Evaluating the Impact of Index Insurance on Cotton Farmers in Peru[1]

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9 Case study: Evaluating the impact of index insurance on cotton farmers in Peru[2]

This section describes the research design put in place to evaluate the impact of index insurance on the welfare of small-holder cotton farmers in Peru. In contrast to the previous two case studies, this intervention is implemented by private sector actors, namely a local insurance company and bank, instead of either the government or donors. This research project is an example of a number of recent efforts by researchers to evaluate technological and contractual innovations in rural financial markets in developing countries (McKenzie, 2009).

From an academic point of view, carrying out research with private sector actors is attractive for several reasons. First, it generates direct insights into the performance of contracts and markets, thereby providing important contextualization and feedback regarding economic theory. Second, given the recent retreat of government from direct intervention in rural financial markets, the deepening and extension of rural financial markets to low income households will, to a large degree, depend on the decisions of micro-finance institutions, rural banks, and insurance companies. On the other hand, designing impact evaluations for interventions implemented by private sector actors presents a number of challenges. First, in contrast to many government interventions, the products offered by the private sector are not free. This may lead to relatively low take-up rates (demand) by the population of interest to the researcher. Second, private actors may be reluctant or unwilling to deny access to a product that they are offering in order to create the type of control group required by researchers to identify causal impact. A potential research design that addresses these two challenges, and indeed the one used in the Peru project, is the randomized encouragement design.

This case study is organized as follows. First, how index insurance works and why there is significant excitement around it as a poverty alleviation tool is explained. Next, the pilot insurance project in Peru is described. The following section then turns to the encouragement design. After laying out the general logic of randomized encouragements and the specific impact evaluation questions that it can (and cannot) answer, the specific methodology used in Peru is explained. The last section concludes with some general (and hard learned) lessons from this specific research design experience.

9.1 A primer on index insurance

In order to understand what index insurance is, it is perhaps easiest to start with what it is not. Conventional, multi-peril crop insurance provides an indemnity when the insured farmer suffers damage to his or her crop. Prior to making an indemnity payment, the insurance company typically sends an adjustor to verify that the damages actually occurred. Two immediate challenges of conventional insurance are apparent. First, verification of damages can be quite costly, especially in developing countries where rural infrastructure is poor. Second, the insurer faces significant informational problems in the form of moral hazard (did the farmer do everything he could to avoid damages?) and adverse selection (did higher risk farmers disproportionately purchase insurance?). The end result is that markets for conventional crop

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insurance tend to by very thin or completely missing unless they are accompanied by significant subsidies, a luxury developing countries can ill afford.

In contrast, index insurance provides an indemnity to insured farmers when the value of an external index crosses a critical threshold, or strike-point. An ideal index should be correlated with farmers' yields but have a probability distribution that is not affected by either the actions or composition of insured farmers. For example, an index insurance contract based on rainfall might pay out if accumulated precipitation falls below the strike-point, with payouts increasing in the size of the shortfall. Other examples of indices include alternative meteorological variables, such as wind and temperature; satellite-based indices such as the NDVI, which measures deviations in plant mass from historical means; and directly measured average yields in a specified region.[3] In addition to circumventing moral hazard and adverse selection, index insurance can (at least in theory) be offered at significantly lower cost, because the insurer does not have to verify damages on insured farmers' plots.

One of the primary challenges facing the development of index insurance markets is that it offers less protection than conventional insurance. The reason is that a farmer may suffer a loss on his or her own farm but will not receive an indemnity if the index does not fall below the strike-point. The size of this so-called "basis risk" will depend on the degree to which an individual farmer's yields co-vary with the index. In general, basis risk will be reduced, and thus the value of index insurance greatest, in contexts where covariate shocks – such as drought -- account for a large fraction of total risk and when the index closely moves with the source of the covariate shock. Other challenges include insufficient quantity and quality of historical data on indices to price contracts, lack of familiarity by farmers with insurance, and lack of trust by farmers in insurance providers.

Index insurance represents a potentially powerful poverty alleviation tool. There are two primary channels of impact. First, the availability of index insurance may permit small farmers to abandon costly self-insurance strategies; for example inducing them to move away from traditional low yield but safe crops and techniques towards more productive, although riskier, alternatives. Second, if farmers are insured, lenders may be willing to offer credit to farmers previously deemed un-creditworthy because of risk or to offer larger loans to finance larger investments. In this sense, a vibrant index insurance market may "crowd-in" credit supply.

While there is significant excitement in the academic and policy world regarding index insurance, there is little empirical evidence about its performance and impact on small farmers. Cole et al. (2008) study demand for rainfall insurance in India. Their research design includes four separate treatments hypothesized to affect demand, each randomized at the household level.[4] The treatments are: price, liquidity, informational visits, and an endorsement from a trusted microfinance institution. Each treatment is found to have a significant impact on demand in the expected direction. Cai et al (2009) use an innovative instrumental variable approach to estimate the impact of a program to insure sows in rural China. The authors create exogenous variation in insurance demand by randomizing the incentives to marketing agents that sell the insurance. The authors find positive and significant impacts of buying insurance on the number of sows owned. As described in detail below, assuming a number of conditions hold, their

[3] Barnet, Barrett, and Skees (2008) provide an overview of recent experiences with index insurance in developing countries.

[4] The authors do not evaluate the impact of insurance on farmer welfare.

estimate is a Local Average Treatment Effect; an estimate of the average impact on those farmers induced to buy insurance by receiving a sales pitch from a more highly incentivized marketing agent. Finally, Gine and Yang (2008) examine the impact of rainfall insurance on the demand for formal credit in Malawi. Their research design randomizes whether loans made by a microfinance institution are bundled with the insurance. The authors find the counter-intuitive result that insurance reduces the demand for credit. They attribute this result to the fact that the credit contract already provides some insurance via limited liability so that bundling insurance with the loan implies an increase in the interest rate without any additional insurance.

The research project described below is an attempt to provide additional evidence on the performance and impact of index insurance in developing countries.

9.2 The intervention and primary research hypotheses

In 2008, a research team from the University of California at Davis and the Instituto de Estudios Peruanos, in conjunction with a local insurance company and micro-finance institution, launched a pilot program to examine the impacts of index insurance on cotton farmers in the valley of Pisco, on Peru's south coast. The valley contains approximately 22,000 irrigable hectares and is dominated by small-holders who work less than 10 hectares. Cotton is the dominant crop in the valley, accounting for between 50–75% of planted area over the last decade. On average, approximately 60% of the 5,000 farmers in the Pisco valley plant cotton each year.

The insurance product developed for this project, called Agro-Positiva, is an area yield insurance contract. The index is the average yield of planted cotton area in the valley. The first step in designing the contract was to estimate the probability density function of area yields in the valley. This was done using the 25 year time series of annual yield estimates published by the Ministry of Agriculture between 1982 and 2006.[5] The estimated distribution function was then used to calculate the actuarially fair premium. The contract established a strike-point of 1,412 kilograms per hectare, or 85% of average valley yield. If the average valley yield falls below the strike-point, policy holders are paid an indemnity of $0.63 per kilogram below the strike-point. The final premium was set at $48 per insured hectare. The actuarially fair premium was $22 per hectare. The final premium reflects loading costs (administrative costs, taxes, plus profits) of the insurance company minus a 30% premium subsidy provided by the Ministry of Agriculture.

While the research team conducted the statistical analysis underlying the insurance contract, the contract is offered by a triangular arrangement involving three private institutions. The insurance contract is formally offered and registered with the Superintendence of Banks and Insurance by the Peruvian insurance company La Positiva. The insurance is sold, however, by the Caja Rural Señor de Luren, a locally-based rural bank and the largest formal lender to agriculture in the region. The Caja is essentially an agent of the insurance company; collecting a small commission for each policy sold. Finally, HanoverRe provides reinsurance to La Positiva.

9.2.1 Hypotheses to be tested

The primary objective of the research program is to evaluate the impact of this insurance contract on three types of farmer-level outcome variables. First, as suggested above, index insurance may relax credit constraints faced by farmers, leading to an increase in farmers' participation in the

[5] Using maximum likelihood, the de-trended yield data were used to fit a Weibul distribution.

formal credit market. On the supply side, quantity rationing may be reduced if insurance induces lenders to extend credit to farmers that were previously deemed too risky. The bundling of insurance with credit in the contract was designed, in part, to maximize this supply side impact. Indemnity payments are made directly to the bank in order to pay down the loan. After canceling the loan, any remaining indemnity payment goes to the farmer. On the demand side, insurance may reduce risk rationing, which occurs when farmers qualify for loans but choose not to borrow because they are afraid of losing the assets (typically land) required as collateral (Boucher et. al., 2008). As it should reduce the risk of default and collateral loss, index insurance is expected to reduce the incidence of risk rationing. In the survey, a module to directly elicit farmers' credit rationing status in the formal sector is therefore included.[6]

Second, index insurance is expected to increase the intensity of input use and investment on cotton plots. On one hand, insurance should increase a risk averse farmer's input demand, moving the farmer towards a risk neutral, profit maximizing level. On the other hand, insurance – by relaxing credit constraints – will allow this increased input demand to be met. In the case of cotton in Pisco, yield is significantly affected by the farmer's per-hectare expenditure in land preparation and fertilization. In the survey, detailed information is collected on these costs.

The first two types of hypothesized impacts can be considered intermediate outcomes. Both credit access and the intensity of input use are mechanisms through which insurance may affect farmers' welfare. The research project is also concerned with measuring impacts on welfare. As such, the household survey also measures cotton yield and net income from cotton production.

9.3 Research design: Randomized price encouragement

In order to test the hypotheses and examine the impact of index insurance on the outcome variables described above, a randomized encouragement design was implemented. The section is divided into two parts. First, a simple model that will illustrate the basic idea underlying the encouragement design is outlined. Then, the specific types of average treatment effects that can be answered are defined.

9.3.1 A basic econometric model

The following simple model, based on Moffitt (2008), illustrates the basic ideas underlying the encouragement design. It consists of the following 2 equations:

$$y_i = \beta_i + \alpha_i d_i \tag{13}$$

$$d_i = 1\left\{k\left(z_i, \delta_i\right) \geq 0\right\} \tag{14}$$

In equation (13), the variables y_i and d_i are, respectively, the observed outcome variable (yield in this example) and a binary indicator taking value 1 if the farmer buys insurance and 0 if not. β_i is the individual specific parameter giving the farmer's yield without insurance; $\alpha_i + \beta_i$ is the farmer's yield with insurance; and thus the parameter α_i is the change in yield due to purchasing insurance. Equation (14) states that the farmer decides to purchase insurance if the function $k(z_i, \delta_i)$ is positive. This decision is a function of the observed value of z_i and the unobserved

[6] See Boucher, Guirkinger, and Trivelli (2009) for a description of the direct elicitation methodology.

parameter, δ_i. The variable z_i is an *instrument* which obeys certain assumptions, which are discussed below in greater detail.

This potential outcome framework illustrates the fundamental evaluation problem. At any given point in time, either yield with insurance, $\alpha_i + \beta_i$, or yield without insurance, β_i, can be observed but not both. Thus, the impact of insurance, α_i, for an individual cannot directly be observed. One might be tempted to compare mean yields of insurance purchasers versus non-purchasers. Since farmers self-select into the insurance market, however, this comparison would likely give a biased impact estimate. Specifically, the unobserved determinants of demand, δ_i, are likely to be correlated with the unobserved impact of insurance, α_i.[7]

So how can an unbiased average treatment effect be estimated? One possibility would be to directly randomize farmers into and out of the index insurance program. In other words, use a randomized control trial with perfect compliance, or in which everybody offered insurance purchases it, while nobody who is not offered insurance purchases it. Perfect compliance, however, is unlikely for several reasons. First and foremost, it is unfeasible (and unethical) to compel farmers to purchase insurance.

An alternative strategy is to use an instrumental variable strategy in which the researcher randomizes a variable that affects the probability that a farmer purchases insurance but does not directly affect yield. Conceptually, the simplest strategy is to randomize farmers' *eligibility* to purchase insurance. This research design would require a mechanism by which randomly chosen farmers are not allowed to purchase insurance (ineligibles) while others are allowed to purchase it if they want it (eligibles). In some contexts this will be a viable strategy and, if followed rigorously (i.e., the ineligible group is truly randomly selected and effectively denied access), can yield consistent estimates of the average treatment effect. This is the strategy pursued by Giné and Yang (2009) in their study of the impact of rainfall insurance on credit demand in Malawi. In the Peruvian context, it was not feasible to randomize eligibility because neither the insurance company nor the lender was willing to deny access to a random group of farmers.

Given the infeasibility of randomizing eligibility, the option of a randomized encouragement design was used. In this case, while all farmers are eligible to purchase insurance, a subset of farmers is randomly selected to receive an additional "encouragement" to purchase insurance in the form of discount coupons that lower the price of the premium. To see how this random encouragement can generate consistent estimates of average treatment effects, return to the basic equations. Now let z_i indicate the coupon value. For simplicity, assume there is a single coupon value so that z_i takes value 1 if the farmer receives a coupon and 0 if not.

Taking conditional expectations of the model with respect to z_i, yields:

$$E\left(y_i \mid z_i = z\right) = E\left(\beta_i \mid z_i = z\right) + E\left(\alpha_i \mid d_i = 1, z_i = z\right) P\left(d_i = 1 \mid z_i = z\right) \tag{15}$$

$$E\left(d_i \mid z_i = z\right) = P\left(d_i = 1 \mid z_i = z\right) = P\left(k\left(z_i, \delta_i\right) \geq 0\right) \tag{16}$$

where $P(.)$ denotes probability and (15) follows because d_i is a [0,1] binary variable.

[7] For example, farmers with lower unobserved basis risk – who are thus be more likely to purchase insurance -- may tend to have more productive land. In this case, the average yields of non-purchasers would underestimate what the average yields of purchasers would be in the absence of insurance. The result would be an over-statement (positive bias) of the average treatment effect.

In order to identify average treatment effects, the instrument (our coupon) must satisfy the following four criteria (Moffitt 2008). First, z_i must satisfy *mean independence*:

$$E\left(\beta_i \mid z_i = z\right) = \beta \tag{17}$$

Independence implies that coupon assignment does not depend on what individuals' average outcomes (farmers' yields) would be without insurance. Since the evaluation will be comparing average outcomes across farmers with and without coupons, this criterion ensures that the non-recipients are a good counterfactual to the recipients. By randomly assigning who gets coupons, it is more likely that the independence criterion is satisfied.[8]

Second, z_i must satisfy an *exclusion restriction*:

$$E\left(\alpha_i \mid d_i = 1, z_i = z\right) = g\left(P\left(d_i = 1 \mid z_i = z\right)\right) \tag{18}$$

The left hand side is the average gain in the outcome among those assigned $z_i = z$. The right hand side rewrites this expectation as the function $g()$, which in turn is a function of $P\left(d_i = 1 \mid z_i = z\right)$, the probability that a person assigned $z_i = z$ buys insurance. This criterion requires that the instrument (coupon) has no direct effect on the outcome of interest (yields); instead the instrument only affects the outcome through its impact on the probability that individuals purchase insurance. Given that α_i is likely to vary across farmers, the impact of insurance that is measured in an encouragement design depends crucially on the composition of farmers that are induced to purchase insurance by the instrument (coupon). This point will be returned to shortly. For now, note that a coupon that lowers the price of insurance should have no direct effect on farmers' investment decision and is thus likely to satisfy the exclusion restriction.

Third, z_i must be *relevant*:

$$Cov\left(z_i, d_i\right) \neq 0 \tag{19}$$

This criterion implies that the coupon has some predictive power with respect to whether or not farmers buy insurance. The stronger is the covariance, the easier it will be to detect impacts of a given size.

These first three criteria are similar to those underlying a conventional instrumental variable approach. In the case of an encouragement design, one additional criterion is required that will permit an interpretation of the estimates as an average treatment effect. This fourth assumption is *monotonicity*:

$$d_i = 1 \mid z_i = z^j \rightarrow d_i = 1 \mid z_i = z^k \text{ and } d_i = 0 \mid z_i = z^k \rightarrow d_i = 0 \mid z_i = z^j \;\; \forall i, \forall z^k > z^j. \tag{20}$$

This criterion, introduced by Imbens and Angrist (1994), says that the values of the instrument z_i can be ordered in such a way that moving from z_i^j to z_i^k, the sign of the impact on the decision to participate must be the same for everyone. In this case, giving farmers a coupon (or a larger

[8] As in the case of RCT's, randomization of the instrument does not necessarily imply that independence is met, especially in small samples. The researcher should always check the quality of the randomization by comparing means of variables hypothesized to be related to the outcome variable across groups assigned different instrument values.

coupon in the case of multiple coupon values) should either induce them to buy insurance or have no effect, but not push some people to buy insurance while dissuading others.[9]

With these assumptions in hand, equations (13) and (14) can be rewritten in an estimable form as:

$$y_i = \beta + g\left(P\left(d_i = 1 | z_i = z\right)\right)P\left(d_i = 1 | z_i = z\right) + e_i \tag{21}$$

$$d_i = P\left(d_i = 1 | z_i = z\right) + u_i \tag{22}$$

Equation (21) states that the value of y_i for everyone assigned $z_i = z$ is equal to the mean outcome without insurance, β, plus the average impact of insurance given the share of this sub-population that buys insurance, $g(P(.))$, weighted by the share of this same sub-population buying insurance, $P(.)$, plus white noise. The error terms e_i and u_i are random variables with expected values equal to 0, conditional on the probability of buying insurance.

9.3.2 What treatment effects can be estimated?

Theory suggests that the impact of insurance is likely to be quite different across individuals depending on several difficult to observe factors including risk aversion, credit constraints, farm aptitude, etc. For this reason, the theoretical model is written to allow α_i, and thus also the function *g(.)*, to vary across farmers. This has important implications for the type of treatment effect that can be estimated.

The first treatment effect that one might want to estimate is the marginal treatment effect (MTE):

$$\frac{\partial E\left(y_i\right)}{\partial P\left(d_i = 1 | z_i = z\right)} = g\left(P\left(d_i = 1 | z_i = z\right)\right) + \frac{\partial g\left(P\left(d_i = 1 | z_i = z\right)\right)}{\partial P\left(d_i = 1 | z_i = z\right)} P\left(d_i = 1 | z_i = z\right) \tag{23}$$

If the outcome variable of interest is yield, the MTE would give the change in the average yield due to an arbitrarily small change in the share of the farmers purchasing insurance. If the impact of insurance is heterogeneous across farmers, the MTE will vary depending on who the marginal farmers are that are induced to participate. To estimate MTEs, first it is necessary to estimate insurance demand (equation (22)) using a model, such as logit or probit, that generates a continuous range for the probability of purchasing insurance.[10] Once predicted probabilities of buying insurance have been generated for the sample, the MTE can be estimated by picking a continuous flexible functional form for $g(.)$, and estimating it along with its derivative with respect to the probability of buying insurance.

Conceptually, the MTE is the building block for all other treatment effects, as the latter can be expressed as weighted averages of the former (Heckman and Vytlacil, 2007). In practice, however, the MTE is not commonly estimated because the data requirements are steep; it

[9] If providing a coupon had opposing effects on farmers, then the estimated expected gain from buying insurance would include the expected gain among those induced to buy insurance by the instrument, minus the expected gain for those induced to not buy the insurance. In general, the direction of the inequalities in equation 20 is not important, as they could be reversed and the assumption would still serve its purpose.

[10] Semi-parametric estimators offer more flexible alternatives to the logit and probit models. See the reviews of these methods by Ichimura and Todd (2007) and Chen (2007).

requires a sample with sufficient observations at multiple values of the instrument, or a continuous instrument.[11]

A much more commonly estimated treatment effect with encouragement designs is the Local Average Treatment Effect (LATE). The LATE, introduced by Imbens and Angrist (1994), is the discrete version of the MTE. The LATE for any pair of instrument values z^1 and z^2, is:

$$\frac{E\left(y_i \mid z_i = z^2\right) - E\left(y_i \mid z_i = z^1\right)}{P\left(d_i = 1 \mid z_i = z^2\right) - P\left(d_i = 1 \mid z_i = z^1\right)} \tag{24}$$

The LATE is quite intuitive. Suppose, for example, that z is the price of insurance, and half of the sample has been randomly chosen to receive a discounted price z^2 should they elect to buy insurance, while the other half must pay the market price z^1. The numerator in equation (24) is the difference in expected yields between farmers offered the discounted premium and those offered the market price, while the denominator is the difference in the fraction of farmers purchasing insurance across the two groups. The LATE is thus the average impact of buying insurance on the outcome of interest for "compliers," i.e., individuals who would buy insurance if assigned the lower price, z^2 , but not buy insurance if assigned the higher price, z^1. Impacts on individuals who would always buy the insurance (the "always-takers") or would never buy insurance ("never-takers") given the values of z are not represented in the estimated LATE (Angrist and Imbens, 1995). The inequalities in the monotonicity condition (equation (20)) rule out the possibility of "defiers," i.e., individuals who would only buy the insurance if not assigned the lower price.

Finally, note that estimation of the LATE is straightforward. It can be estimated non-parametrically by replacing the components of (24) with sample averages, or via two-stage least squares.

9.4 Implementation in Pisco

In this section, the operationalization of the encouragement design in the research setting in Pisco, Peru is described.

9.4.1 Choice of instruments

The primary instrument used in the encouragement design is randomly distributed coupons which lower the price of the insurance premium. Four different coupon values were distributed: 15 S/., 35 S/., 65 S/., and 90 S/. per insured hectare.[12] The 90 S/. coupon lowered the effective price of the insurance to just below the actuarially fair rate. Five hundred and forty-three cotton farmers were randomly selected to receive the coupons, with one-fourth receiving each value.[13] Randomly distributed coupons were expected to serve as an ideal instrument for the encouragement design as it would likely meet the four criteria listed above. First, by randomizing the distribution of the coupons, the value of the coupon received should be independent of farmers' yields and other outcome variables. Second, the exclusion restriction should be satisfied

[11] For applications of MTE estimation, see Heckman et. al. (2006), Carneiro et al (2006), and Carneiro and Lee (2009).
[12] The Peruvian currency is the Nuevo Sol and $1 = S/.3.2.
[13] The cost of the redeemed coupons is assumed by the research team.

as variation in the price of insurance may impact the composition of insurance purchasers but should have no direct impact on farmers' outcome variables. Third, given their monetary value, the coupons were expected to significantly affect farmers' demand for insurance. Finally, monotonicity is likely to be satisfied as a lower price should induce some farmers to purchase, but would never dissuade a farmer who would purchase without the coupon from buying with a coupon.

In addition to the coupons, invitations to insurance education sessions were randomly distributed. In the sessions, farmers participated in a game, based on experimental economics, which simulated farmers' credit, technology and insurance purchase decisions. Participating farmers earned between $2 and $15, depending on their choices and random draws simulating covariate and idiosyncratic shocks.[14] In addition, a member of the research team made a brief presentation describing index insurance. The distribution of invitations had two levels of randomization. First, 16 of the 40 irrigation sub-sectors in which the valley is divided were randomly chosen. In each of the sub-sectors, a single educational session was carried out. Within each sub-sector, invitations to the session were delivered to 60 randomly selected cotton farmers. Only invited individuals were allowed to participate. Approximately 75% of invitation recipients attended the sessions. While the original intention was that the invitations to the education sessions would serve as a second instrument, eventually it was concluded that the sessions may violate two of the criteria. First, because significant information was provided about yield risk, it was possible that participation in the sessions could alter farmers' perceptions of risk which, in turn, may have a direct impact on production and credit decisions. Second, for related reasons, monotonicity may not hold. While the sessions likely induce some farmers to purchase insurance, they may dissuade other farmers who, in the absence of the education sessions, would have purchased insurance for example because of heightened sensitivity to risk. While invitations to the education sessions could (and should) still be used as a control variable for both insurance demand and impact, it was decided not to rely on this variable as an instrument.

9.4.2 Sample size calculations

In order to choose the sample size, a slightly modified version of the power calculation given by equation (7) in section 5 was used. Specifically, at 80% power and a significance level of 5%, the minimum detectable effect size, α_{MDE}, is given by:

$$\alpha_{MDE} = 2.49 * \sqrt{\frac{\sigma^2(1-R^2)}{NP(1-P)}}\frac{1}{c-s} \qquad (25)$$

where σ^2 is the variance of yield or an alternative outcome variable. R^2 is the share of the variance of yields explained by the model. N is the total sample size, and P is the share of the sample that receives the encouragement (a coupon of any size). Finally, c-s is the difference in compliance rates between encouraged farmers (coupon recipients) and non-encouraged farmers (coupon non-recipients). Specifically, c is the share of coupon recipients that purchase the insurance while s is the share of non-recipients who buy the insurance. Equation (25) demonstrates an important risk of the encouragement design. If the instrument is weak, then compliance rates between encouraged and un-encouraged farmers will be low. The term $1/(c\text{-}s)$ will be large and, as a result, the precision of the estimate will be low, thereby requiring the true

[14] The daily wage in agriculture is approximately $7.

impact to be quite large in order to be detected for a given sample size. If, on the other hand, there is perfect compliance, the term 1/(*c*-*s*) takes its minimum value equal to 1.

Determining the sample size requires making some educated guesses, since neither σ^2, R^2, nor the compliance rates, *c* and *s*, are known. To make the calculations, R^2 is set to 0.3, which is a typical R^2 value for yield regressions. Taking advantage of a panel data set of farm yields conducted by the Ministry of Agriculture, the variance of cotton yields, σ^2, was estimated. This data set, called ENAPROVE, was applied annually to approximately 250 cotton farmers between 2002 and 2006 in Pisco. Given the potentially strong impacts on the precision of the estimates to compliance rates, equation (25) is used to calculate the necessary sample sizes for different compliance rates. Figure 6 graphs α_{MDE} as a function of the sample size for several different compliance rates. In all cases, sample is equally split between encouraged and un-encouraged farmers.

[FIGURE 6 HERE]

Figure 6 shows the sensitivity of the minimum detectable effect size to compliance rates. At a sample size of 800, insurance would have to raise yields by just under 1,000 kilograms per hectare, which is over 60% of mean yields, in order to be detected when the coupons raise demand by just 10%. This drops to just under 500 kilos per hectare when coupons raise demand by 20% and to 311 kilos per hectare, or about 18% of mean yields, when coupons raise demand by 30%. Based on these calculations, a sample size of 800 farmers was chosen, with an equal split between coupon recipients and non-recipients.

9.4.3 Sample frame and sample selection

In order to draw the sample of cotton farmers, the research team worked closely with the Irrigation Commission (*Junta de Regantes*). To gain access to irrigation water, each farmer must file with the Commission a production and irrigation plan (*plan de cultivo y riego*). The Commission thus maintains a list of the population of farmers and the areas planted to each crop. Using this list, farmers throughout the valley were randomly selected for inclusion. A letter describing the research and notification of an enumerator visit along with the coupon was distributed to each farmer selected in the sample by the Commissions field staff (*sectoristas*).[15]

9.4.4 Household surveys

A baseline survey was applied to the 800 farm households in August of 2008, prior to the first year of insurance availability. The survey collected data on household demographics, land holdings, cropping patterns, detailed production and cost data for cotton, and a credit market module designed to explore terms of credit access and identify households' credit rationing status. The survey was repeated in August 2009, with a final survey planned for August 2011. Cuanto, a local firm with significant experience carrying out agricultural surveys, was hired to administer the survey. Field supervision was provided both by Cuanto as well as a team of graduate students from the Instituto de Estudios Peruanos and UC-Davis.

9.4.5 Budget of survey administration

[15] The research team paid a small honorarium plus gasoline costs to each sectorista and contributed a printer to the central office in recognition of their support.

As described above, the research team hired a local survey firm to administer the survey, enter the data, and deliver a cleaned data set in STATA format.[16] The survey firm charged $85 per survey, for a total cost of ($85/survey)*(800 surveys) = $68,000 per survey round. The research team also assumed the cost of measuring average valley yield to determine whether or not an indemnity payment would be made. This required applying a separate, but much shorter, yield survey to 650 households. The same survey firm was hired to carry out the yield survey at a cost of $11 per survey, or ($11/survey)*(650 surveys) = $7,150 per round. This gives a total cost of $75,150 per survey round. With three annual survey rounds, the total research cost is approximately ($75,150/round)*(3 rounds) = $214,500.

9.5 Pisco sour?[17] Lessons from a discouraging encouragement design

In spite of the best intentions of the research team, the demand for insurance among farmers in Pisco has been disappointingly low. In the first year, a total of 51 farmers purchased the insurance, with coverage of just over 140 hectares. This rose to 75 farmers with 220 hectares in the second year and 150 farmers with just over 400 hectares in the third year. While the increase in demand raises hope for the continuing development of the market, the initially low take-up rates greatly reduce the ability of the team to conduct impact evaluation of the insurance program. Most importantly, the coupon turned out to be a surprisingly weak instrument, with take-up rates essentially independent of whether or not farmers received a coupon and, conditional on receiving a coupon, on the size of the coupon. The fraction of coupon recipients purchasing insurance across all three years is less than 5% greater than non-recipients. Referring back to Figure 6, with a sample size of 800, it would be necessary for insurance to more than double farmers' yields, which is clearly unreasonable, in order to detect any significant effect.

So what went wrong? Why has take-up been so low and farmers so unresponsive to price reductions? Several hypotheses have been explored which are briefly outline here.

First, farmers' understanding of index insurance may remain low. While the experimental game has high potential for teaching farmers about index insurance, it appears that the games were less effective than initially hoped at conveying the basics of the insurance contracts.[18] Based on an exit survey from the games and focus group discussions, many farmers did not understand that the payout depends on average instead of individual yields. In addition, many farmers whose yields were consistently higher than the valley average were quite skeptical about buying the insurance because their *own* yields were very unlikely to fall below the strike-point. Additional work is needed in improving farmer education, in particular to convey the notion that the value of the insurance depends on the degree of co-movement between an individual farmer's yields and the index (in this case valley average yield).[19]

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[16] The research team developed and field tested the questionnaire, created the sample frame and drew the sample. The research team also trained the survey firm project managers and enumerators. Neither these costs, nor the time of UC-Davis faculty and graduate students, are included in the budget figures reported in the text.

[17] Thanks to Alan DeBrauw for suggesting this sarcastic, but creative, title.

[18] At the end of the game sessions, a brief survey was administered to farmers. One question asked if the indemnity payout depended on the farmer's draw from the idiosyncratic shock bag. Just over a quarter of the farmers incorrectly said that it did.

[19] An entirely different, although perhaps no less important, challenge is the notion of "average". Many farmers seem to equate "average yield" (*rendimiento promedio*) with the parcel's potential yield or the yield they would expect to get in a good year. Similarly, farmers seem to discount exceptionally bad years from their mental calculation of average yields. This factor tend to make farmers' perceptions of average yields (both their own and

A second problem that emerged rather unexpectedly was a resistance to the project from the manager of the local branch of the Caja Rural. In hindsight, the research team failed to develop sufficient understanding of the incentives in the management system in the Caja. At the outset of the project, the research team negotiated a participation agreement with the board of directors of the Caja. The board of directors, who meet in the departmental capital in the city of Ica, then passed the decision down to the manager of the Caja branch in Pisco. For a number of reasons, this manager was not enthusiastic with the project. First, the manager likely resented the process; the order to implement an experimental pilot program came down from above without any input from the branch itself. Not only was the order to participate very vertical, but it also implied costs in terms of time and training for the loan officers who would be the face of the insurance product. Second, the board of directors ordered an interest rate cut on loans for farmers who also purchased the insurance. It was later learned that the manager resented this because he felt it reduced the branch's earnings. Although in the long run, insurance would likely reduce default rates and thus offset the interest loss, the manager was understandably concerned with the short run earnings position of his branch.

Third, communication between the insurance company and the Caja was less than optimal. For example, there was confusion about who would lead the marketing campaign for the insurance product. By the time the confusion was cleared up, the insurance sales season had already begun.[20]

9.6 Concluding remarks

Encouragement designs offer a potentially viable research design in contexts where randomized control trials are not feasible. This is likely to be the case for the types of innovative contract design in financial markets, such as the index insurance example described here, because private actors—insurance companies and lenders—may be reluctant to deny access to new products to maintain a strict "control" group. Encouragement designs can also be valuable when participation rates are likely to be low, thus reducing the precision of impact estimates. Mullally (2010) suggests that encouragement designs may be especially valuable in contexts of new products or markets. Providing additional incentives, such as below market prices, can induce farmers who would otherwise not participate for lack of understanding or familiarity to participate. Viewed this way, under certain circumstances an encouragement design could be viewed as a public policy to get nascent markets off the ground.

As evidenced from this experience with index insurance in Peru, encouragement designs face a number of challenges. First and foremost, identification of average treatment effects requires an

in the valley) quite a bit higher than the statistical mean. Equivalently, farmers' subjective pdf's of average valley yields may be shifted significantly to the right relative to the pdf generated from official statistics. If this is the case, then farmers would underestimate the value of the insurance and be less likely to buy it.

[20]Attempts have been made to address these problems by increasing communications flow between all parties and, in particular, by attempting to create incentives for the manager to fully get behind the project. In part this has taken the form of including the manager and the loan officers in discussions about how to improve the product, making it more valuable to their clients. Monetary incentives have been provided for loan officers and the manager based on the number of policies sold.

effective instrument that induces participation in the project or, in this case, take-up of the contract. If instead the instrument turns out to be weak, then the overall research agenda may be compromised. In retrospect, the research team would have been well served carrying out more preliminary research, perhaps via focus groups, to evaluate the likely effectiveness of the coupons.

A second caveat to encouragement designs, and indeed to any instrumental variable approach, is that the researcher must be cautious in interpreting the estimated treatment effect. Typically, encouragement designs permit estimation of local average treatment effects, which are the average impact on compliers, those induced to participate by the encouragement. If theory suggests that impacts vary across the population, in particular across difficult to observe characteristics, then the composition of the compliers must be considered. Are the compliers the most interesting group for policy purposes? In the Peru example, it is important to ask how useful it would be to learn the impact of insurance on farmers that are only induced to purchase insurance if they receive a very high level of price discount. Given the fiscal constraints of governments, it is unlikely that these farmers would participate in any insurance program that would exist in the real world.

Finally, the experience suggests that, while there are a unique set of challenges in designing impact evaluations around private sector interventions, collaboration with the private sector can be both feasible and productive. This is particularly encouraging given that the types of financial market innovations most likely to benefit until-now excluded segments are likely to be driven by these actors. An increasingly rich set of innovative and rigorous collaborations across academia, the public sector and the private sector is anticipated as a means of pushing poverty alleviation policies forward in the future.

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Figure 6: Minimum detectable effect as a function of sample size