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ExPost

Randomized Controlled Evaluation of SKY Health Insurance in Cambodia

Survey Protocol

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1. Background

This proposal describes an impact evaluation of the SKY Health Insurance Program in Cambodia.

The origins of SKY (Sokapheap Krousar Yeung, "Health for Our Families") lie in a microfinance program launched by Groupe d'échange et de recherche technologiques (GRET) in Cambodia in 1991. Observing that borrowers frequently cited health problems as a reason for dropping out of the program, GRET began to develop a micro-insurance product aimed at mitigating the disastrous consequences of health shocks. Piloted in 1998 and expanded in subsequent years, SKY offers households, for a fixed monthly premium, free and unlimited primary and emergency care at contracted health centers, as well as a number of other health services. One of SKY's primary goals is to enable families to cover health costs without risking impoverishment. SKY typically represents the only health insurance option available in the regions where it operates.

SKY currently offers insurance to households in several rural districts in Takeo and Kandal provinces, and has recently expanded into the capital, Phnom Penh, targeting specific groups such as garment workers and market vendors. In December 2005, the program had 4,392 beneficiaries from 917 households, up over 160% from the previous year. Take-up of insurance ranges from 2% in regions where insurance has been only recently introduced to 12% in the longest served regions (SKY briefing note, 2005).

We will study SKY's 2007-8 expansion into additional districts in Takeo and Kandal provinces as well as parts of Kampot. We will evaluate the health and economic effects of the SKY program on households using a **randomized controlled trial.** The central methodological tool of this evaluation is randomized distribution of "Lucky Draw" coupons that will induce random variation in the likelihood of taking up insurance. Randomization allows estimation of the causal effects of health insurance as distinct from all other characteristics that vary across insured and non-insured households. We will also use in-depth interviews of a small group of households in selected villages to take a closer look at household behavior. Finally, interviews with households, health center staff, and SKY Member Facilitators will help to understand SKY's effects on health centers.

2. Objectives of the Study

The research has four objectives:

1. To estimate the causal effect of health insurance on households.

- Economic outcomes (e.g., medical spending; sales of productive assets; household debt and loans)
- Health outcomes (e.g., frequency and duration of illness; subjective self-health assessments; objective health measures)
- Health utilization (e.g., public health facility utilization; substitution to public facilities from private health centers and traditional medicine; preventative care utilization; timely utilization of curative care; self-medication)

2. To understand the determinants of take-up of health insurance; the extent of adverse and positive selection; the potential for risk-pooling.

- Household characteristics (e.g., wealth and income; age and sex composition; past health utilization; health status; perceptions of public facilities; expected utilization of public facilities; access to alternate sources of health financing; risk aversion)
- Public health facility characteristics (e.g., objective measures of quality; convenience of access)

3. To identify potential effects of health insurance on public health facilities (e.g., hours and drug availability; subjective and objective quality indicators).

4. To contribute to the body of knowledge on health insurance in Cambodia and in developing countries more broadly.

3. Research Design

We will use a mix of qualitative and quantitative methods, including both in-depth interviews of SKY employees, health care providers and households and a large household survey.

The large household survey will measure the impact of insurance on rural Cambodians. To overcome the key obstacle to causal inference—that those who choose to purchase insurance typically differ in many observable and unobservable ways from those who do not—we will implement a randomized controlled trial that allows us to identify the impact of health insurance independently from all other factors that may affect a household's decision to take up insurance. No household will be denied access to insurance. Rather, by subsidizing the premium of a randomly selected group of households, we can estimate the effect of insurance on households without substantially altering the existing SKY program.

The study design will also enable us to examine characteristics of the groups that buy or decline insurance at different prices. Using data from the baseline survey, we will examine the characteristics (in particular, health status and prior health care utilization) of those who choose to purchase health insurance at different prices. Using data from the follow-up survey, we will also compare the utilization rates of people who buy insurance at different prices, and compare the dropout rates of those who utilize facilities at different rates. Both techniques allow us to estimate the extent and nature of self-selection.

The randomized evaluation will be implemented as SKY visits villages to promote insurance. When the SKY program first rolls out into a region, SKY holds a village meeting to describe the insurance product to prospective customers. At

the end of each meeting, SKY will hold a Lucky Draw lottery in which a random subset of households receives coupons for discounts on the insurance premium.

The randomization of premiums will provide a discount to some households. Of households that attend the village meeting, 20% of households, up to a maximum of 12 households per village, will win a coupon for 5-months free insurance in the first 6-month cycle, with the option to renew for a second 6-month cycle with a coupon for 3-months free. The remaining households in attendance at the meeting will be entitled to a coupon for a 1-month discount on insurance, which is the usual policy for the SKY program.

SKY will be holding village meetings in Takeo, Kandal, and Kampot provinces over the course of the thirteen months from November 2007 to December 2008 (roughly the period during which the baseline survey will be administered). Table 1 shows an approximation of the distribution of the targeted sample across regions. The column "Total Meetings Held" indicates the number of village meetings SKY plans to hold in each district or province and "Total Lucky Draws" indicates the number of Lucky Draws held in the given district. The final column, "% of Villages Surveyed", indicates the approximate percentage of the sample that will come from each area. Note that we will hold Lucky Draws at most village meetings in 2008. We will only survey villages in which we hold a Lucky Draw.

Table 1: Targeted Regions

	Total Meetings Held	Total Lucky Draws	% of Villages Surveyed
Takeo Province	128	128	51%
Ang Roka villages (older villages)	32	32	13%
Kirivong villages	48	48	19%
Donkeo villages	48	48	19%
Kandal Province 74 72 29%			
Koh Thom villages	74	72	29%
Kampot Province 50 50 20%			
Kampot villages	50	50	20%
All Villages	**252**	**250**	**100%**

In addition to the randomized intervention, we will also be studying the effect of SKY on the quality and functioning of public health facilities. The fact that higher quality facilities are targeted by SKY means that there can be no valid control group in a longitudinal study, but we will use several methods to measure changes in health center quality over time that will allow us to gain some insight into the impact of SKY.

4. Sampling Strategy

For the quantitative survey, the main evaluation sample will consist, roughly, of half coupon winners and half non-winners. More precisely, we will interview all large coupon winners, and randomly select an equal number of small coupon winners to interview. In addition, to address the question of selection into insurance, we will oversample households that purchased insurance from the population of small coupon winners. In total, our sample of approximately 5,200 households will consist of approximately 46% large coupon winners and 54% small coupon winners. For the quantitative survey, the main evaluation sample will consist, roughly, of half coupon winners and half non-winners. More precisely, we will interview all large coupon winners, and randomly select an equal number of small coupon winners to interview. In addition, to address the question of selection into insurance, we will oversample households that purchased insurance from the population of small coupon winners. In total, our sample of approximately 5,200 households will consist of approximately 46% large coupon winners and 54% small coupon winners[1].

The survey team will interview households between two and seven weeks after the village meeting[2]. An Evaluation Representative will attend each village meeting to help run the Lucky Draw lottery. At the end of each village meeting the Evaluation Representative will randomly select a number of the small coupon winners to be interviewed. SKY Insurance Agents will visit these households, as well as all large coupon households, before the 21st of the month. Once all households have had the opportunity to purchase insurance, we will interview these sampled households. Table 2 offers a breakdown of the interview sample.

Table 2: Quantitative survey sample

	Large coupon winners (100% sampled)		Small coupon winners (25% sampled) buyers		Additional small coupon (50% sampled)	Total
	1	2	3	4	5	6
	Total	Buyers (est.)	Total	Buyers (est.)	(Estimated)	(Estimated)
Per meeting	9.5	3.8	9.5	0.57	1.8	20.8
Total	**2,375**	**950**	**2,375**	**143**	**453**	**5,203**

Notes: We assume 250 village meetings. Based on the results from the pilot test in 2007, we assume 40% of large coupon winners purchase SKY and 6% of small coupon winners purchase SKY.

Columns [1] and [3] of Table 2 show the number of large and small coupon winners to be interviewed, respectively. Columns [2] and [4] show the number of buyers that will be interviewed out of those two groups, with sample sizes estimated based on the winter 2007 pilot take-up rates. Column [5] shows the estimated number of additional small coupon buyers we will oversample. Finally, column [6] shows the total number of households interviewed per village and overall.

[1] Actual number and distribution (buyers/non-buyers, etc.) of households will vary depending on village meeting attendance and take-up in each village.

[2] Households attending earlier meetings, e.g., in December and January, will be interviewed more than 7 weeks after the village meeting.

5. Methodology

In this section we detail the four sources of data that will be collected in the evaluation, namely, the household survey, village leader interview, village monographs, health center surveys and health logbooks.

5.1. Household Survey

The principal component of data collection is a large-scale survey collecting three rounds of quantitative data from approximately 5,200 households. The sampling is described above in section 4 of this protocol.

The baseline will be a rolling survey, administered to each village approximately two to seven weeks after the Lucky Draw is held[3]. The second round will use the same rolling format, and will be administered 12 months after the baseline. The final survey round will be administered 24 months after the baseline.

This survey will collect data on household demographics, wealth indicators, self-perceived and objective health measures, health care utilization and spending, assets and asset sales, savings, debt, health risk behaviors, willingness to take financial risks, trust of institutions, means of paying for large health expenses, and willingness to trade current for future income. We describe the elements of this survey in detail in Section 6, below.

The survey will ensure high quality data using multiple means. All surveys will be reviewed at the household by the enumerator. All surveys will then be reviewed by a data editor who reviews unclear responses and outliers and checks for data consistency (e.g., mothers who are male or age 7). All surveys will then be reviewed by a field supervisor. The field editor and supervisor also review a subset of questions with a subset of households to check on data quality. These checks will include all very large responses for high health care costs (an important point, as health care costs are dominated by a small number of outliers.) When data are entered, we run a further set of checks for consistency and permissible values. Unexpected answers and outliers lead to rechecking the paper questionnaires.

5.2. Village Leader Interview

In each village, we will interview the village chief or village leader in order to collect general village-level information, including the location of all local health facilities (public, private, pharmacies, traditional healers, etc.), recent village-level shocks (drought, flood, epidemics, etc.), availability of lending institutions, seasonal access to roads and price and availability of paid transportation.

Because the randomization is at the household level, the information garnered from the village leader will be of greatest use in the selection study (e.g., testing if villages far from regional hospitals have lower take-up), in assessing the external validity of the evaluation, and potentially in estimating the effect of insurance in villages with different characteristics.

[3] See previous footnote: the timelag between village meetings and interviews will be longer for households attending earlier village meetings.

5.3. Village Monographs

To better understand how SKY affects outcomes and why people buy and/or drop out of SKY, we will collect detailed village-level information from four villages that have already been exposed to SKY (including two villages in Kandal, where SKY has been present the longest) and two villages in which SKY will enter in 2008. We will interview all past and present SKY members in each village, ten non-SKY members, and representatives of all of the health care providers (public as well as private sector).

The resulting village monographs will provide detailed qualitative evidence regarding the dynamics of SKY membership over time (who joins first, who drops out, and why) as well as potential effects of SKY on non-members (e.g., how attitudes towards insurance and health-seeking behavior change). We will also use the interviews to examine how SKY and local health care providers operate and interact in practice. In addition, we will test future rounds of the survey instrument in these villages.

Table 3: Qualitative Villages Targeted

Province	Takeo	Kandal	Total
Current SKY villages	2	2	4
Future SKY villages	2	0	2
Total number of monographs	**4**	**2**	**6**

5.4. Health Center Survey

The main evaluation will be at the household level. At the same time, there are also potentially interesting effects of SKY on health care providers and of health center quality on SKY take-up. Our randomization will not capture these effects directly. Thus, we will use multiple methods to gain some understanding of these effects. To do so we will analyze the following sources of information on health center quality:

• The household survey will measure perceptions of health center quality.

• GRET health center surveys prior to SKY introduction provide quality measures for most facilities.

• The Village Monograph will use in-depth interviews and observation to measure health center quality and to examine how SKY has affected health center quality.

• Subsets of these interview questions will be replicated at roughly ten additional health centers to check whether results from the in-depth case studies generalize to other health care providers.

• Two waves of a short survey of SKY Member Facilitators will be administered to measure aspects of health center quality that member facilitators can observe. The survey will include Member Facilitator reports on:

Doctor skills and respect for patients

• Other clinician skills and respect for patients

• Sufficiency of drugs

• Cleanliness

• Waiting times (average and longest of the week)

• Share of scheduled hours the center is open

• Side payments typically paid by SKY and by non-SKY members

• Changes in health center quality since SKY arrived (or since Member Facilitator started working at this health center)

We will use all of the data sources to study how the quality

of health centers has changed since SKY began in the area[4].

We will also use each household's perceptions, village-level average perceptions, member facilitator responses, and SKY's baseline measures of health center quality to study

1) whether quality perceptions of households, of Member Facilitators and of GRET evaluators tend to match,

2) whether high quality of local health centers is correlated with take-up and renewal of SKY insurance and,

3) which aspects of center quality have the largest effects on people's decisions to purchase and renew insurance.

5.5. Health Logbooks

To increase the accuracy of data on health care utilization and expenditure over the 12 months between the baseline and follow-up surveys, we will experiment with distributing log books at the time of the baseline survey. In these log books, which will contain a daily calendar, households will be asked to mark down each time a household member is ill, each time they visit any type of health facility, including traditional healers and drug stores, and the amount they spend on drugs and health care for each visit. Every month, a designated village monitor will collect completed logbook pages from households. Collection will be in a locked box to ensure confidentiality.

By minimizing recall problems, logbooks have the potential to be an extremely valuable source of information. It is difficult to collect accurate data on health events using long recall periods, especially for minor health events. In our household survey, we will deal with imperfect recall by using a one-year recall period for very large health events but limiting the recall period for small health problems to four weeks. Logbooks, if they are successful, could improve even further on this strategy, allowing households to record information on their health and health-seeking behavior on a daily (or almost daily) basis. This shorter recall period can lead to substantial improvements in data quality and thus more accurate estimates of the effects we aim to measure. Detailed logbook information may also give us better estimates of preventive health behavior—an extremely important component of health-seeking behavior.

Because logbooks are an innovative method of data collection for health care, we will condition their use on success in field tests. We will judge logbooks valid if they meet the following criteria:

- Households complete logbooks completely and regularly.
- Households are able to fill in logbooks (with or without help from a literate friend or family member) without creating undue burden on the household.
- Information recorded in logbooks is accurate.

The above criteria will be evaluated via random audits of logbook data, comparison with SKY (and perhaps other health center) utilization data, and comparison with recall questions in follow-up household interviews. We will extensively test logbooks during the first months of the roll-out. If logbooks appear valid, we will distribute logbooks on a large scale. We will continue to measure logbook quality via ongoing validity tests, as described above.

[4] We had initially planned to perform a longitudinal health center survey. Unfortunately, results from such a survey would not be credible. There is not a comparison group to show what changes would have occurred at these health centers without SKY's intervention. For example, if there is a national budget crisis and health center quality declines on average at health centers with SKY contracts – but far more at health centers without a SKY contract – our analysis would erroneously indicate a SKY contract reduced health center quality. Conversely, if average health center quality increases, our results will be biased in favor of showing a benefit for SKY.

Thus, we have reduced the planned data collection at health centers and increased the number of villages where we will collect household data – raising statistical precision for the main analyses. We will still have longitudinal data on health centers, as noted above. Because we are emphasizing qualitative research methods that identify the causal links behind changes we observe, our results should be more credible than a statistical study with more measures but without a comparison group. At the same time, we will explore the Ministry of Health's set of quality indicators and see if a statistical study encompassing both SKY and comparison regions is feasible.

6. Analytical Framework

As discussed in Section 2, this project has the following objectives:

1. to estimate the causal effect of health insurance on households;
2. to understand the determinants of take-up of health insurance; the extent of adverse and positive selection; and the potential for risk-pooling;
3. to identify potential effects of health insurance on public health facilities;
4. to contribute more broadly to the body of knowledge on the effect of health insurance in Cambodia and in developing countries.

This section outlines our methodology for achieving each goal. For objectives 1 through 3, we include a list of indicators that we plan to measure using our survey instruments. Note that the final list of indicators will depend on pre-test results, and on the power we have to measure SKY impacts using each indicator with the given study design.

Methodology for Objective 1: The Causal Effect of Health Insurance

6.1. Statistical Methods

To estimate the causal effect of health insurance on household health and economic outcomes, we will implement a number of estimators. The overall econometric technique is to generate multiple estimates of the effect of insurance on the outcomes of interest, thus allowing us to construct a credible range of the estimated program effect. To begin with, we will construct a lower bound for the program effect - the Intention-to-Treat effect. Here, outcomes at baseline will be differentiated from outcomes in the follow-up, across large and small coupon winners. This estimate provides a credible lower bound for the true program effect, because many of the large coupon winners will not take up insurance, while some of the small coupon winners will, yielding an underestimate of the true program effect. Formally, we will run a regression that includes a vector of individual fixed effects (individualj) to absorb characteristics of the household or individual, observed or unobserved, that do not change over time and a vector of year fixed effects (year t) to control for changes over time that affect all individuals (or households). We can also control for J and I time-variant individual- and household-level covariates (Xjt and Zjit) that might affect health, health care utilization, and other outcomes, e.g., current household income. Thus, we can estimate:

$$Yjivt = \beta\ postit * large\text{-}couponi + \gamma\ postit + \Sigma li=1\ \alpha\ Xit + \Sigma Jj=1\ \delta\ Zjit + individualj + yeart + Uvt + \varepsilon jivt$$

In this specification, postit indicates follow-up observations, Uvt represents village-level shocks collected in the village leader survey, and εjivt is a random error term representing individual variation from the mean. The coefficient β is the difference-in-differences estimate of the effect of being offered a high-value coupon. Outcome indicators (the Yjivt in the equation above) for Objective 1 are listed below.

A more policy-relevant estimate we will generate is the effect of insurance on those who were induced to take up insurance upon receiving a large coupon (also known as the Local Average Treatment Effect), using the "large coupon" as an instrumental variable for taking up insurance. The details of this estimator are presented in the Statistical Appendix.

6.2. Economic Outcomes

If successful, SKY health insurance will have the following economic effects: (a) lower health-related spending; (b) lower health-related debt; (c) fewer cases of asset sales, savings and decreased investment; (d) less lost income due to illness. This section describes the indicators we will use to measure these economic impacts.

6.2.1. Indicators for health-related spending

If we implement logbooks on a large scale, we will collect daily data on health incidents.

In the household survey, we will collect data on all health incidents in the last 4 weeks.

In addition, we will collect data using a 12- month recall period for "major" health incidents. A health incident will be defined as "major" if it meets one of the following definitions:

1. the individual was unable to perform daily activities for multiple consecutive days
2. treating the health incident was expensive (using some objective measure)
3. involved an overnight stay at a health facility

We will ask about health incidents by individual, not simply by household. This will allow us to assess the effect of insurance on intra-household health care disparities. Indicators include:

• Expenditure on drugs; non-drug treatments and exams (e.g., surgery, shots, etc.); overnight stays; other services (e.g., "thank you" payments); transport to health care providers

• Total cost of health-related expenditure, broken down by provider: public facilities (health center, referral hospital, provincial hospital); private doctor or provider; traditional healer; pharmacist; other health care provider

Specific to large expenditure (within the last 12 months):

• Amount spent on funeral expenses (if relevant)

• Amount spent per month treating chronic illnesses or symptoms

• Amount spent on preventative care (immunizations, prenatal care etc.)

6.2.2. Health-related debt

Our qualitative research leads us to expect that households typically incur debt to cover health expenditure before selling off productive assets. Thus, health insurance has the potential to substantially reduce debt burdens after illness or injury. In the household survey we will use several indicators to measure these impacts:

• % of households with outstanding debt

• Amount of outstanding debt, including amount of debt specifically incurred from health expenses

• % of households paying high interest rates (e.g., > 8% / month) on existing debt

• % of households taking out a loan to pay for a health-related expense in the past 12 months

6.2.3. Asset sales, savings and decreased investment

Severe health shocks may lead households to sell assets, including productive assets such as livestock. Related to this is disinvestment in human capital, for example, if children are pulled out of school to care for the ill. One effect of insurance may be to reduce the amount of disinvestment upon experiencing a health shock; another may be that the household will reduce the amount of precautionary savings. To measure the impact of insurance on the sale of assets, changes in savings and changes in investment, we will collect information on:

• % of households that sold any type of asset, and % of households that sold a productive asset (animal, load- bearing animal, farm equipment, farmland, other land etc.) This question will also be asked in relation to specific health-related expenses or debts.

• % of households using savings to pay for health care, as well as the type of savings (e.g., gold, jewelry, etc.)

• % of households decreasing child school hours due to health shocks

• % of households increasing child labor hours to pay for health care expenses

6.2.4. Income lost due to illness

To measure the impact of insurance on income lost due to illness we will use the following indicators (collected for each household member):

• Days unable to perform usual activities

• Days unable to perform usual income-producing activities

• (Estimated) Implied income lost (equal to daily wage * lost work days)

6.3. Health Outcomes

To estimate the causal effect of health insurance on health outcomes, we will examine (a) the frequency, duration and severity of health incidents; (b) perceived health; (c) objective measures of health.

6.3.1. Frequency, duration and severity of health incidents

In the household survey and logbooks, to measure the impact of insurance on frequency, duration and severity of health incidents, we will collect:

• % of individuals with any illness in the last 4 weeks

• % of individuals with a major health incident in the last 12 months (defined above)

• % of individuals with a chronic illness

• Average duration of illness (measured by number of days an individual could not perform normal activities due to illness)

6.3.2. Perceived health

Using the large-scale sample survey, we will ask the respondent to evaluate the general level of health of each household member. We will construct:

• % of individuals reporting poor health

• % of individuals whose health has deteriorated in the last 12 months

6.3.3. Objective measures of health

In addition to using occurrence of symptoms as an objective measure of health, we will use the large-scale sample survey to collect anthropometric information on all household members under age 60 months. This will allow us to measure:

• % of children with low height for their age (stunting)

• % of children with low weight for their height (wasting)

We will also collect measures of self-reported ability to perform Activities of Daily Living (ADLs) (e.g., ability to walk one mile) which will allow us to construct

• % of individuals that perform basic ADLs with difficulty

6.4. Health Care Utilization

We expect health insurance to have an impact on the following health-seeking behaviors: (a) treatment and choice of provider when ill; (b) preventive health care; (c) knowledge of appropriate preventive health behavior. We will use the following indicators to measure these impacts.

6.4.1. Treatment and choice of provider when ill

The large-scale sample survey and the logbook will collect data for the following indicators for all health incidents (relying on four-week recall in surveys) and for major health incidents, as previously defined (relying on 12-month recall in surveys):

• % of individuals seeking any treatment for a given health incident

• % of individuals visiting a public health facility first or at all for treatment for a given health incident

• % of individuals getting treatment by other means (traditional, private, self care, etc.) for a given health incident

• Average number of days before seeking treatment for a given health incident

• % of households obtaining trained medical advice before drug purchase

In addition, data for the following indicators will be collected relying on 12-month recall:

• % of individuals forgoing treatment for a major health incident (due to high expense)

6.4.2. Preventative health care

In the household survey we measure the following preventive health behaviors:

- For women pregnant in the last 12 months:

• % getting at least one or all recommended prenatal care visits

• % receiving a tetanus vaccine

• % delivering (including C-sections) in a public facility

• % delivering (including C-sections) by a trained medical professional

- For households with children under age 6:

• % receiving at least one recommended immunization

• % receiving each recommended immunization (polio, tetanus, etc.)

- For all households :

• % of households that boil or otherwise clean or treat drinking water (for prevention of diarrhea)

• % of households that keep water jugs covered (for prevention of dengue and diarrhea)

Some measures may be gathered by direct observation of household survey enumerators.

6.4.3. Knowledge of appropriate preventative health carebehavior

Because insurance is likely to provide increased exposure to public health facilities and thus to public health information provided at these facilities, family members' knowledge of preventive health behavior may be impacted. We will use the household survey to measure the following indicators of knowledge of appropriate preventive health behavior:

• % of respondents with knowledge of:

• Appropriate infant and child care (including immunizations)

• Methods to prevent dengue

Methodology for Objective 2: The determinants of take-up of health insurance

A. Statistical Methods

A major issue that voluntary insurance programs must contend with is adverse selection, whereby people who anticipate high health care costs are more likely to buy health insurance. Such self-selection increases utilization and costs. The resulting higher premiums and lower take-up can reduce or eliminate the risk-pooling aspect of health insurance.

However, another group of people that may buy health insurance are those who are very risk averse with both their health and their finances. These people may buy insurance to protect themselves financially, and may also be very healthy. If that is the case, this positive selection may balance out adverse selection and allow an insurance company to pool risks and thus remain financially viable.

Another important issue is how well insurance is reaching its target audience. SKY currently has low take-up, and one concern is that households that could be benefiting from the program are failing to enroll. A household may decline SKY due to many reasons, including low perceived risk of a negative health shock, infrequent use of health services,

little past experience with negative health shocks or large health expenditures, low perceived quality of public health facilities, inconvenience of public health facilities, low trust of institutions in general or SKY in particular, high value given to current consumption, low ability to pay for insurance premiums, high ability to pay for health care or to insure themselves in other ways, preferences against trying untested products and so forth.

Understanding the causes of low take-up will allow us to draw conclusions regarding whether households that do not buy SKY are making reasonable decisions or if SKY should explore ways to insure these households.

To measure selection into insurance, we will run five sets of analyses. First, we will look at average baseline characteristics of households that buy SKY and compare them to those who don't buy SKY. We will also run a logistical regression to determine the impact of each indicator on the purchase of insurance (where decision to purchase (0 or 1) is on the left- hand side and household and health facility quality characteristics are on the right-hand side of the equation).

Second, we will compare characteristics and average health care utilization of households that buy SKY at different prices, to examine whether a lower price induces more risk pooling (that is, attracts a healthier applicant pool).

Third, we will use external sources of data—the Cambodia Demographic Health Survey (DHS) and 2008 Census —to compare the profiles of village members who attend SKY village meetings and of SKY members to the general population (at the national, provincial and possibly district level). In the household survey our demographic measures (household size and members' age and sex) and some of our health (e.g., body mass index, that is, weight for height) and socioeconomic status (e.g., education) data are directly comparable to the DHS.

Fourth, we will examine selection using SKY data on membership and member utilization of health facilities. To illustrate the possibilities, in 2007 all but one of the SKY members who had used a health center at least once renewed their membership, which was a far higher renewal rate than among those who had not used a facility at least once. This is a simple analysis using incomplete data, but illustrates the potential for such data to provide important insights into who is keeping and who is dropping insurance.

Finally, the Village Monograph will give us an in-depth qualitative study of how households make the decision to purchase insurance, how they use health facilities differently when covered by it , and how they decide to renew or drop insurance—all of which help us understand selection into the insurance market.

B. Predictors of Take-up

We will use the following measures that are collected using the household survey (described above) to predict take-up of SKY insurance:

- Wealth/income indicators (e.g., a wealth index created by asset ownership)
- Demographic characteristics of the household (age of members, number of members, gender and age of head/primary decision-maker, etc.)
- Past health utilization of family members (as discussed in previous sections)
- Past experience with large health shocks or expenditure
- Health level of family members at date of purchase (as discussed in previous sections)
- Alternate means of health financing in the absence of insurance (access to loans, etc.)

In addition, the household survey will include measures of the following constructs that may affect take-up. When feasible, we ask who in the household was influential in deciding to buy or decline SKY insurance and will pose these questions to someone who was influential. We will measure:

• Perceptions of public health care quality
• Self-perceived predicted health risk/predicted need for health care for each household member
• Cautiousness with money (financial risk aversion), as measured by a series of hypothetical lottery games
• Cautiousness with health (health risk aversion), as measured by a series of questions regarding risky health behaviors
• Willingness to try new products
• Personal discount rate/preference for present consumption, as measured by a series of hypothetical games that trade present for future consumption
• Trust of institutions (government, NGOs, etc.)

We will also measure health care quality using the household survey and GRET's quality assessment, as described above. Finally, we will use village maps and data from the village leader to calculate distance from public and other health facilities.

Methodology for Objective 3: Effects of health insurance on public health facilities

We will examine several measures of public health center quality in the household survey:

• Perceptions of public health facilities (distinguishing whether the respondent has been to public facilities)
• For those who have been to public facilities, reports on:
o Availability of equipment and supplies (including drugs)
o Staff treatment of patients
o Staff and facility observance of official work hours
o Under-the-table payments in addition to official user fee payments

Surveys of member facilitators will provide longitudinal data on:

• Staff and facility hours (including observance of official hours)
• Completeness (and potentially, accuracy) in record-keeping
• Availability of equipment and drugs

Qualitative research will use in-depth interviews to examine how SKY has affected health facilities. Questions will cover facility equipment, supplies, and other measures of overall functioning. In addition, staff will be asked about their salaries, hours, and thoughts on SKY. Health care providers will also be asked about the care-seeking behaviors of SKY versus non-SKY members. For example, we will ask about the number of inpatient versus outpatient visits, severity of illness at visits, and days spent in the hospital for both SKY members and non-SKY members.

We will also look at trends in the quantitative measures of health center quality (from the surveys of households and of member facilitators). SKY members may increase their expectations of health center quality once they join SKY. Thus, we will emphasize attitudes of low-coupon households. SKY membership will be fairly low in this group, so any effects of SKY membership should not bias results substantially.

Methodology for Objective 4: Contribution to the general body of knowledge

Currently, several countries around the world are considering the development of health insurance programs, yet little is known about the effects of health insurance on outcomes of interest. The evaluation of SKY will thus serve as a global public good. Rigorously documenting the program's impact—both what works and what does not—will allow Ministries of Health, donors, and policymakers around the globe to learn from SKY's example. The results of this evaluation will serve as an important tool as governments and aid organizations determine whether voluntary health insurance represents a viable strategy to meet their health goals. In this way, GRET's initial investment in health insurance in Cambodia may multiply to improve health and economic outcomes in developing countries around the world.

The following outputs will allow us to contribute to the general body of knowledge on health insurance, risk research, and development:

• Each year throughout the project, we will meet with Cambodian Ministry of Health officials and international donors to ensure that relevant questions are being answered.

• As data become available, we will produce reports on program impact and take-up of insurance.

- Reports will be produced in English and Khmer, and will be distributed to donors and Ministry of Health officials.

- Reports will also be posted online and will be publicly available.

• At the end of the project, a policy brief will be produced and distributed to Ministry officials, donors, and GRET, and will be posted online.

• Results will be presented at various seminars and venues:

- Once per year, we will present a paper on the evaluation at the Cambodian Socio-cultural Research Congress held at the Royal University of Phnom Penh.

- Results will be presented at academic and non-academic seminars and presentations.

Statistical Appendix

a. Instrumental Variables

We would like to estimate the effect of having health insurance on health care utilization (or some other outcome). We might run a regression:

1) number of health center visits = β_0 + β · has health insurance + controls + ε.

Here, β is the effect of health insurance on the outcome, number of health center visits, and ε is an error term representing individual variation from the average effect of health insurance on health center visits. $\beta 0$ is a constant, representing the number of health center visits when a household does not have health insurance. Contained in ε is any factor that we are not including on the right-hand side of the regression.

If buying health insurance were completely random (at least among people with similar observable control variables), then an Ordinary Least Squares (OLS) regression of the above equation would give us an unbiased estimate of β. In that case, owning health insurance will not be correlated with the error term, and we say that the correlation of health insurance and the error term is equal to zero, or E(health insurance * ε) = 0.

Unfortunately, it is likely that buying health insurance is correlated with unobserved factors such as poor health. In that case, having health insurance will be correlated with the error term, ε, which contains the effect of unobserved influences on number of health center visits. In other words, E (health insurance * ε) is not zero. Therefore, OLS regressions of (1) are biased, meaning that the estimation of β will contain the effect of other factors besides health insurance on health. Intuitively, if sick people both have many health center visits and buy health insurance more often, it will appear that insurance raises the number of health center visits even if the causal effect is zero.

We are running a lottery for coupons that reduce the price of insurance. Thus, we can run a first-stage regression:

2) health insurance = β_0 + α · high-value coupon for insurance + controls + υ.

Here, “health insurance” is a variable equal to 1 if a household buys health insurance and 0 if a household does not buy health insurance, and “high-value coupon for nsurance” is a variable equal to 1 if a household wins a high-valued coupon for insurance, and 0 otherwise. The “controls” are any other characteristics that we believe influence the purchase of health insurance. We can estimate α using a linear probability model (that is, OLS), although other functional forms are possible. We can use the estimated coefficients to predict p^ (“p-hat”), the probability of having health insurance.

3) number of health center visits = controls + β_{2SLS} · p^.

Now we can run a second-stage equation:*

As long as the coupon given in the lottery is uncorrelated with baseline health (or other determinants of health center visits), the two-stage least squares estimate β_{2SLS} will be an unbiased estimate of the true effect of health insurance on health center visits. This procedure is called two-stage least squares or instrumental variables techniques, with the coupon value as the instrument.[5]

It might be simpler to think of the analysis in 2 pieces. We can run a non-causal reduced form equation (where Δ = change in a variable):

4) number of health center visits = controls + γ * high-value coupon for insurance.

Now the coefficient we care about is β, and can be estimated by:

β^ = Δhealth center visits /Δinsurance status

= (Δhealth center visits/Δcoupon value) / (Δ (insurance status) / Δ coupon value)

= γ^ / α^.

That is, the ratio of the coefficient from the reduced form equation (4) (how coupons affect health center visits) to the coefficient from the first stage in equation (2) (how coupons affect insurance uptake) can tell us how insurance affects health center visits.

Intuitively, assume coupons are distributed in random lotteries, and having a valuable one leads to a 50 percentage points higher probability of buying insurance. Moreover, assume households with coupon winners also have 1.5 extra health center visits per year (on average). Then it appears the causal link between buying insurance and visiting health centers = 1.5 more health center visits / 50% more insurance = 3 more health center visits for households with (exogenously) higher insurance.

A good instrument is correlated with the main variable of interest (in this example, health center visits) but not correlated with the error term of the main equation (health at baseline or anything else that predicts health center use).

[5] Mathematical note: When unobserved health status at baseline (which is contained in the residual to equation 1, ε) is correlated with the purchase of health insurance (X), the OLS estimate of the true β is biased up:

E(βOLS) = β + β · cov(ε, Health insurance) / V(health insurance) > β

Assume we have an instrumental variable Z (for example, a large-valued coupon) that is correlated with X but not with unobserved health status ε (that is, E(Z'ε) = 0). In words, Z is correlated with having insurance, but not other factors that affect health care utilization. Then we can create the instrumental variable estimate:

βIV = Z'Y / Z'X = Z'(β·X + ε) / Z'X (*)

The above equation is just the formula for calculating an IV or 2SLS coefficient. Taking expectations gives:

E(βIV) = E [(βZ'X +Z'ε) / Z'X] = E (βZ'X / Z'X) + E [Z'ε / Z'X]

Using the facts that E(Z'ε) = 0 we have

E(βIV)= β,

so the IV estimate is not biased. One can show with lots of algebra that the two-stage least square estimate of β is the same as the starred (*) IV formula above.

Our lotteries provide such a rare instrument, and thus they provide us with causal estimates of the effect of health insurance on the outcome of interest. While there are many policy-relevant questions, the coupons answer only specific questions about those whose behavior is influenced by marginal reductions in insurance prices. Because coupons are only given to households that attend the village meeting, we will only be able to see the effect of insurance on those at the margin of purchasing insurance – not those completely uninterested. What the estimates will provide us is the effect of health insurance on those people that attended the meeting and were induced to buy insurance because of the large-valued coupon.

To illustrate the technique, here is a simplified version of the analysis that will illuminate the method.

Table 4: The effect of health insurance

(with illustrative numbers)	High-value coupon	No extra coupon value	Difference
Odds of insurance	**0.6**	**0.1**	Δ Pr(insurance) / Δ coupon value = 0.5
# health center visits per household prior to lottery		**1.07**	Almost zero, as lottery tickets were randomized.
# health center visits after lottery	**2.5**	**1.03**	Δ visits / Δ coupon value = 1.5
Change in lottery visits / affected household			**1.5 / 0.5 = 3**

The estimated causal effect we want is Δ health center visits / Δ insurance

Arithmetic tells us

Δ health center visits / Δ insurance

= Δ health center visits / Δ coupon value / (Δ insurance / Δ coupon value) = 1.5 / 0.5 = 3.

That is, an extra half of those who won the coupon lottery purchased insurance, so we double the raw effect of winning a coupon on the number of health center visits to get the best estimate of the causal effect of insurance.

b. Difference-in-Differences

A very simple study of the effect of an intervention on an outcome might compare the outcome before and after the intervention. For example, one might compare average health care utilization of people before and after they purchase insurance.

Figure 1 shows average number of health care visits per year for two groups of people. Assume that Group A purchases insurance at the end of 1999. Group B does not purchase insurance. Comparing utilization of Group A before and after buying insurance would lead us to believe that insurance increases utilization by 3 visits per year (column 3, row 1).

Table 5: Utilization of Health Care Facilities

	1999	2001	Difference
Groupe A	**2.5**	**5.5**	**3**
Groupe B	**1.7**	**3**	**1.3**
Difference			**1.7**

However, this estimate ignores underlying trends in utilization that have been taking place between 1999 and 2001.

Therefore, to improve this estimate, one can use a method known as "difference in differences". The difference in differences technique uses a comparison group to establish a trend for health care utilization that takes place even without insurance. To find this underlying trend, we subtract 1999 utilization for Group B from 2001 utilization for Group B, which gives us an increase of 1.3 health care visits. If the trends between the two groups are identical, we can subtract the time trend from our initial estimate to get an improved estimate of the effect of insurance. To do this, we take the difference over time for Group A, 3, and subtract from it the difference over time for Group B, 1.3. This gives us the "difference in differences", 1.7.

However, this simple difference in differences estimate still has a problem. It is possible that Group A and Group B have different trends over time. In our case, this may occur since people who buy insurance are a selected group, and thus may change in different ways over time. In that case, subtracting out Group B's trend over time will not correctly isolate the effect of insurance, since Group B's trend is not equal to Group A's trend.

To improve upon this estimation, we want to ensure that trends over time between the two groups are equal. Randomization allows us to do this. If Group A is randomly assigned insurance and Group B is randomly selected not to receive insurance, trends between the two groups should be the same. In that case, subtracting out Group B's trend should correctly isolate the effect of insurance on Group A.

In relation to the current study, we are randomizing coupons, so that Group A randomly receives coupons and Group B does not. Figure 2 illustrates this idea. Notice that in 1999, utilization is around the same level. This is because, since coupons are randomized, the two groups should be initially about the same.

In this case, the effect we see when we look at the difference in differences, 2.6 in Figure 2, is the effect of receiving a large coupon, not the effect of insurance itself. That effect is known as the "intention to treat" (ITT), since we "intend" for people with large coupons to buy insurance. The "Methods" section of the main text gives an example of how to calculate the effect of insurance from the ITT effect.

Table 6: Utilization with Coupon Randomization

	1999	2001	Difference
Groupe A	**2.5**	**5.5**	**3**
Groupe B	**1.7**	**3**	**1.3**
Difference			**1.7**

The above calculations can also be written in regression form. For the effect of coupons on utilization illustrated in Figure 2, the regression equation is:

(5) utilization = β_0 + β*coupon + γ*2001 + δ*2001* coupon + ε

Here, utilization is the number of visits, coupon is equal to 1 if the household received a coupon and 0 if not, 2001 is equal to 1 for the year 2001 and 0 for the year 1999, and 2001*coupon is equal to 1 if the observation is a household that received a coupon and is being observed in 2001. As for the coefficients, β gives the effect of any pre-existing differences between Group A and Group B. If coupons are perfectly randomized, this should be very small. γ gives the

effect of the time trend on utilization. The coefficient of interest is δ, which tells us how much the coupon increased utilization over and above any time trend or underlying differences between the groups. There are usually additional covariates (independent variables that influence utilization, such as household characteristics). They can be added to the right-hand side of the above equation.

Note that the difference in differences estimator described above uses group means to estimate effects. The above methodology can be used even when one does not have multiple observations of the same individuals over time. All that the above methodology requires is average outcomes for a treatment and control group, before and after the treatment.

c. Fixed Effects

Fixed effects are used when we have more than one measurement of a single unit of observation. In our case, we have baseline and follow-up measurements of the same households over time. Therefore, we can use household fixed effects to improve our estimates.

Suppose we randomly give some households coupons for insurance in 1999, but do not collect any data on these households until 2001. In that case, we have cross-sectional data only, meaning one observation per household. With this data, we can run a regression of health care utilization on coupon receipt, using our data from 2001:

(6) $utilization_i = \beta_0 + \beta * coupon_i + \varepsilon_i$

In the above equation, the subscript i stands for households, so each observation is a household.

Suppose we had observations for a group of households in 1999 and for a different group of households in 2001. In that case, we could use the difference-in-differences model described in equation (5), but we still do not observe the same household over time. This type of data is called a repeated cross-section.

Suppose now that instead of just having observations at one point in time, or at two points in time for different groups of households, we have observations on the same households both before and after they receive a coupon. This type of data is called panel data. In that case, we can use fixed effects to improve the accuracy of our estimates. Essentially, what we are doing is allowing each household to have a unique baseline level of utilization. Thus, the effect of insurance is assumed to be equal for each household, but the regression line represented by the basic regression equation is raised or lowered for each household (i.e., the intercept is different for each household).

For example, if we have panel data we can take our difference-in-differences estimate from equation (5) and add fixed effects. Below, i represents a household and t represents time.

(7) $utilization_{it} = \beta_0 + \beta * coupon_{it} + \gamma * 2001 + \delta * 2001 * coupon_{it} + household_i + \varepsilon_{it}$

In the above equation, we use the difference-in-difference equation, but allow each household to have a unique (fixed) baseline level of the outcome. Essentially, what we are doing is including a variable representing each household. Thus, **household** represents not just one variable, but an entire vector of variables. Adding household fixed effects will give us a more accurate estimation of the variable of interest, δ in equation (7), since we are reducing variance due to fixed household differences.

As in equation (5), we can add additional variables to the

right-hand side of equation (7) that we think will have an effect on utilization. Fixed characteristics of households will be taken care of by the household fixed effects, but we can add household characteristics that vary over time.

d. Clustering

Finally, when we run the regression we will want to take into account clustering of observations. Clustering of observations occurs when certain observations are more similar to each other than other observations. For example, households that are located within the same village may be more similar, and may experience more similar shocks (economic, health, etc.) than households in a different village. In the same way, individuals who live in the same household may be more similar to each other than individuals in other households. If we do not correct for clustering, standard errors will appear artificially low, leading us to overestimate the accuracy of our coefficient estimates.