and the average education level is between primary and secondary school. Rice production is the main source of household income, accounting for almost 70% of total income on average. Households are risk averse on average. In Panel B, we summarize payout issued during one year after the insurance was provided. Because a windstorm hurt some villages in our sample, on average around 60% households received some payouts, with an average size of payout of around 90 RMB. In villages under cost-sharing subsidy policies, 24% households received payout, and 59% households have at least one friend receiving payouts. In villages under free-distribution subsidy policies, 60% households received some payouts, while 79% households observe at least one of their friends receiving payout. As a result, since more households were coved by insurance in free-distribution policy, most households were able to enjoy the benefits of insurance coverage by themselves, or observing their friends' positive experience with the product. Lastly, in Panel C, we show the overall take-up rate in both years. In the first year the take-up rate is 44%, while that in the second year is higher, about 50%.

Randomization checks are presented in Table 2. To check whether the price randomization is valid, we regress the five main household characteristics (gender, age,

[9] Risk attitudes were elicited by asking sample households to choose between increasing amounts of certain money (riskless option A) and risky gambles (risky option B) in Table A1. The number of riskless options was then used as a measure of risk aversion. The perceived probability of future disasters was elicited by asking "what do you think is the probability of a disaster that leads to more than 30% loss in yield next year?"

household size, education, and area of rice production) on a quadratic in the insurance price and a set of village fixed effects:

$$X_{ij} = \tau_0 + \tau_1 Price_{ij} + \tau_2 Price_{ij}^2 + \eta_j + \epsilon_{ij} \quad (1)$$

Where X_{ij} represents a characteristic of household i in village j, $Price_{ij}$ is the post-subsidy price faced by household i in village j, and η_j includes village dummies. In Table 2, the coefficient estimates and standard errors for τ_1 (column (1)) and τ_2 (column (2)) are reported. All of the coefficient estimates are small in magnitude and none of them is statistically significant, suggesting that the price randomization was valid in both years.

4. Estimation Strategies and Results

4.1 The Aggregate Effect of First-year Subsidies on Second-Year Take-up

To compare the overall effect of first-year subsidy policies on second-year insurance demand, we estimate the following equation:

$$Takeup_{ij2} = \alpha_0 + \alpha_1 Price_{ij2} + \alpha_2 Free_{ij1} + \alpha_3 Price_{ij2} * Free_{ij1} + \alpha_4 X_{ij} + \eta_j + \epsilon_{ij} \quad (2)$$

where $Takeup_{ij2}$ is an indicator of the purchase decision made by household i in village j in the second year, which takes a value of one if the household decided to buy the insurance and zero otherwise. $Price_{ij2}$ is the post-subsidy price faced by household i in village j in year two. $Free_{ij1}$ is a dummy variable which equal to one for households in villages under free-distribution subsidy policy in the first year, and zero otherwise. X_{ij} includes household characteristics such as gender, age, production size, etc., and η_j includes village dummies.

According to results in Table 3, column (1) suggests that the overall second year take-up rate among households under full subsidy policy in the first year is not substantially higher than that of households under partial subsidy policy (5.9 percentage points, about a 10% increase), and adding additional controls does not affect the result significantly (column (2)). In column (3), we show that the price sensitivity (slope) does not look different between households with different first year subsidies. As a result, the magnitude of the difference in second year demand curve is not large compared with the amount of extra subsidies spent under the free-distribution case. In order to explain why, we compare

the following three channels: effect of having access to insurance, learning effect, and price anchoring.

4.2 Decomposing the Aggregate Effect

4.2.1. Habit formation

We run the following regression to test whether households are more likely to buy insurance in the second year if they purchased it in the first year:

$$Takeup_{ij2} = \alpha_0 + \alpha_1 Price_{ij2} + \alpha_2 Takeup_{ij1} + \alpha_3 Price_{ij2} * Takeup_{ij1} + \alpha_4 X_{ij} + \eta_j + \epsilon_{ij} \quad (3)$$

where $Takeup_{ij1}$ is an indicator of the purchase decision made by household i in village j in the first year, which takes a value of one if the household decided to buy the insurance and zero otherwise. Since take-up decisions in the first year are endogenous to the second year purchase behavior, we use first year subsidy policies (cost-sharing or free-distribution) and the randomized default options as the IVs for $Takeup_{ij1}$.

The estimation results are in Table 4. First, column (1) shows that the two IVs significantly affect first year take-up decisions. Second, IV results in columns (4) and (5) suggest that having the insurance for one year does not influence either the level or the slope of the demand curve in the following year. As a result, simply enlarge the coverage rate in the initial year is not sufficient to improve the second take-up rate.

4.2.2. Learning-by-doing and Social Learning

Although first year take-up decisions do not improve second year demand, some specific types of experience of themselves or their friends may still affect long-term decisions. For insurance, the most important type of experience that farmers care about in this context is receiving payouts from the insurance company: after the first year, if disasters happen, farmers can see whether anyone received a payout, whether loss checking and payout issuance are done fairly, etc., which can potentially affect their own long-run insurance demand. As a result, in this part, I measure the effect of receiving payouts or observing friends' payout experience on the second year insurance demand curve.

First, we study the effect of directly receiving payout in the first year on the insurance demand curve in the following year, using the sample of households who paid 30% of the

premium to purchased insurance (cost-sharing villages) and households who were willing to pay 30% to purchase insurance (free-distribution villages) in the first year. In Figure 1.1 and 1.2, I compare the insurance demand curve of households that received payout to the insurance demand curve of households that did not. It shows that people are significantly more likely to renew the contract if they received some payouts in the first year. Furthermore, those who received payouts are less sensitive to subsidy removal in the second year; the insurance demand curve is almost flat for them. However, households who paid to buy insurance react more strongly to payout.

We then estimate whether these effects are statistically significant using the following regression:

$$Takepup_{ij2} = b_0 + b_1 Price_{ij2} + b_2 Payout_{ij1} + b_3 Price_{ij2} * Payout_{ij1} + \eta_j + \epsilon_{ij} \quad (4)$$

where $Payout_{ij1}$ is a dummy equal to one if the household received payout last year. Estimation results are shown in Table 5. Columns (1)-(4) are based on households in villages with partial subsidy policy in the first year, and columns (5)-(8) are based on the sample in villages with full subsidy policy in the first year. First, columns (1)-(2) suggest that receiving payouts improves second year take-up rate by almost 40 percentage points, and mitigates the subsidy removal effect by around 70%. While this effect could be driven by "learning by doing," it could also be explained if people changed their risk altitudes or perceived probability of future disasters after experiencing some weather shocks. In order to control for this effect, I use a regression discontinuity method to re-estimate the effect of receiving payouts on renewal behavior, using loss rate in yield during the first year as the running variable. According to the results in columns (3) and (4) in Table 5, these effects did not cause much change, so the weather shock mechanism can be ruled out as a possible explanation. However, for households who received the insurance for free in the first year, columns (5)-(8) shows that although receiving payouts have a positive effect on second year take-up, there's no significant effect on the slope of demand curve. Comparing households under different first-year subsidy policies, column (9) suggests that the effect of receiving payout is much higher under the partial subsidy policy.

Next, we look at the effect of social learning about friends' payout experience. By definition, a farmer who did not buy insurance in the first year could not directly receive a

payout, so his or her second year behavior cannot be explained as "learning by doing." However, it is possible that households' insurance demand curve changes according to friends' payout experience because households may update their beliefs about the potential benefits of this product or the uncertainty about this program after observing friends' payout experience.

In figures 2.1 and 2.2, we compare the second year insurance demand curve of households who have an above-median fraction of friends receiving payouts in the first year and that of those who have a below-median proportion of friends receiving payouts. We can see that, when a farmer has more friends who received payouts, the insurance demand curve is significantly higher and flatter. Again, this effect is much weaker among households who received the insurance for free in the first year.

To estimate this empirically, I use the following regression:

$$Takeup_{ij2} = \psi_1 Price_{ij2} + \psi_2 NetworkPayout\_high_{ij1} + \psi_3 Price_{ij2} * NetworkPayout\_high_{ij1} + \psi Networktakeup_{ij1} + \psi_4 X_{ij2} + \eta_j + \epsilon_{ij} \quad (5)$$

where $NetworkPayout_{ij1}$ is the proportion of friends in one's social network who have purchased insurance in the first year and received payout[10], and $NetworkPayout\_high_{ij1}$ is a dummy which is defined as one if $NetworkPayout_{ij1}$ is higher than the sample median and zero otherwise.

For households who paid to buy insurance in the first year, we estimate equation (5) using three different samples: the whole sample, only those who purchased insurance in the first year, and only those who did not purchase in the first year. According to columns (1), (3) and (5) in Table 6.1, overall, households who have an above-median proportion of friends receiving payouts are about 20 percentage points more likely to purchase insurance in year two on average. However, the effect is only significant for those who did not purchase insurance in the first year; those who bought insurance in year one do not care about whether other people received payout. Moreover, as shown in columns (2), (4) and (6) of Table 6.1, friends' payout experience affects price sensitivity for all households, regardless of whether a household purchased insurance in the first year. Specifically, observing an above-median proportion of friends receiving payouts mitigates almost 46% of

[10] For example, if a household listed five friends, four of them purchased insurance in year one, and two of them received payouts, then the variable is defined as 2/4 = 0.5.

the negative subsidy removal effect, which is equivalent to the effect of reducing the average insurance premium by around 35%[11]. As a result, the effect of learning from friends' experience on the *level* of insurance demand equals about half the effect of directly receiving payout; the effect of learning from friends' experience on the *slope* of the insurance demand curve equals about 70% of the learning by doing effect. Consequently, observing more friends receiving payouts can substantially influence a farmer's own insurance demand and price sensitivity in future periods.

For households who received insurance for free in the first year, we estimate equation (5) using three different samples: the whole sample, only those who received payout in the first year, and only those who did not receive payout in the first year. According to Table 6.2, the effect of observing friends receiving payout only affect the second year decisions of households who did not receive payout by themselves. This means farmers weights their own experiences more than their friends' experiences. More importantly, comparing Table 6.1 and Table 6.2, the effect of observing friends receiving payout is much smaller in villages under full subsidy policy in the first year.

What factors are driving the impact of self or friends' payout experience on long-term insurance demand? While it could be driven by the learning-by-doing or social learning effect, it's also possible that the effect is induced by improved trust on insurance companies or income effect. We test the trust and income effect in Tables 7-8. The results show that receiving or observing payout does not affect the level of trust on the insurance program, in either type of villages. Moreover, the payout effect does not vary by the level of initial household income in all villages. As a result, the effect is mainly driven by a learning story. Then why do we observe a smaller learning effect in villages under full-subsidy policy, compared with that in villages under partial-subsidy policy? We show in Table 9 that although the attendance rate of second-year information session is not significantly different between villages with different subsidy policy (column (2)), obviously households paid less attention to the payout information if they did not pay for the insurance in the first year (column (1)). This suggest that the outcome of the insurance policy is less salient to

[11] We calculate the price equivalence of the social network effect X by the following formula:

$$X = \frac{Coef(NetworkPayout\_High)\left[\left(1-Coef\left(Price*NetworkPayout_{High}\right))*Mean(Price)\right)\right]}{Coef(Price)[1-Coef(Price*NetworkPayout\_High)*Mean(NetworkPayout\_Highetwork)]}/Mean(Price)$$

households who received the insurance for free, because they paid less attention to the information about how many people received payout after receiving insurance.

4.2.3. Price Anchoring

We next consider whether there's price anchoring effect, which makes the full subsidy less policy less attractive. To identify the anchoring effect, we only keep households who agreed to purchase at 3.6RMB and who were assigned with a price lower than 3.6 RMB. The idea is that among this price range, for fully subsidized households, the price increased in the second year, while for partially subsidized household, the price decreased. If there's anchoring effect, we should see a lower take-up among fully subsidized households. However, regression results in Table 10 show that the difference is small and insignificant. As a result, we do not see a price anchoring effect in our case.

5. Conclusions

This paper uses a two-year randomized field experiment in rural China to analyze and compare the effect of two different short-run subsidy policies on the long-run adoption of a new weather insurance product. We find that free distribution subsidy policy performs no better than cost-sharing policy on improving long-run take-up rates. The results show that overall, first-year subsidy policy does not affect second-year take-up and price sensitivity substantially. In order to explain why, we compare three effects between the two subsidy policies: effect of having access to insurance, learning effect, and price anchoring. We find that the learning effect under a free distribution policy is much smaller than that under a partial subsidy policy, while the payout rate in the former case is higher, which drives to the similar average take-up rates in the second year under these two policies. In addition, we find no price anchoring effect. As a result, free distribution subsidy policy performs no better than cost-sharing policy, while the cost of the former is much larger than the later. A policy implication is that raising the payout rate (two-strike contract) and disseminating information about other people's payout more effectively would be good ways of enhancing long-run adoption and sustainability.

Reference

To be added

Figures and Tables

Figure 1.1. Effect of Own Payout Experience on 2nd Year Insurance Demand
1st Year Partial Subsidy

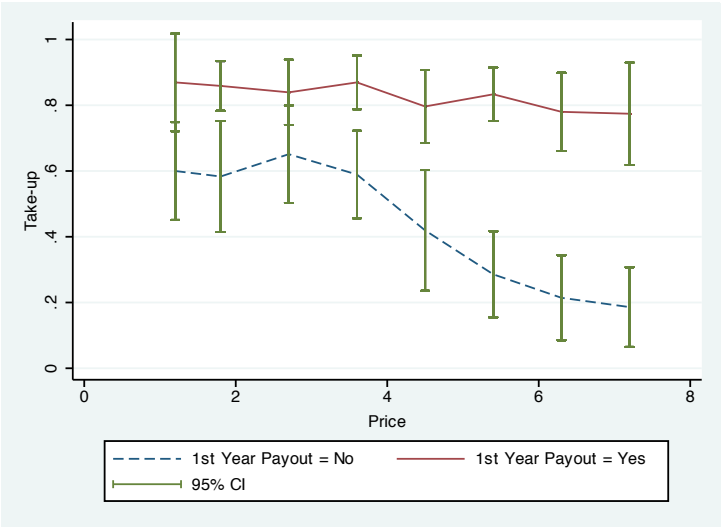

Figure 1.2. Effect of Own Payout Experience on 2nd Year Insurance Demand
1st Year Full Subsidy

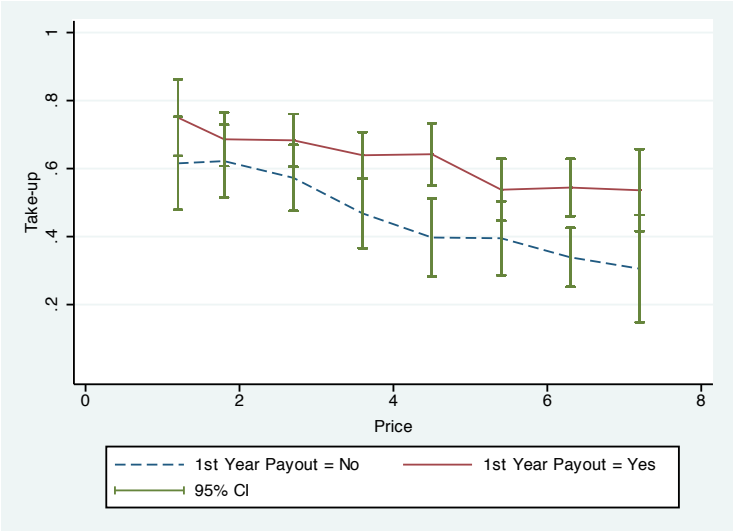

Figure 2.1. Effect of Friends' Payout Experience on 2nd Year Insurance Demand
1st Year Partial Subsidy

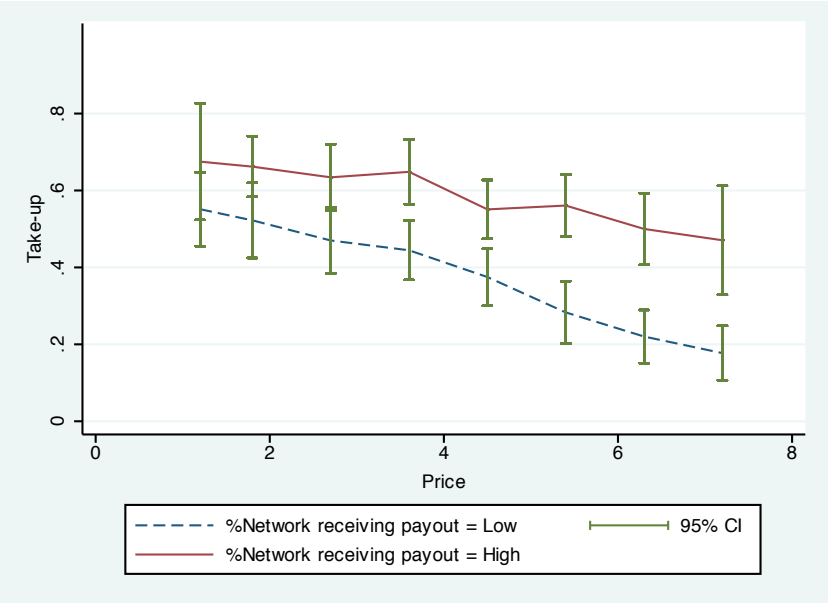

Figure 2.2. Effect of Friends' Payout Experience on 2nd Year Insurance Demand
1st Year Partial Subsidy

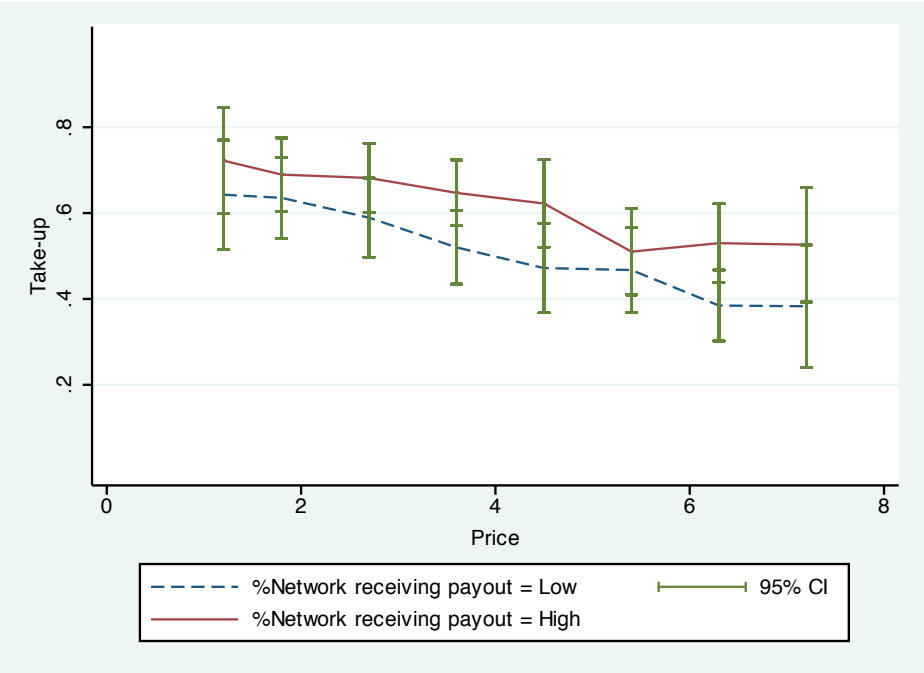

Table 1. Summary Statistics

	Sample Mean			
	Non-free	Free		All
PANEL A: HOUSEHOLD CHARACTERISTICS				
Gender of Household Head (1 = Male, 0 = Female)	0.9732	0.9645		0.9692
	(0.0037)	(0.0046)		(0.0029)
Age	52.8646	53.3298		53.0795
	(0.2678)	(0.3009)		(0.2002)
Household Size	5.1676	5.3023		5.2298
	(0.0543)	(0.0611)		(0.0406)
Education (0 = illiterate, 1 = literate)	0.7162	0.7197		0.7178
	(0.0104)	(0.0112)		(0.0076)
Area of Rice Production (mu)	11.7013	11.274		11.5038
	(0.2979)	(0.2766)		(0.205)
Share of Rice Income in Total Income (%)	67.9222	69.6579		68.7358
	(0.6901)	(0.7991)		(0.5243)
Risk Aversion (0-1, 0 as risk loving and 1 as risk averse)	0.799	0.7928		0.7964
	(0.0077)	(0.0076)		(0.0055)
Perceived Probability of Future Disasters (%)	32.9079	33.3065		33.0919
	(0.3974)	(0.3524)		(0.2688)
PANEL B: INSURANCE PAYOUT				
Payout Rate (#hhs received payout/sample size, %)	24.16	60.19	***	40.79
	(0.99)	(1.22)		(0.83)
Payout Rate Among First Year Buyers (%)	56.71	60.91		58.58
	(1.76)	(1.93)		(1.3)
Amount of Payout Received by First Year Buyers (RMB, per mu)	98.0376	87.4654		93.3413
	(7.2917)	(6.2216)		(4.9057)
Having at Least One Friend Receiving Payouts (1 = Yes, 0 = No)	0.5986	0.7869	***	0.6856
	(0.0113)	(0.0102)		(0.0079)
%Friends Reciving Payouts	56.58	52.33	***	54.51
(#friends receiving payout/#friends covered by insurance)	(1.07)	(0.89)		(0.7)
PANEL C: OUTCOME VARIABLE				
Insurance Take-up Rate (%), Year One	42.6	39.91		41.36
	(1.14)	(1.23)		(0.84)
Insurance Take-up Rate (%), Year Two	49.92	56.26	***	52.85
	(1.16)	(1.24)		(0.85)
No. of Households: 3476				
No. of Villages: 143				

Note: Standard errors are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 2. Price Randomization Check

Sample: All	OLS Coeff on Price (1)	OLS Coeff on Price Squared (2)	P-Value Joint Test (Price and Price Squared) (3)
Gender of Household Head	0.0089	-0.0011	0.6268
(1 = Male, 0 = Female)	(0.0093)	(0.0012)	
Age	0.2693	-0.0306	0.9023
	(0.5983)	(0.0685)	
Household Size	-0.0065	0.0019	0.8932
	(0.128)	(0.0147)	
Area of Rice Production (mu)	0.596	-0.069	0.7012
	(0.7296)	(0.009)	
Literacy	0.0207	-0.002	0.5668
(1 = Yes, 0 = No)	(0.0232)	(0.0027)	
Number of Obs.	3476	3476	3476

Note: This table checks validity of price randomization. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Effect of Free Distribution on Second Year Demand Curve

VARIABLES	Insurance take-up (1 = Yes, 0 = No)		
Sample: All	(1)	(2)	(3)
Price	-0.0487***	-0.0491***	-0.0506***
	(0.00545)	(0.00538)	(0.00765)
Free 1st year	0.0597*	0.0569*	0.0436
(1 = Yes, 0 = No)	(0.0304)	(0.0307)	(0.0510)
Price * Free_1st year			0.00328
			(0.0107)
Age		0.00275***	0.00274***
		(0.000851)	(0.000853)
Male		-0.0289	-0.0285
(1 = Yes, 0 = No)		(0.0514)	(0.0516)
Household size		0.0111***	0.0111***
		(0.00367)	(0.00368)
Literacy		0.0616***	0.0615***
(1 = Yes, 0 = No)		(0.0201)	(0.0201)
Rice production (mu)		0.00255***	0.00256***
		(0.000761)	(0.000761)
Observations	3,474	3,442	3,442
R-squared	0.036	0.047	0.047
P-value of joint significance test: Price and Price*Free			0.0000***
Free and Price*Free			0.1824

Notes: Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Effect of Having Insurance on Second Year Demand Curve

VARIABLES	1st year Access (1 = Yes, 0 = No)	Insurance take-up (1 = Yes, 0 = No)			
Sample: All		OLS		IV	
	(1)	(2)	(3)	(4)	(5)
Price		-0.0477***	-0.0478***	-0.0521***	-0.0409**
		(0.00522)	(0.00972)	(0.00620)	(0.0160)
1st year access		0.209***	0.209***	0.0526	0.130
(1=Yes, 0 = No)		(0.0242)	(0.0537)	(0.0658)	(0.112)
Price * 1st year access			0.000110		-0.0184
			(0.0111)		(0.0238)
Age		0.00232***	0.00232***	0.00196**	0.00200**
		(0.000840)	(0.000843)	(0.000977)	(0.000987)
Male		-0.00897	-0.00895	0.000238	-0.000590
(1 = Yes, 0 = No)		(0.0505)	(0.0504)	(0.0585)	(0.0588)
Household size		0.0109***	0.0109***	0.0127***	0.0127***
		(0.00367)	(0.00367)	(0.00406)	(0.00407)
Literacy		0.0495**	0.0495**	0.0700***	0.0703***
(1 = Yes, 0 = No)		(0.0201)	(0.0201)	(0.0227)	(0.0226)
Rice production (mu)		0.00242***	0.00242***	0.00265***	0.00266***
		(0.000732)	(0.000733)	(0.000792)	(0.000794)
Free 1st year	0.615***				
(1 = Yes, 0 = No)	(0.0274)				
1st year default	0.0755*				
(1 = Yes, 0 = No)	(0.0419)				
Observations	2,730	3,442	3,442	2,701	2,701
R-squared	0.302	0.081	0.081	0.067	0.067
P-value of joint significance test:					
Price and Price*Access			0.0000***		0.0000***
Access and Price*Access			0.0000***		0.4898

Notes: Columns (2)-(3) are OLS estimation results, and columns (4)-(5) are IV results, using free distribution and default in the first round as the IVs for access to insurance in insurance in the first year. Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Compare Effect of Receiving Payouts under Different Subsidy Policies, 1st Year Takeup = 1

VARIABLES	Insurance take-up (1 = Yes, 0 = No)								
Sample: 1st Year Takeup=Yes	*Partial Subsidy (all sample)*				*Full Subsidy (all sample)*				*All Sample*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Price	-0.0500***	-0.0821***	-0.0828***	-0.0787***	-0.0425***	-0.0614***	-0.0608***	-0.0604***	-0.0467***
	(0.00839)	(0.0136)	(0.0136)	(0.0143)	(0.0105)	(0.0197)	(0.0193)	(0.0212)	(0.00646)
Payout	0.398***	0.154*	0.142*	0.191	0.172***	0.0447	0.0375	0.0575	0.366***
(1 = Yes, 0 = No)	(0.0406)	(0.0865)	(0.0849)	(0.115)	(0.0414)	(0.0786)	(0.0760)	(0.109)	(0.0367)
Price * Payout		0.0604***	0.0594***	0.0515***		0.0321	0.0331	0.0340	
		(0.0167)	(0.0165)	(0.0179)		(0.0220)	(0.0215)	(0.0260)	
Free_1st year									0.157***
(1 = Yes, 0 = No)									(0.0493)
Payout*Free_1st year									-0.202***
									(0.0537)
Age	0.00192	0.00179	0.00202	0.00209	0.00345*	0.00342*	0.00374**	0.00413**	0.00262**
	(0.00144)	(0.00141)	(0.00139)	(0.00143)	(0.00175)	(0.00176)	(0.00183)	(0.00182)	(0.00106)
Male	-0.0300	-0.0216	-0.00782	-0.00763	0.0729	0.0752	0.0807	0.116	-0.0304
(1 = Yes, 0 = No)	(0.101)	(0.0974)	(0.0960)	(0.0974)	(0.128)	(0.128)	(0.126)	(0.150)	(0.0749)
Household size	0.00690	0.00604	0.00516	0.00535	-0.00415	-0.00495	-0.00485	-0.00625	0.00295
	(0.00723)	(0.00699)	(0.00681)	(0.00680)	(0.00670)	(0.00669)	(0.00677)	(0.00741)	(0.00487)
Literacy	0.0941**	0.0870**	0.0802**	0.0808**	0.00932	0.0127	0.0153	0.0124	0.0595**
(1 = Yes, 0 = No)	(0.0365)	(0.0349)	(0.0348)	(0.0357)	(0.0422)	(0.0420)	(0.0415)	(0.0422)	(0.0275)
Rice production (mu)	0.000809	0.000491	0.000542	0.000576	0.00456***	0.00476***	0.00456***	0.00488***	0.00222***
	(0.000878)	(0.000881)	(0.000874)	(0.000890)	(0.00141)	(0.00144)	(0.00139)	(0.00149)	(0.000734)
Risk Aversion ([0,1])			0.106**	0.107**			0.0805	0.0691	0.110***
(0-risk loving, 1-risk averse)			(0.0521)	(0.0527)			(0.0556)	(0.0558)	(0.0372)
Perceived Probability of			0.00164**	0.00166**			0.000783	0.000864	0.00140***
Disaster			(0.000722)	(0.000702)			(0.000674)	(0.000693)	(0.000480)
Loss rate in yield				-0.00122				0.00469	
				(0.00329)				(0.00436)	
Square of loss rate in yield				9.16e-06				-6.54e-05	
				(3.19e-05)				(4.57e-05)	
Observations	729	729	729	729	632	632	632	608	1,422
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.261	0.275	0.285	0.285	0.131	0.134	0.138	0.138	0.184
P-value of joint significance									
test: Price and Price*Payout		0.0000***	0.0000***	0.0000***		0.0007***	0.0009***	0.0022**	
Payout and Price*Payout		0.0000***	0.0000***	0.0000***		0.0006***	0.0005***	0.041**	
Payout and Payout*Free									0.0000***
Free and Payout*Free									0.0009***

Note: Columns (1)-(4) tests the effect of receiving payout using the sample households who received partial subsidy in the first year; columns (5)-(8) tests that using households who received full subsidy in the first year; column (9) is based on the whole sample. Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6.1. Effect of Observing Friends Receiving Payouts on Second Year Insurance Demand Curve - Partial Subsidy

VARIABLES	Insurance Take-up (Year two, 1 = Yes, 0 = No)					
Sample: Partial Subsidy in Year One	*All Sample*		*1st Year Take-up = Yes*		*1st Year Take-up = No*	
	(1)	(2)	(3)	(4)	(5)	(6)
Price	-0.0494***	-0.0638***	-0.0500***	-0.0664***	-0.0466***	-0.0647***
	(0.00805)	(0.0109)	(0.00846)	(0.0115)	(0.0106)	(0.0149)
%NetworkPayout_High	0.206***	0.0779	0.0458	-0.0899	0.221***	0.0590
(= 1 if % > median, and 0 otherwise)	(0.0338)	(0.0731)	(0.0386)	(0.0840)	(0.0383)	(0.0975)
Price * %NetworkPayout_High		0.0296*		0.0320*		0.0385*
		(0.0161)		(0.0175)		(0.0205)
Observations	1,627	1,627	665	665	962	962
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.159	0.166	0.300	0.304	0.149	0.154
P-value of Joint-significance: Price and Price*%NetworkPayout_High		0.0000***		0.0001**		0.0001***
%NetworkPayout_high and Price*%NetworkPayout_High		0.0000***		0.0955*		0.0000***

Note: Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6.2. Effect of Observing Friends Receiving Payouts on Second Year Insurance Demand Curve - Full Subsidy

VARIABLES	Insurance Take-up (Year two, 1 = Yes, 0 = No)					
Sample: Full Subsidy in Year One	*All Sample*		*1st Year Payout = Yes*		*1st Year Payout = No*	
	(1)	(2)	(3)	(4)	(5)	(6)
Price	-0.0372***	-0.0423***	-0.0247***	-0.0107	-0.0493***	-0.0550***
	(0.00752)	(0.0120)	(0.00889)	(0.0198)	(0.0115)	(0.0134)
%NetworkPayout_High	0.104***	0.0634	0.00509	0.0849	0.134***	0.0334
(= 1 if % > median, and 0 otherwise)	(0.0303)	(0.0677)	(0.0363)	(0.0993)	(0.0470)	(0.128)
Price * %NetworkPayout_High		0.0101		-0.0195		0.0248
		(0.0156)		(0.0235)		(0.0280)
Observations	1,552	1,552	917	917	635	635
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.097	0.098	0.084	0.085	0.135	0.137
P-value of Joint-significance: Price and Price*%NetworkPayout_High		0.0000***		0.0177**		0.0003***
%NetworkPayout_high and Price*%NetworkPayout_High		0.0044**		0.6922		0.0106**

Note: Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Effect of Receiving or Observing Payouts on Trust

VARIABLES	Trust (0-1)				
	Partial subsidy		Full subsidy		All
Sample:	*1st Year Take-up = Yes*	*1st Year Takeup = No*	*1st Year Take-up = Yes*	*1st Year Takeup = No*	*All sample*
	(1)	(2)	(3)	(4)	(5)
Payout	-0.0541		0.0516		
(1 = Yes, 0 = No)	(0.0377)		(0.0427)		
%NetworkPayout_High		0.0243		0.0347	
(= 1 if % > median, and 0 otherwise)		(0.0259)		(0.0471)	
Free_1st year					-0.0246
(1 = Yes, 0 = No)					(0.0321)
Age	0.00164	0.00443***	0.00622***	0.00231	0.00448***
	(0.00188)	(0.00145)	(0.00182)	(0.00212)	(0.000882)
Male	-0.142	-0.109	-0.105	-0.185*	-0.0941**
(1 = Yes, 0 = No)	(0.114)	(0.110)	(0.106)	(0.110)	(0.0447)
Household size	0.00227	0.00267	-0.00101	0.00374	0.00108
	(0.00762)	(0.00557)	(0.00842)	(0.00700)	(0.00333)
Literacy	-0.0148	0.0699*	0.0583	-0.00429	0.0279
(1 = Yes, 0 = No)	(0.0454)	(0.0370)	(0.0449)	(0.0518)	(0.0205)
Rice production (mu)	-0.000657	-0.00520***	0.000446	-0.00235	-0.00214**
	(0.00123)	(0.00145)	(0.00196)	(0.00219)	(0.000842)
Observations	753	627	905	614	3,317
Village fixed effects	Yes	Yes	Yes	Yes	No
Household characteristics	Yes	Yes	Yes	Yes	Yes
R-squared	0.075	0.046	0.080	0.053	0.038

Note: Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Heterogeneity (income) of the Payout Effect, 1st Year Takeup = 1

VARIABLES	Insurance take-up (1 = Yes, 0 = No)	
Sample: 1st Year Takeup = Yes	*Partial subsidy*	*Full subsidy*
	(1)	(3)
Price	-0.0462***	-0.0417***
	(0.00862)	(0.0113)
Payout	0.405***	0.149***
(1 = Yes, 0 = No)	(0.0594)	(0.0493)
Income (1000 RMB)	0.00685	-0.000910
	(0.00670)	(0.00334)
Payout*Income	-0.00351	0.00181
	(0.00552)	(0.00305)
Age	0.00290**	0.00425**
	(0.00145)	(0.00173)
Male	-0.0181	0.0590
(1 = Yes, 0 = No)	(0.101)	(0.163)
Household size	0.00509	-0.00208
	(0.00699)	(0.00656)
Literacy	0.0883**	0.0215
(1 = Yes, 0 = No)	(0.0376)	(0.0430)
Rice production (mu)	-0.00134	0.00425**
	(0.00210)	(0.00162)
Observations	699	618
Village fixed effects	Yes	Yes
Household characteristics	Yes	Yes
R-squared	0.26	0.134
P-value of joint significance test:		
Payout and Payout*Income	0.0000***	0.0010***
Income and Payout*Income	0.0000***	0.0007***

Note: Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Effect of Subsidy Policies on Attention to the Session

VARIABLES	Answer to payout question (1 = Right, 0 = Wrong)	Attendance (0-1)
Sample: All	(1)	(2)
Free_1st year	-0.194***	-0.0188
(1 = Yes, 0 = No)	(0.0676)	(0.0189)
Age	-0.00269***	-7.70e-05
	(0.000829)	(0.000111)
Male	0.0512	0.00268
(1 = Yes, 0 = No)	(0.0472)	(0.00706)
Household size	-0.00303	-0.00118*
	(0.00357)	(0.000647)
Literacy	0.00625	0.00220
(1 = Yes, 0 = No)	(0.0198)	(0.00271)
Rice production (mu)	0.000801	-9.58e-05
	(0.000747)	(0.000158)
Observations	3,442	3,442
Village fixed effects	Yes	Yes
Household characteristics	Yes	Yes
R-squared	0.146	0.218

Note: Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10. Test Price Anchoring Effect: First Year Takeup = 1, Price <= 3.6

VARIABLES	Insurance take-up (1 = Yes, 0 = No)	
Sample: all price <= 3.6	(1)	(2)
Price	-0.0112	0.00602
	(0.0240)	(0.0329)
Free_1st year	0.0186	0.120
(1 = Yes, 0 = No)	(0.0378)	(0.0798)
Price * Free_1st year		-0.0405
		(0.0357)
Observations	745	745
Household characteristics	Yes	Yes
R-squared	0.019	0.020
P-value of joint significance test:		
Price and Price*Free		0.312
Free and Price*Free		0.3037

Note: Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A1. Compare Effect of Amount of Payouts under Different Subsidy Policies, 1st Year Takeup = 1

VARIABLES	Insurance take-up (1 = Yes, 0 = No)				
Sample: 1st Year Take-up = Yes	*Partial subsidy*		*Full subsidy*		*All*
	(1)	(2)	(3)	(4)	(5)
Price	-0.0545***	-0.0651***	-0.0391***	-0.0466***	-0.0465***
	(0.00898)	(0.0112)	(0.0101)	(0.0130)	(0.00663)
Amount of Payout (1000 RMB)	0.392***	-0.163	0.386***	0.0554	0.387***
(1 = Yes, 0 = No)	(0.121)	(0.299)	(0.104)	(0.188)	(0.104)
Amount of Payout*Price		0.139**		0.0885	
		(0.0634)		(0.0684)	
Free_1st year					0.0436
(1 = Yes, 0 = No)					(0.0460)
Amount of payout*Free					0.0148
					(0.147)
Age	0.00196	0.00183	0.00333*	0.00327*	0.00237**
	(0.00155)	(0.00155)	(0.00184)	(0.00184)	(0.00109)
Male	0.0209	0.0231	0.0794	0.0771	0.00418
(1 = Yes, 0 = No)	(0.101)	(0.101)	(0.118)	(0.118)	(0.0708)
Household size	0.0101	0.00985	-0.00415	-0.00457	0.00567
	(0.00801)	(0.00807)	(0.00662)	(0.00665)	(0.00539)
Literacy	0.0923**	0.0896**	0.00467	0.00640	0.0615**
(1 = Yes, 0 = No)	(0.0420)	(0.0419)	(0.0409)	(0.0406)	(0.0291)
Rice production (mu)	-0.000262	-0.000976	0.00402***	0.00402***	0.00149*
	(0.00109)	(0.00107)	(0.00135)	(0.00135)	(0.000886)
Risk Aversion ([0,1])	0.146**	0.152***	0.0684	0.0656	0.122***
(0-risk loving, 1-risk averse)	(0.0568)	(0.0561)	(0.0586)	(0.0578)	(0.0411)
	0.00274***	0.00265***	0.000754	0.000753	0.00208***
Perceived Probability of Disaster	(0.000662)	(0.000680)	(0.000678)	(0.000675)	(0.000484)
Observations	729	729	632	632	1,422
Village fixed effects	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes
R-squared	0.156	0.161	0.121	0.123	0.115
P-value of joint significance test:					
Price and Price*Payout		0.0000***		0.001***	
Payout and Price*Payout		0.0000***		0.0025**	
Payout and Payout*Free					0.0000***
Free and Payout*Free					0.0009***

Note: Columns (1)-(2) tests the effect of receiving payout using the sample households who received partial subsidy in the first year; columns (3)-(4) tests that using households who received full subsidy in the first year; column (5) is based on the whole sample. Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A2.1. Effect of Observing Friends Receiving Payouts on Second Year Insurance Demand Curve - Partial Subsidy

VARIABLES	Insurance Take-up (Year two, 1 = Yes, 0 = No)					
Sample: Partial Subsidy in Year One	*All Sample*		*1st Year Take-up = Yes*		*1st Year Take-up = No*	
	(1)	(2)	(3)	(4)	(5)	(6)
Price	-0.0493***	-0.0574***	-0.0498***	-0.0700***	-0.0448***	-0.0522***
	(0.00769)	(0.0141)	(0.00857)	(0.0160)	(0.0100)	(0.0160)
Network Payout	0.240***	0.184**	-0.00423	-0.123	0.285***	0.230**
(= 1 if Yes, = 0 if No)	(0.0440)	(0.0854)	(0.0613)	(0.0986)	(0.0456)	(0.0937)
Price *Network Payout		0.0126		0.0279		0.0129
		(0.0165)		(0.0179)		(0.0194)
Observations	1,623	1,623	663	663	960	960
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.166	0.172	0.303	0.306	0.168	0.169
P-value of Joint-significance: Price and Price*Network Payout		0.0000***		0.0000***		0.0002***
Network Payout and Price*Network Payout		0.0000***		0.3019		0.0000***

Note: Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A2.2. Effect of Observing Friends Receiving Payouts on Second Year Insurance Demand Curve - Full Subsidy

VARIABLES	Insurance Take-up (Year two, 1 = Yes, 0 = No)					
Sample: Full Subsidy in Year One	*All Sample*		*1st Year Payout = Yes*		*1st Year Payout = No*	
	(1)	(2)	(3)	(4)	(5)	(6)
Price	-0.0371***	-0.0433***	-0.0247***	-0.0268	-0.0490***	-0.0426***
	(0.00753)	(0.0123)	(0.00889)	(0.0346)	(0.0114)	(0.0141)
Network Payout	0.112***	0.0798	-0.00175	-0.0109	0.0593	0.104
(= 1 if Yes, = 0 if No)	(0.0391)	(0.0672)	(0.0852)	(0.150)	(0.0452)	(0.0991)
Price * Network Payout		0.00777		0.00216		-0.0111
		(0.0138)		(0.0355)		(0.0201)
Observations	1,552	1,552	917	917	635	635
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.094	0.094	0.084	0.084	0.126	0.127
P-value of Joint-significance: Price and Price*Network Payout		0.0000***		0.0263		0.0003***
Network Payout and Price*Network Payout		0.0197**		0.9973		0.4065

Note: Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A3.1. Effect of Observing Friends Receiving Payouts on Second Year Insurance Demand Curve - Partial Subsidy

VARIABLES	Insurance Take-up (Year two, 1 = Yes, 0 = No)					
Sample: Partial Subsidy in Year One	All Sample		1st Year Take-up = Yes		1st Year Take-up = No	
	(1)	(2)	(3)	(4)	(5)	(6)
Price	-0.0534***	-0.0508***	-0.0499***	-0.0462***	-0.0512***	-0.0524***
	(0.00786)	(0.00879)	(0.00860)	(0.0108)	(0.0105)	(0.0116)
Amount of Network Payout	0.0311	0.0887	0.0169	0.102	0.0704	0.0323
(1000 RMB)	(0.0554)	(0.116)	(0.0646)	(0.163)	(0.0892)	(0.164)
Price *Amount of Network Payout		-0.0129		-0.0178		0.00886
		(0.0217)		(0.0273)		(0.0331)
Observations	1,614	1,614	663	663	951	951
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.120	0.127	0.303	0.304	0.103	0.104
P-value of Joint-significance: Price and Price*Amount of Network Payoutt		0.0000***		0.0000***		0.0000***
Amount of Network Payout and Price*Amount of Network Payout		0.7391		0.8072		0.7187

Note: Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A3.2. Effect of Observing Friends Receiving Payouts on Second Year Insurance Demand Curve - Full Subsidy

VARIABLES	Insurance Take-up (Year two, 1 = Yes, 0 = No)					
Sample: Full Subsidy in Year One	All Sample		1st Year Payout = Yes		1st Year Payout = No	
	(1)	(2)	(3)	(4)	(5)	(6)
Price	-0.0377***	-0.0412***	-0.0247***	-0.0171	-0.0493***	-0.0584***
	(0.00773)	(0.00951)	(0.00892)	(0.0115)	(0.0117)	(0.0128)
Amount of Network Payout	0.00972	-0.0394	-7.17e-05	0.0885	-0.0464	-0.222
(1000 RMB)	(0.0388)	(0.0723)	(0.0433)	(0.0790)	(0.0661)	(0.136)
Price * Amount of Network Payout		0.0120		-0.0219		0.0418*
		(0.0141)		(0.0201)		(0.0241)
Observations	1,552	1,552	917	917	635	635
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.088	0.088	0.084	0.085	0.125	0.127
P-value of Joint-significance: Price and Price*Amount of Network Payout		0.0000***		0.0149**		0.0001***
Amount of Network Payout and Price*Amount of Network Payout		0.6575		0.5197		0.2272

Note: Robust clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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Do acute health care needs of the poor crowd out their chronic care utilization? Evidence from Rural India

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Abstract

Acute illnesses tend to get higher priority when the poor seek care in India. In rural areas, much of the public health facilities are better equipped to provide treatment for either acute illnesses or reproductive and child health needs. As a result, to receive health care for a chronic illness requires one to travel to secondary or tertiary hospitals located in sub-division or district headquarters or in the big cities. Consequently, treatment of chronic illness goes under-utilized and when accessed becomes relatively much more expensive for those seeking care. This paper tests whether there is a crowd-out effect on treatment of chronic illnesses because households ration the amount they spend on care for chronic illness, especially in poor households, and whether the most common victims of such rationing are women and elderly. Preliminary descriptive results seem to support our hypothesis.

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Introduction

Non-communicable chronic diseases (NCD) are the leading causes of death globally. It is estimated that 63% of global deaths were due to NCD. Nearly 80% of NCD deaths occur in low and middle income countries [Wang, Lim and Wu 2012]. On top of the unfinished agenda of infectious diseases in low- and middle-income countries, development, industrialization, urbanization, globalization and increased life expectancy are drivers of an epidemic of NCD.

NCD, particularly cardiovascular diseases, mental health disorders, diabetes and cancer are important public health problems in India, fast becoming responsible for a major proportion of mortality and morbidity when infectious diseases have not yet been controlled completely. Demographic changes and changes in lifestyle along with increased rates of urbanization are the major reasons responsible for the tilt towards NCD [Upadhyay 2012]. In developed countries, research has shown that lower socio-economic groups experience higher mortality rates from NCD. To the contrary, in India till the 1990s, NCD were mostly regarded as the disease of the affluent classes (Chadha, 1990). This has changed significantly in the last two decades where socio-economically disadvantaged groups have become more vulnerable. Risk of NCD such as cardio-vascular diseases are higher among the lower socio-economic classes (Xavier et al 2008). Malnutrition and infection in early life also increase the risk of NCD in later life and in adult life making the poor even more vulnerable. [Bygbjerg 2012]. The effects of NCD are inequitable with higher risk factors among lower socio-economic groups and greater financial implications for the poorer households in India [Thakur et al 2011]. Research has shown that although NCD prevalence is higher in urban areas, rural India is not far behind (Office of Registrar General 2009).

Health care is provided by both public and private providers but funded mostly by out-of-pocket spending by care-seekers in India. Acute illnesses tend to get higher priority due to the immediate nature of such health episodes and a larger share of out of pocket spending occurs on treating acute conditions (Dror 2008). Following much of the literature, morbidity episodes have been classified into broadly chronic ailments such as diabetes and hypertension and acute ailments such as malaria, hepatitis, diarrhea, etc. Much of the public health networks in rural areas are better equipped to provide treatment for acute illnesses or reproductive and child health needs. The technology and medication needed for chronic care is available mostly in hospital settings (Thakur 2011). Therefore, to get the right kind of care for a chronic NCD one has to travel to secondary or tertiary hospitals located in sub-division or district

headquarters or in the big cities. Consequently, treatment of chronic illness becomes relatively much more expensive.

Many of the chronic illnesses are prevalent among the elderly population who are mostly non-earning members of the household. As a result of higher life expectancy and large spousal age gap at marriage, elderly women tend to live longer with these chronic ailments. With limited resources that can be spent on health care, it is likely that not only is there a crowding-out of treatments for chronic illnesses but also when households ration the amount they spend on chronic NCDs, especially in poor households, the individuals facing such rationing are mostly the women and elderly. This paper tests this hypothesis with data on chronic and acute illnesses collected from rural households in the Birbhum district of West Bengal.

Why Birbhum?

India is in a socio economic and demographic transition. The health care priorities of the country are changing. On the one hand India still has a significant burden of communicable disease, some of which are emerging and "re"emerging posing new challenges. In addition, a broad range of non-communicable diseases (NCDs)- once considered a burden of the affluent western nations- are evolving . While the basic biology of such health conditions are same globally, many of these have social, economic and lifestyle correlates that vary regionally and influenced by cultural and ethnic factors. In addition, all these characteristics also keep on changing over a period of time. Catching the dynamics of such interactions over a time frame can provide clues to biology, economic impact of the health problem and guide health planning-prioritization and resource allocation. In order to derive population based health and socio-economic information, the need for a population laboratory had been recognized as a global necessity. There had been several such attempts in developing countries, beginning with Matlab in Bangladesh and Framingham in United States. To fulfill this need, it was felt that having a well-defined cohort of people whose health behavior is observed and analyzed in a scientific manner over a period of time would provide valuable information for critical policy-making. To that end, the Government of West Bengal initiated a project called "Health and Demographic Surveillance System" in 2008 in the district of Birbhum[4]. This has initially been set out for a project duration of five years with the vision to create an institution which would

[4] http://www.shds.in/, accessed Sep 30, 2013.

improve the pursuit of knowledge and culture for innovations in population based health research to ultimately improve health services, especially in a rural setting.

Data

Data from 2012 Birbhum Population survey collected by the Birbhum Population project is used for this analysis. Birbhum district is an administrative unit in the Indian state of West Bengal. It is the northernmost district of Burdwan division — one of the three administrative divisions of West Bengal. Birbhum is one of the 19 districts of West Bengal sharing 5.12 per cent of the land area of the state but 3.76 per cent of its total population, indicating a relatively lower density of population per square kilometer in the district (663) vis-à-vis the state (903). Between 1991 and 2001 census years, the population of Birbhum increased from 25.56 lakhs to 30.15 lakhs registering 18.0 per cent decennial growth rate, which is marginally higher than the growth rate of population in West Bengal as a whole (17.8 per cent). Hindus form around 65% of the population according to 2001 census. Muslims are about 33% of the population. There is a sprinkling of other religious groups in the population. According to the 2001 census, 29.5% of the population belongs to the scheduled castes and 6.7% to the scheduled tribes. 32 Other than those speaking the local dialect of Bengali, there are tribal Santhals and ten other tribal communities in Birbhum with some presence, amongst whom Koda, Mahali and Oraons are more common. The district has a population density of 771 inhabitants per square kilometre (2,000 /sq mi) .Its population growth rate over the decade 2001-2011 was 16.15 %. Birbhum has a sex ratio of 956 females for every 1000 males, and a literacy rate of 70.9 % (http://www.shds.in/life_in_birbhum.html).

Survey data was gathered on general morbidity, health consumption and health expenditure at the household and individual level. Within Birbhum district, 4 rural blocks were selected based on diversity in socio-economic profile. Villages were selected by stratified random sampling from within the selected four blocks and in selected villages all households were surveyed. The sample size is 63031 individuals and 12557 households.

Methods

We use both bivariate analyses and multivariate regression models although this preliminary draft primarily reports just the descriptive statistics. Household level data have been

categorized according to poor, lower middle, middle and upper middle/rich class according to per capita consumption expenditure. Household characteristics, incidence of illness, proportion of ill seeking care and proportion of hospitalization have been categorized according to economic class, age and gender to understand disease prevalence, care seeking behavior and gaps in health utilization according to economic class, age and gender. In addition, this paper will look into incidences of co-morbidities and the crowd-out effect. The following statistical analysis will be carried out:

1. Probit models will be used to assess the likelihood of seeking care for chronic illness if there is an acute symptom at the same time both at the individual and household level.
2. To test crowd-out effect at the household level, distance to the nearest tertiary hospital will be considered as a shadow price for cost of chronic illness. We will examine the price effect on chronic care utilization by the household if a household member has an acute episode at the same time.
3. We will disaggregate this further by looking at the price effect on chronic care utilization of elderly or women within the household if a non-elderly or male member has an acute episode.

The survey collected information on self-reported illness episodes. These illness episodes were coded in 45 categories according to the respondent's description of their illness episode. The 'other' category coded some illnesses that overlapped with other categories. All these episodes were further categorized into five categories: acute (such as malaria, typhoid), chronic (such as cancer, hypertension), accidents, reproductive and routine care with the help of an author, a physician (AB). A table showing these classifications is included in the appendix.

Results

Sample description

Households in this rural sample lie mainly in lower middle to poor section of the income distribution (Table 1). The majority of households do not have toilets, drainage or gas connection. Although 78% of all households have electricity, there are differences by economic class; while only 60% of poor households have electricity while 96% of the upper middle class households do. Only quarter to a third of the sample live in pucca structures. While 70% of the

upper class has pucca structures, only 6-12% of poor households have pucca structures (Table 2). Hence there are substantial class disparities in basic standards of living.

Descriptive results

Preliminary descriptive results indicate that the more affluent report higher incidences of illness compared to the poor. Incidence of chronic illness is much lower compared to acute illnesses (Table 3). Disaggregating by gender in Table 4, women report significantly higher incidence of illness (both acute and chronic) compared to men. The elderly (aged 60 years and above) report higher incidence of chronic illnesses compared to all other age groups. While at all age groups, incidence of acute illness is greater, this is the opposite among the 60+ age group, for whom chronic disease incidence is greater than acute symptoms (Table 5) as expected.

Overall, greater proportion of ill respondents reported seeking care for acute illnesses (Table 6). Although women report greater incidence of illness, results (in Table 7) show significantly lower health utilization as well as hospitalization for women compared to men. Consistently, there is lower health utilization for chronic illnesses compared to acute illnesses. This is evident starkly for the elderly group for whom although they report greater chronic illnesses, there is a huge drop-off of on seeking care for chronic illnesses. According to Table 8, a greater proportion of elderly seek care for their acute illnesses. In terms of hospitalization though, elderly tend to be hospitalized more for their chronic diseases than acute ailments (Table 11).

Table 12 and 13 provide evidence that chronic illnesses get treated less than acute illnesses across all income categories but in a much smaller proportion for those living in poor households. While there are fewer hospitalizations associated with chronic illnesses compared to acute illnesses across all income groups, the poorest have the lowest rates of hospitalization compared to all other income classes.

Statistical analysis

Table 14 reports a preliminary ordinary lease squares regression model that looks at correlation between expenditure on chronic illness by households when there are expenses incurred for acute illnesses concurrently. There is a significant negative correlation between the two suggesting that households spend less on chronic illness when they have to spend on acute illnesses. Chronic illness expenses are less on females compared to males, higher for older age groups and higher in higher socio-economic classes.

Discussion

Preliminary results support our hypothesis that acute illnesses get higher attention than chronic illnesses. However, we also see higher reports of acute illness episodes. While women report greater incidence of both acute and chronic illness compared to men, health utilization is lower for women than men. Although elderly report greater chronic illnesses, health care utilization is higher for acute illnesses. These differences are greater among poorer socio-economic classes. This suggests that the poor seem to seek care for acute illnesses crowding out care for chronic illness and the women and the elderly seem to be mostly affected by these trade-offs made at the household level.

While these results point to the nature of household decisions being made at a point in time when they are faced with dual morbidities, the important question is that with the rise of NCDs, is the supply side being able to cope with providing adequate care for the rural populations? Is the system set up in a way that seeking care for acute illnesses get priority because this kind of care is readily available and care for chronic conditions is almost non-existent in rural setting, creating the underlying preference at a systemic level?

Perhaps if there is strong evidence of various combinations of co-morbidity, these will provide crucial policy implications around delivery and access to appropriate care in rural areas. For example, when a patient visits a health facility for a particular acute illness, it would be an efficient targeting if his/her other chronic health care need could be taken care of at that point.

Poverty and chronic non-communicable diseases are inextricably linked. Evidence shows that to have an impact on the burden of chronic diseases, interventions must occur at three levels: population-wide policies, community activities and health services. The latter include both preventive services and appropriate care for persons with chronic conditions.

The accelerating epidemics of non-communicable diseases in India call for a comprehensive public health response which can effectively combat and control them before they peak and inflict severe damage in terms of unaffordable health, economic and social costs. Interventions influencing behavioral risk factors (like unhealthy diet, physical inactivity, tobacco and alcohol consumption) though policy, public education or a combination of both have been demonstrated to be effective in reducing the non-communicable diseases risk in population as well as individuals. Policy interventions are also effective in reducing the levels of several major biological risk factors linked to NCDs (high blood pressure, overweight and obesity; diabetes and abnormal blood cholesterol). Secondary prevention along the lines of

combinations of pills and ensuring evidence based clinical care are also critical. Though the evidence for health promotion and primary prevention are weaker, policy interventions and secondary prevention when combined with these are likely to have a greater impact on reducing national NCD burden. [Singh, Reddy and Prabhakaran 2011]

Although a wide range of cost-effective primary and secondary prevention strategies for chronic diseases are available, their coverage is generally low, especially in poor and rural populations. Much of the care for chronic diseases and injuries is provided in the private sector and can be very expensive. Integration of national programs for various chronic diseases and injuries with one another and with national health agendas [Patel 2011] would be the key to solving resource problems at the same time achieving public health goals.

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Table 1: Definition of economic class used in the analysis

Economic class	N	Per capita consumption expenditure (Rs)			distribution of population (column wise)
		Range	Mean	Median	
poor	19,849	0-782	580	610	31.49
lower middle	31,124	783-1564	1087	1049	49.38
Mid middle	9,166	1565-3131	2070	1559	14.54
upper middle /rich	2,891	>=3132	5468	4278	4.59
All	***63030***				***100***

Source: Birbhum Population Survey 2012

Table 2: Household characteristics by economic class

	poor	low mid	mid middle	up mid/rich	total
distribution of socio-religious class (row wise)					
ST	62.0	33.3	3.9	0.8	100
SC	31.6	54.2	11.7	2.5	100
Muslim	22.4	57.3	16.1	4.2	100
Others	8.7	49.5	30.4	11.4	100
total	25.7	51.8	17.3	5.2	100
percentage of HH without toilet	94	79	48	27	75
percentage of HH without drainage	89	76	50	32	72
percentage of HH with LPG connection	1	4	23	43	9
percentage of HH having electricity	60	81	91	96	78
percentage of HH having pucca floor	12	27	55	71	30
percentage of HH having pucca wall	9	22	47	66	25
percentage of HH having pucca roof	6	17	42	62	21

Source: Birbhum Population Survey 2012

Table 3: Prevalence of illness per 1000 population

Economic class	Acute	Acute / acute symptoms of chronic	All acute	Communicable chronic	Non-communicable chronic	All chronic	All illness
poor	99	8	107	1	25	26	133
lower middle	123	16	139	1	40	41	181
Mid middle	112	23	136	1	48	49	185
upper middle /rich	105	16	121	1	51	51	172
All	113	15	128	1	37	38	166

Source: Birbhum Population Survey 2012

Table 4: Prevalence of illness per 1000 population by gender group

Economic class	Acute	Acute / acute symptoms of chronic	All acute	Communicable chronic	Non-communicable chronic	All chronic	All illness
male							
poor	91	4	95	1	20	21	116
lower middle	116	11	127	2	31	33	160
Mid middle	105	15	120	1	34	35	155
upper middle /rich	104	13	117	1	43	44	161
All	106	10	116	1	29	30	146
Female							
poor	107	11	118	1	30	31	149
lower middle	129	22	151	1	49	50	201
Mid middle	120	31	151	1	62	63	215
upper middle /rich	106	18	124	0	58	58	183
All	119	20	139	1	45	46	185

Source: Birbhum Population Survey 2012

Table 5: Prevalence of illness per 1000 population by gender group

Economic class	Acute	Acute / acute symptoms of chronic	All acute	Communicable chronic	Non-communicable chronic	All chronic	All illness
0-12 years							
poor	151	2	153	0	4	4	157
lower middle	181	4	185	0	9	9	194
Mid middle	175	32	207	0	13	13	220
upper middle /rich	229	14	242	0	7	7	249
All	169	6	175	0	7	7	182
13-39 years							
poor	63	7	71	1	16	17	88
lower middle	96	18	114	1	27	28	142
Mid middle	95	19	114	1	25	26	140
upper middle /rich	8	10	18	0	20	20	38
All	82	14	96	1	23	24	120
40-59 years							
poor	112	18	130	2	64	66	196
lower middle	127	27	154	3	79	82	236
Mid middle	119	32	152	2	80	82	234
upper middle /rich	108	25	133	1	91	92	225
All	120	26	146	2	76	79	225
60 years and above							
poor	95	12	107	1	152	153	260
lower middle	109	18	126	4	229	233	359
Mid middle	95	26	121	2	217	219	340
upper middle /rich	74	20	94	3	268	272	366
All	100	18	118	3	207	210	328

Source: Birbhum Population Survey 2012

Table 6: percentage of illness which sought health care

Economic class	Acute	Chronic	All
poor	89	82	88
lower middle	92	86	91
Mid middle	95	94	95
upper middle /rich	94	93	94
All	***92***	***87***	***91***

Source: Birbhum Population Survey 2012

Table 7: percentage of illness which sought health care by sex group

Economic class	Male			Female		
	Acute	Chronic	All	Acute	Chronic	All
poor	91	84	90	88	81	87
lower middle	94	85	92	91	86	90
Mid middle	94	95	95	96	94	95
upper middle /rich	95	94	95	93	92	93
All	***93***	***87***	***92***	***91***	***87***	***90***

Source: Birbhum Population Survey 2012

Table 8: percentage of illness which sought health care by age group

Economic class	Acute	Chronic	All	Acute	Chronic	All
	0-12 years			13-39 years		
poor	92	86	92	88	85	88
lower middle	95	88	94	92	86	91
Mid middle	90	100	91	95	97	95
upper middle /rich	94	100	95	504	86	280
All	***93***	***90***	***93***	***95***	***87***	***94***
	40-59 years			60 years and above		
poor	86	79	84	89	46	64
lower middle	90	85	88	89	42	59
Mid middle	94	91	93	93	54	68
upper middle /rich	92	94	93	96	42	56
All	***90***	***85***	***89***	***90***	***46***	***61***

Source: Birbhum Population Survey 2012

Table 9: Incidence of hospitalization per 1000 population for different types of illnesses by economic class

Economic class	Acute	Acute / acute symptoms of chronic	All acute	Communicable chronic	Non-communicable chronic	All chronic	All
poor			15			6	21
lower middle			22			12	34
Mid middle			26			24	50
upper middle /rich			27			27	54
All			20			13	33

Source: Birbhum Population Survey 2012

Table 10: Incidence of hospitalization per 1000 population for different types of illnesses by economic class

Economic class	Male			Female		
	acute	Chronic	All	acute	Chronic	All
poor	16	6	22	13	7	21
lower middle	22	11	33	22	12	34
Mid middle	27	24	51	25	23	49
upper middle /rich	34	29	63	21	25	45
All	21	12	34	19	13	32

Source: Birbhum Population Survey 2012

Table 11: Incidence of hospitalization per 1000 population for different types of illnesses by economic class

Economic class	acute	Chronic	All	acute	Chronic	All
	0-12 years			13-39 years		
poor	16	2	18	11	4	14
lower middle	19	6	25	20	8	29
Mid middle	29	4	33	21	15	36
upper middle /rich	31	14	44	25	14	39
All	19	4	23	18	8	26
	40-59 years			60 years and above		
poor	19	17	36	25	20	45
lower middle	26	18	44	27	35	63
Mid middle	26	35	61	46	60	107
upper middle /rich	24	33	57	47	81	128
All	24	22	46	31	38	70

Source: Birbhum Population Survey 2012

Table 12: Percentage of households which reported treated illness episodes and average and median treatment expenditure incurred by households which reported the illness episodes by economic classes.

Economic class	Acute illness			Chronic illness		
	HHs with treated episodes (%)	Mean expenditure (Rs)	Median expenditure (Rs)	HHs with treated episodes (%)	Mean expenditure (Rs)	Median expenditure (Rs)
poor	42.1	286	125	12.3	636	250
lower middle	44.3	474	200	15.2	860	366
Mid middle	40.3	559	250	17.7	1379	555
upper middle /rich	34.0	766	350	17.8	1647	700
All	42.6	453	200	15.0	967	400

Source: Birbhum Population Survey 2012

Table 13: Percentage of households which reported hospitalization and mean and median hospitalization expenditure incurred by households which had hospitalisation by economic classes.

Economic class	Acute illness			Chronic illness		
	HHs with hospitalisati on (%)	Mean expenditure (Rs)	Median expenditure (Rs)	HHs with hospitalisati on (%)	Mean expenditure (Rs)	Median expenditure (Rs)
poor	7.8	2064	1458	3.8	3788	2210
lower middle	9.1	3829	2100	5.1	7346	4000
Mid middle	9.8	5650	2600	8.3	17335	11200
upper middle /rich	9.6	8897	3100	9.6	68292	12300
All	8.9	4066	200	5.5	14832	5300

Source: Birbhum population survey 2012

Table 14: OLS regression with log of expenditure for chronic illnesses as dependent variable

	Coefficient	P-value
Constant	6.8059	0.000
Ln(expenditure on acute illness)	-0.9355	0.000
Sex		
Male (Reference)	---	---
Female	-0.0930	0.012
Age Group		
0-12 years (Reference)	---	
13-39 years	0.3978	0.000
40-59 years	0.8012	0.000
60 years & above	1.0686	0.000
Economic Status		
Poor (Reference)	---	---
Lower Middle	0.3088	0.000
Mid middle	0.7402	0.000
Upper Middle / Rich	0.8329	0.000
N	10049	
Adjusted R squared	0.79	

Appendix: Table showing classification of illness episodes

code	health care	Acute	chronic	Total	Final Type *
1	RtI	3,516	14	3,530	a
2	Malaria	15	0	15	a
3	Filaria	4	28	32	c c
4	Fever	806	1	807	a
5	typhoid	48	0	48	a
6	Diarrhoea	426	2	428	a
7	Dysentry	81	3	84	a
8	Antrik	453	0	453	a
9	Hepatitis	70	5	75	a
10	tB	13	18	31	c c
11	UtI	31	8	39	a
12	StI	3	4	7	c c
13	Eye inf	56	11	67	a
14	Anemia	81	52	133	nc c
15	cancer	0	9	9	nc c
16	cerebral Stroke	7	10	17	nc c
17	Eye problem	59	29	88	nc c
18	Measles	38	0	38	a
19	chicken pox	162	0	162	a
20	Mumps	36	0	36	a
21	Polio	1	1	2	c c
22	Skin disease	323	65	388	nc m
23	Ear inf	71	19	90	a
24	Malnutrition	283	43	326	nc c
25	Asthma	19	277	296	m
26	Diabetes	2	45	47	nc c
27	Mental	25	85	110	nc c
28	Newro disorder	19	60	79	nc c
29	Dental problem	233	14	247	a
30	HtN/IHD	63	227	290	nc c
31	Gastro	556	203	759	nc m
32	Kidney	19	26	45	nc c
33	chronic bone	12	531	543	nc c
34	Geratric	19	22	41	nc c
35	Abortion	8	0	8	r
36	Gynae	77	77	154	nc m
37	Accidental	385	20	405	i

38	Headache	98	46	144	nc m
39	Abdominal	171	54	225	a
40	ANc	73	0	73	r
41	PNc	3	0	3	r
42	child check up	1	0	1	u
43	Immunization	1	0	1	r
44	Routine check up	4	10	14	u
45	Others (specify)	317	180	497	

***Final Type: Acute=a; Chronic=c; chronic non communicable = c nc/nc c; chronic communicable = c c; chronic but can have acute manifestation = m /nc m; reproductive or child health care = r; injury/accident=I; routine check up = u**

SMS messages Increase Adherence to RDTs Results Among Malaria Patients Seeking Care in the Private Sector: Results from a Pilot Randomized Control Study in Nigeria

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ABSTRACT

Background: The World Health Organization now recommends parasitological confirmation for malaria case management. Rapid diagnostic tests for malaria (RDTs) are an accurate and simple diagnostic to confirm parasite presence in blood. However, where they have been deployed, adherence to RDT test results has been poor, especially when the test result is negative. Few studies have examined adherence to RDTs distributed or purchased through the private sector.

Methods: The Rapid Examination of Malaria and Evaluation of Diagnostic Information (REMEDI) study assessed the acceptability of and adherence to RDT results for patients seeking care from private sector drug retailers in two cities in Oyo State in Southwest Nigeria. In total, 465 adult participants were enrolled upon exit from a participating drug shop having purchased anti-malaria drugs for themself. Participants were given a free RDT and the appropriate treatment advice based on their RDT result. Short Message Service (SMS) text messages reiterating the treatment advice were sent to a randomly selected half of the participants one day after being tested. Participants were follow-up via phone four days after the RDT was conducted to assess adherence to the RDT information and treatment advice.

Results: Adherence to RDT results was 14.3 percentage points (P-val <0.001) higher in the treatment group who were sent the SMS. The higher adherence in the treatment group was robust to several specification tests and the estimated difference in adherence ranged from 9.68 to 16.1 percentage points. Further, the higher adherence was specific to the treatment advice for antimalarial drugs and not other drugs purchased to treat malaria symptoms.

Conclusions: SMS text message substantially increased adherence to RDT results for patients seeking care for malaria from privately owned drug retailers in Nigeria and may be a simple and cost-effective means for boosting adherence to RDT results if and when RDTs are introduced as a commercial retail product.

Keywords: Malaria; Nigeria; Adherence to Rapid Diagnostic Test for Malaria; SMS intervention

INTRODUCTION

Although recent World Health Organization guidelines for malaria management in endemic countries recommend that treatment should be reserved for confirmed malaria cases [1], presumptive diagnosis of malaria and subsequent over-administration of antimalarial drugs remain the norm [2]. With the recent improvements in the accuracy of Rapid Diagnostic Tests (RDTs) for malaria, there is an opportunity to improve the quality of diagnosis and treatment. RDTs are antigen-based tests that detect parasite-specific antibodies or antigens in a drop of blood within 15 minutes. RDTs are easy to use and have similar or superior specificity and sensitivity to microscopy, the previous gold-standard diagnostic [3, 4]. The relative simplicity of RDTs enables their use by those with minimal medical training [5] and their low cost make them a potentially cost-effective intervention for malaria management in low resources settings [6].

There has been a rapid increase in the availability of RDTs throughout many Sub-Saharan African countries [7]. However, patient adherence to test results has been poor, especially when the test results are negative [8, 9]. When RDT-negative patients disregard RDT results, the potential of RDTs to increase the cost-effectiveness of malaria treatment is undermined because 1) savings from unnecessary treatments are not realized, and 2) over-administration of first-line antimalarial drugs may fuel parasite resistance and/or diminished efficacy over time.

Strategies to improve adherence to RDTs have been attempted in countries where the public sector has implemented strict case management guidelines. In Senegal, where artemisinin-based combination therapies (ACTs) are only given with a positive RDT result, the number of ACT prescriptions now matches malaria prevalence [10]. However, such strategies are unlikely to be successful in countries where the majority of health services are provided through the private sector, such as in Nigeria [11]. In these countries, alternative strategies to reinforce treatment adherence to test results may be needed in lieu of strong regulatory control.

A series of recent studies have shown that reminders sent via short message service (SMS) can 'nudge' people to increase adherence to a variety of health related behaviors, including applying sunscreen [12], remembering to take diabetes drugs [13], following HIV antiretroviral therapy treatment regimens [14], and adhering to asthma treatment [15]. For RDT use in malaria case management, a 'nudge' may help people adhere to RDT results among those who are hesitant to follow through because of insufficient previous experience. Reinforcement of the appropriate health behavior indicated by the RDT may be particularly valuable when the test result conflicts with prior expectations of having malaria. While there is some consensus that SMS-based reminder interventions may be well-suited to developing countries [16, 17], there has been not been any work to date to examine the extent to which SMS messages can be leveraged to increase adherence to RDT results.

This study evaluates a pilot SMS-based intervention included in the Rapid Examination of Malaria and Evaluation of Diagnostic Information (REMEDI) study conducted in Oyo State in southwest Nigeria. The goal of the REMEDI study was to evaluate the acceptability of and adherence to RDT results for patients seeking care from private sector drug retailers (detailed below in study design section and in [18]). As a part of the study, half of the enrolled participants

were randomly assigned to receive an SMS message, which 1) reiterated the participant's RDT result and corresponding treatment advice, and 2) provided a platform whereby participants could send their questions to an advice nurse via SMS.

METHODS

Study design

The REMEDI SMS pilot was a double-blinded, parallel-group study conducted in Oyo state in Southwest Nigeria, in and around the cities of Ibadan and Ogbomosho. Ibadan is a large urban center, while Ogbomosho is mainly peri-urban. The main goal of the REMEDI study was to examine the acceptability of and adherence to results of RDTs among clients of private sector retail outlets. Privately owned pharmacies and proprietary and patent medicine vendors (PPMVs) were initially randomly selected from the enumerated sites within four local government areas and enrolled into the study. Two weeks in to the study, the site selection was modified to exclude small drug retailers whose main business was not medicinal sales; these PPMVs were replaced with other PPMVs in the adjacent local government areas.

At each retail site, adult customers exiting the shop were approached, screened for eligibility, and asked to complete a short survey. The REMEDI study protocol is detailed elsewhere [18]. In brief, the protocol entailed 1) enrolling non-pregnant adults outside of a pharmacy/drug shop who stated and demonstrated that they had just purchased malaria treatment for him/herself; 2) offering and conducting an RDT performed by a trained nurse; 3) taking a detailed survey and inventory of drugs purchased; 4) discussing the test result with the patient; and 5) providing appropriate treatment advice based on the test result at the end of the survey. If the participant's RDT was positive, the patient was told that the positive result indicates the presence of malaria and was instructed to take a course of ACTs that was provided for free. If the participant's RDT test was negative, the patient was told that the negative result indicates the absence of malaria and that antimalarial drugs would not be needed, but could be saved for use at a later time. The nurse informed participants that they could still take other medications they purchased for other conditions or to treat symptoms.

Regardless of the test result, all participants were referred to local clinics and hospitals where they could seek care if their condition was not malaria or if their illness became worse. All participants were also given an informational card that outlined treatment steps based on their test result. Lastly, participants were asked for their cell phone contact information and told to expect a short 5-10 minute phone call in four days to check on the status of their illness, during which they would be compensated with 100 Naira (US$0.63) in phone credits for taking the call.

In the follow-up phone survey four days after the initial encounter, participants were asked which of the purchased drugs they had used. Responses to this question were used as a measure of adherence to the RDT information and treatment advice.

SMS intervention protocol and complications

SMS messages were sent to randomly selected participants one day after the initial encounter. The same message was sent to each participant once in English and once in Yoruba (the dominant local ethnic group). The content of the message repeated the advice given by the nurse the previous day at the time of testing and included a final line stating that patient could reply to

the nurse via text if they need additional information. See Figure 1 for the text of the messages sent.

To ensure that none of the survey staff would know who were chosen to receive the SMS, the study manager, who did not have any interaction with participants, randomly assigned surveys into the treatment group after the surveys were returned to the study office each day. The protocol for treatment assignment entailed assigning every other survey to the SMS treatment group on the day of the baseline survey. However, due to disruptions in electricity and communications channels with the study site in Ogbomosho, surveys collected in Ogbomosho were not returned to the study office in time for assignment to treatment/control group the same day. On days when disruptions prevented the study manager from following assignment protocol, treatment assignment was slightly altered. Additional surveys from Ibadan were randomly assigned to the treatment group to compensate for fewer treatment assignments from Ogbomosho. During these periods of delay in transmissions from Ogbomosho, one additional Ibadan-based survey was selected to receive an SMS for every two surveys collected in Ogbomosho. These "off-protocol" treatment assignments are taken into account in the statistical analyses of the data.

Participants

Of 465 adults enrolled adults, all of whom completed the baseline survey. 32 participants were not reached for follow-up and eight baseline surveys were incorrectly numbered,[a] leaving 425 participants who were reached in the follow-up phone survey. Only 419 observations are analyzed because four surveys did not have drug information data, one was missing site information and one was ineligible upon further inspection. In total, 213 participants were sent the SMS reminder one day after their RDT was conducted at the phone number they provided. Figure 2 presents a flow chart outlining the surveys included for analysis.

Statistical analyses

Descriptive analyses present sample characteristics, the randomization of participants, and the distribution of variables between treatment and control groups. The main analysis examines whether participants adhered to the recommended treatment course based on their RDT result. Specifically, participants are considered to have "followed treatment advice" if (1) they were RDT-negative and did not take any of the anti-malarial medications purchased or (2) if they were RDT-positive and took an ACT. This binary outcome is predicted using logistic regression to compare the SMS treatment group to those who were not sent an SMS. Standard errors are clustered by retail site where the surveys were conducted.

The primary analysis estimates the "intent to treat" effect. Mean differences across treatment and control groups, odds ratios (risk ratio), and risk differences (marginal effects) are reported. Four model specifications are employed:

Model 1

$$\text{Log}\ (\text{Adhere}/(1-\text{Adhere})) = \alpha + \beta\ (\text{SMS}) + \varepsilon$$

Model 2

$$\text{Log (Adhere/(1-Adhere))} = \alpha + \beta\,(\text{SMS}) + \gamma\,(\text{SMS*Ibadan}) + \pi\,(\text{Ibadan}) + \varepsilon$$

Model 3

$$\text{Log (Adhere/(1-Adhere))} = \alpha + \beta\,(\text{SMS}) + \gamma\,(\text{SMS*Ibadan}) + \pi\,(\text{Ibadan}) + \eta\,(\text{SMS*Off-protocol}) + \mu\,(\text{Off-protocol}) + \varepsilon$$

Model 4

$$\text{Log (Adhere/(1-Adhere))} = \alpha + \beta\,(\text{SMS}) + \gamma\,(\text{SMS*Ibadan}) + \pi\,(\text{Ibadan}) + \eta\,(\text{SMS* Off-protocol}) + \mu\,(\text{Off-protocol}) + \boldsymbol{\rho}\,(\text{Unbalance variables}) + \varepsilon$$

Model 1 compares the difference in mean adherence between the treatment and the control group. To account for potential differences in the effectiveness of the SMS intervention between Ogbomosho and Ibadan, Model 2 adds an indicator variable for each city and an interaction term between the treatment and the city in which participants were surveyed. In Model 3, an interaction term for SMS treatment assignment on off-protocol days and an indicator for off-protocol assignment are added to examine potential differences due to disruptions experienced in Ogbomosho. Lastly, Model 4 adds controls for observable sociodemographic variables that are unbalanced across groups to further control for potential differences between treatment and control individuals.

Two additional outcomes are analyzed. A secondary analysis examines the effect of the SMS intervention on those who report actually seeing the SMS, or the "treatment on the treated effect." This analysis excludes 39 participants who were sent an SMS, but who reported that they did not see it. Lastly, to determine whether the SMS influenced only anti-malarial drug adherence versus other non-malaria drugs purchased, the same regression analyses are used to predict the likelihood of taking malaria and non-malaria drugs among a subset of RDT-negative participants who bought both types of drugs. While the SMS message only provided advice on the appropriate use of anti-malaria drugs, it is unclear whether this advice also affected participants' usage of non-malaria drugs that are beneficial for treatment symptoms. This analysis captures the specificity of the response to the content of the SMS message by examining the relationship between receiving the SMS message and usage of both anti-malarial and symptom drugs. For these additional outcomes group differences, odds ratios (risk ratio), and risk differences (marginal effects) are reported, as well as results for regression models 1 and 3.

Ethical considerations

The University of California, San Francisco's institutional review board and Nigeria's National Health Research Ethics Committee approved the REMEDI study protocol. Surveyors obtained written consent from all study participants.

RESULTS

Table 1 presents sample characteristics for participants across treatment and control groups. The intervention and control groups are significantly different in terms of literacy and having cable TV, a motorcycle, a bank account, and a flush toilet. Once the city in which the survey was conducted is controlled for, only having a bank account (88.7% vs. 74.8%) and not being able to

read or write (98.1% vs. 91.7%) remain significantly different. Hence, these unbalanced variables are controlled for in subsequent regression model specifications.

Table 2 shows estimates of the intent to treat analysis. A simple comparison of means shows that 79.8% of participants who were sent an SMS adhered to the treatment advice compared to only 65.5% who were not sent an SMS. This translates into a 14.3 percentage point (95% CI: 6.68-21.9 percentage points) increase in adherence, with an odds (risk) ratio of 2.08 (95% CI: 1.44-3.01). When controlling for and allowing for different treatment effect by location in row 2, the estimate reduces to a 10.1 percentage point difference (95% CI: 3.66-16.6 percentage points) between the treatment and control groups, with an odds (risk) ratio of 1.70 (95% CI: 1.20-2.42). Further accounting for the influence of assigning treatment to SMS reminders off-protocol in row 3 and controlling for unbalanced baseline asset variables in row 4 returns treatment effect estimates similar to the unadjusted one.

In Table 3, estimates of the treatment effect on the treated excluding 39 participants who reported that they did not see the SMS are presented. Adherence to the treatment advice in the SMS group increases to 81.6%. Unconditional means in row 1 show that adherence was higher in the SMS group by 16.1 percentage points (95% CI: 8.89-23.2 percentage points). The SMS group had a higher likelihood of adhering to treatment advise with an odds (risk) ratio of 2.33 (95% CI: 1.62-3.36). In specifications where differences by city and/or issues of off-protocol assignment were accounted for, the estimated treatment effect on the treated ranges from 11.3 percentage points (95% CI: 3.76-18.9 percentage points) to 15.5 percentage points (95% CI: 7.86- 23.2 percentage points).

In Table 4, only the subset of RDT-negative participants who bought both symptom drugs and antimalarial drugs are analyzed. Of these 240 RDT-negative participants, 21.4% who were sent an SMS took their purchased malaria drugs compared to 41.5% were not sent an SMS. In other words, significantly fewer participants—20.1 percentage points (95% CI: -30.7 to -9.46 percentage points)—who were sent an SMS took the unnecessary antimalarial drugs. For comparison, 73.5% of participants who were sent an SMS took their symptomatic drugs compared to 80.5% were not sent an SMS, a difference of 7 percentage points that was not statistically significant.

DISCUSSION

This study demonstrated that a simple SMS text message substantially increased adherence to RDT results. Compared to those who were not sent a reminder message, the probability of adhering to the correct malaria treatment advice increases by 10-15 percentage points among those who were sent the reminder. Previous estimates of the magnitude in the effect size of SMS interventions to increase adherence to drug treatments range from 8-17% [12-14, 19] —the estimates in this study are very much in line with those studies. Together, these studies suggest that for many health-related behaviors, individuals may benefit from additional reinforcement to help them to follow though with intended or prescribed behaviors.

Unlike previous SMS intervention studies in which the message often persuaded participant to do something that the individual already stated that s/he wanted to do (e.g. use sunscreen, take medication, or save money), in this study the message reinforces a previously given medical

advice, which most often contradicted the planned behavior of the participants. Indeed, the vast majority of the participants tested were found to have negative results (96%). These individuals, having just purchased treatment medicines, were essentially asked to change behavior and **not** do something they usually would do—take an anti-malaria drug—to treat a suspected episode of malaria. This suggests that reminder messages can help individuals break from their default behaviors in addition to helping them follow through with intended behaviors.

To further understand how the SMS intervention may have influenced participants' choices, participants in the treatment group were asked to describe which aspect of the SMS message they found helpful. The majority of respondents (52%) said that the SMS reminded them of their test result, while another 30% said that it reminded them of the correct treatment course. Only about 10% mentioned the usefulness of having a link to an advice nurse and only 10 participants actually contacted the advice nurse for consultation regarding unresolved febrile symptoms or to confirm that they should not take the antimalarial medication. Hence, it appears that the reinforcement of the medical advice reiterated by the SMS was the most helpful to participants in choosing which drugs to take for their condition.

Given the ubiquity of cell phones throughout Nigeria and the relatively low cost of sending an SMS compared to the cost of anti-malaria drugs, the SMS intervention is cost-effective from a societal point of view. The average cost of the anti-malaria treatment course purchased within the study sample was 350 Naira (US$2.50). This cost reflects only the retail price of treatments and does not include the large-scale international subsidy for ACTs, which is about $4.00 a course. Based on the results of this study, a similar SMS intervention would save $0.25 per episode in direct treatment costs alone. If the proportion of participants who bought ACTs and international subsidy were taken into consideration, then the cost savings would be average $0.43 per episode. We estimate the cost of sending an SMS manually to total 10 cents including labor and service costs, suggesting that the SMS intervention is cost-saving one. If the SMS messages were automated the cost be would even lower.

This study provides further evidence that reminding or nudging people can alter health-related behaviors. However, these results should be interpreted in light of several caveats. For various reasons, randomization was not perfectly balanced, although application of regression controls helped to adjust for these factors. Because the pilot study was conducted in primarily urban areas, this sample is unlikely to be representative of the state or of the country. Results reflect a wealthier and more educated population and health behaviors may differ, including responses to SMS messages, for poorer populations located in rural areas. An expanded version of this study is currently underway to assess the extent to which SMSs increase RDT adherence in other states, in areas with higher entomological inoculation rates, and in more representative populations. Further, because adherence to treatment advice is a self-reported measure, some reporting bias may be present if the SMS message also prompted individuals to self-report "better" outcomes. To the extent that the SMS increased reporting bias, the intervention effect size could be over-estimated. However, because results for malaria drugs and non-malaria drugs showed differential drug-taking behavior, such self-reporting bias may be minimal. Finally, the SMS contained the exact same information that was provided by the advice nurse. For this reason, the SMS may have served as a general reminder of the testing experience, and we can differentiate the effect a general reminder from the effect of the precise message contained within the SMS. Future

interventions would need to test different wording of the message to see which word choices increase adherence the most.

Currently, innovations in integrating RDTs into malaria case management in the private sector are well underway. The results of this pilot study show that SMS may be a tool to increase adherence to RDT test results as RDT roll out is expanded to the private sector.

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LIST OF ABBREVIATIONS USED

ACT- Artemisinin-based combination therapies
CI – Confidence interval
PPMV- Proprietary and patent medicine vendors
RDT- Rapid diagnostic tests for malaria
REMEDI - Rapid Examination of Malaria and Evaluation of Diagnostic Information study
SMS – Short message service
WHO- World Health Organization

COMPETING INTERESTS
The authors declare they have no competing interests.

AUTHOR CONTRIBUTIONS
SM JL conceived of the intervention
SM JL ES AD wrote questionnaire, operations manuals, and trained staff for REMEDI study
ES implemented and oversaw SMS intervention
CI, EN, JA, CU & DM facilitated the intervention through funding and infrastructure of wider REMEDI study
SM did analysis
SM structured and wrote the manuscript
JL ES and AD participated in the presentation of results and contributed to writing the manuscript
All authors read and approved the final manuscript.

ENDNOTES
[a] Eight surveys had duplicated survey numbers. These observations were dropped, as it was impossible to tell which entry was correct.

Table 1: Summary Statistic for Treatment and Control Samples

	SMS Sent (N=213)		SMS Not Sent (N=206)	
	N	Mean or %	N	Mean or %
Sites				
PPMV[&]	81	38.0%	109	52.9%
Ogbomosho[&]	36	16.9%	79	38.3%
Demographics				
Male	109	51.2%	102	50.0%
Age		40.7		37.3
Married	157	73.7%	131	63.9%
Literacy Status				
Can not read or write*[&]	209	98.1%	189	91.7%
Education Level				
Less than Primary	14	6.6%	16	7.8%
Completed Primary	27	12.7%	29	14.1%
Completed Secondary	88	41.5%	76	37.1%
More than Secondary	83	39.2%	84	41.0%
Assets/Infrastructure				
Electricity	204	95.8%	196	95.1%
Radio	199	93.4%	189	91.7%
TV	193	90.6%	188	91.3%
Refrigerator	144	67.6%	129	62.6%
Cable TV[&]	114	53.8%	90	43.7%
Generator	152	71.4%	144	70.6%
AC	36	16.9%	29	14.1%
Computer	75	35.4%	78	38.0%
Electric Iron	195	91.5%	179	87.3%
Fan	194	91.1%	189	92.2%
Motorcycle/Scooter[&]	36	16.9%	58	28.3%
Car	102	47.9%	91	44.2%
Bank account*[&]	189	88.7%	154	74.8%
Flush toilet in house[&]	171	80.3%	144	69.9%
Kerosene to cook	116	54.5%	126	61.2%
Concrete floors	139	65.9%	126	62.1%

[&] Variables are statistically different across treatment and control groups
[*] Variables remain statistically different across treatment and control groups even within city (Ibadan/Ogbomosho)

Table 2: Logistic regression results for the intent to treat effect of SMS reminders on adherence to treatment advice.

	Primary outcome	Percentage (N)		Risk ratio (95% CI)	Risk difference (95% CI)
		SMS Sent (213)	SMS Not Sent (206)		
1	Followed treatment advice	79.8% (170)	65.5% (135)	2.08 (1.44-3.01)	14.3% (6.68%-21.9%)
2	Followed treatment advice*			1.70 (1.20-2.42)	10.1% (3.66%-16.6%)
3	Followed treatment advice**			2.19 (1.50-3.18)	14.8% (8.14%-21.4%)
4	Followed treatment advice***			2.13 (1.45-3.12)	14.2% (7.33%-21.1%)

N=419

* Controls for city survey was conducted (Ibadan/Ogbomosho) and treatment*city survey was conducted (Model 2)

** Controls for city survey was conducted (Ibadan/Ogbomosho), treatment*city survey was conducted, whether assignment to the treatment/control group was made off-protocol, and treatment* whether survey assignment to the treatment/control group was made off-protocol (Model 3)

*** Controls for city survey was conducted (Ibadan/Ogbomosho), treatment*city survey was conducted, whether assignment to the treatment/control group was made off-protocol, and treatment* whether survey assignment to the treatment/control group was made off-protocol, an indicator variable for surveys conducted at PPMVs, an indicator variable for participant who had a bank account, and an indicator variable for participant could not read or write (Model 4)

Standard errors are clustered by retail site

Table 3: Logistic regression estimates of the treatment on treated effect of SMS reminders on adherence to treatment advice.

Primary outcome	Percentage (N) Received SMS (n=174)	Percentage (N) SMS Not Sent (n=206)	SMS Risk ratio (95% CI)	SMS Risk difference (95% CI)
1 Followed treatment advice	81.6% (142)	65.5% (135)	2.33 (1.62-3.36)	16.1% (8.89%-23.2%)
2 Followed treatment advice*			1.84 (1.19-2.85)	11.3% (3.76%-18.9%)
3 Followed treatment advice**			2.34 (1.46-3.74)	15.5% (7.86%-23.2%)
4 Followed treatment advice***			1.68 (1.08-2.61)	9.68% (1.84%-17.5%)

Exclude 39 observations who were sent a text message but who reported not seeing the text message in the follow-up survey, N=380.

* Controls for city survey was conducted (Ibadan/Ogbomosho) and treatment*city survey was conducted (Model 2)

** Controls for city survey was conducted (Ibadan/Ogbomosho), treatment*city survey was conducted, whether assignment to the treatment/control group was made off-protocol, and treatment* whether survey assignment to the treatment/control group was made off-protocol (Model 3)

*** Controls for city survey was conducted (Ibadan/Ogbomosho), treatment*city survey was conducted, whether assignment to the treatment/control group was made off-protocol, and treatment* whether survey assignment to the treatment/control group was made off-protocol, an indicator variable for surveys conducted at PPMVs, an indicator variable for participant who had a bank account, and an indicator variable for participant could not read or write (Model 4)

Standard errors are clustered by retail site

Table 4: Logistic regression results for the effect of SMS reminders on malaria and non-malaria drug-taking.

Medications taken	Percentage (N) SMS Sent (n=117)	SMS Not Sent (n=123)	SMS Risk ratio (95% CI)	SMS Risk difference (95% CI)
1 Took anti-malarial	21.4% (25)	41.5% (51)	0.38 (0.23-0.62)	-20.1% (-30.7% to -9.46%)
2 Took anti-malarial**			0.36 (0.20-0.63)	-21.1% (-31.8% to -10.5%)
3 Took symptom drug	73.5% (86)	80.5% (99)	0.67 (0.37-1.20)	-7.0% (-17.3% to 3.31%)
4 Took symptom drug**			0.89 (0.42-1.90)	-2.1% (-15.2% to 11.1%)

Sample includes RDT negative participants who bought both an antimalarial drug as well as drugs to treat their symptoms, N=240

** Controls for city survey was conducted (Ibadan/Ogbomosho), treatment*city survey was conducted, whether assignment to the treatment/control group was made off-protocol, and treatment* whether survey assignment to the treatment/control group was made off-protocol (Model 3)

Standard errors are clustered by retail site

Figure 1: REMEDI Study SMS Message

RDT-positive participants:

ENGLISH

Yesterday, you tested POSITIVE for malaria. Take LUMARTEM. If questions, please reply or TEXT. A nurse will call you soon. ID[XX]

YORUBA

Lanaa, iwadii fi han pe E NI IBA. E maa lo ACT yin bo se ye. Tee ba nibeere, e fesi ateranse yii, noosi wa yoo si daa yin lohun logan. ID[XX]

RDT-negative participants:

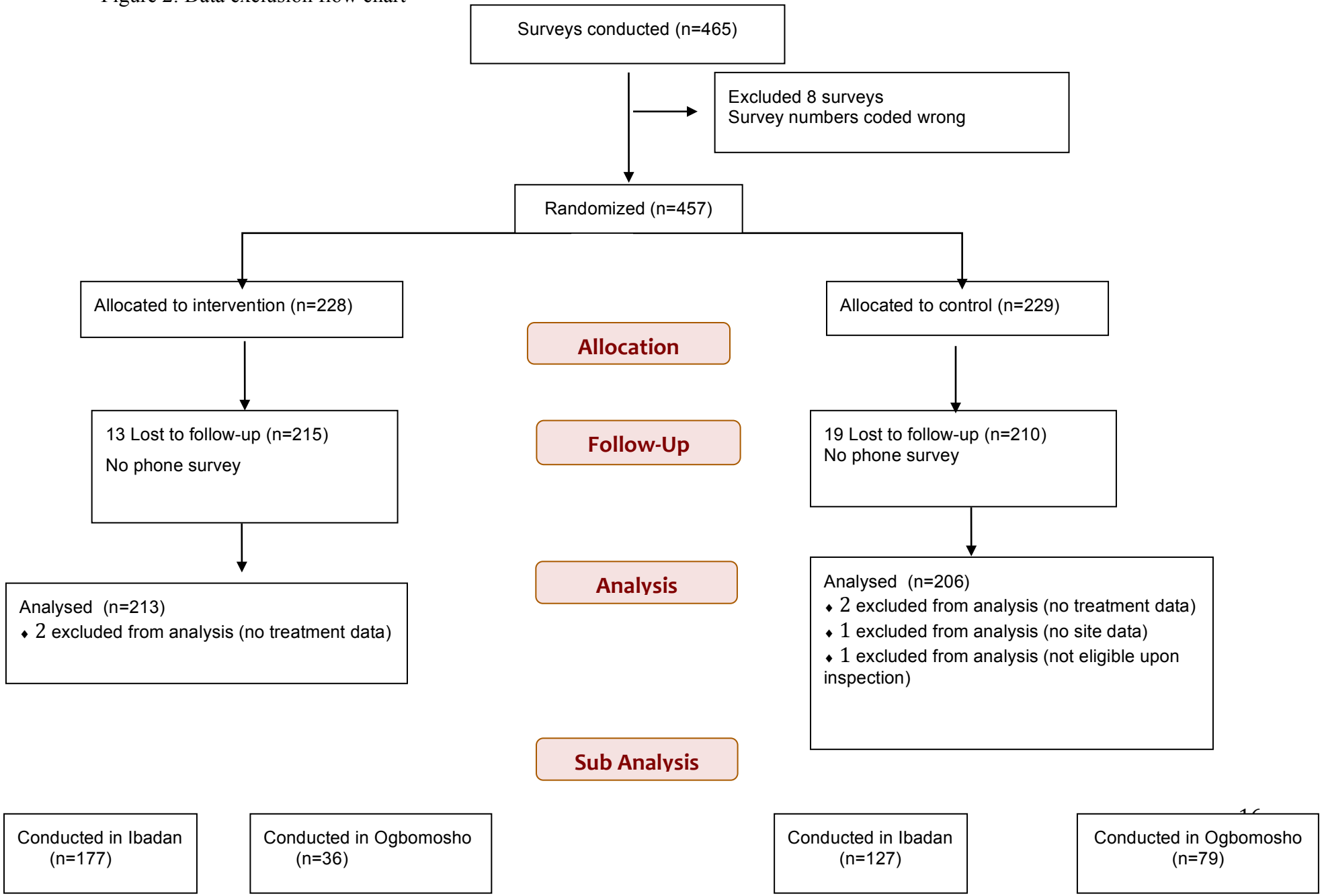
ENGLISH

Yesterday, you tested NEGATIVE for malaria. Malaria drugs NOT needed. If questions, please reply or TEXT. A nurse will call you soon. ID[XX]

YORUBA

Lanaa, iwadii wa han pe E O NI IBA. Eyi fi han pe e o nilo oogun iba. Te e ba ni ibeere, e fesi ateranse yii, noosi wa yoo si daa yin lohun logan. ID[XX]

Figure 2: Data exclusion flow chart



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