Abstract: Recent emphasis in applied development economics has been on evaluating complex financial market interventions, such as microfinance programs, savings mechanisms, and innovative insurance products. However, these programs are often plagued by low participation, making the task of impact evaluation more difficult due to imprecision of estimates. Furthermore, the program impacts identified by different designs will vary when individuals are heterogeneous in ways that affect program participation and outcomes. This paper is a methodological exploration of research design in the presence of low participation and individual heterogeneity. To put this issue in context, I use the example of index insurance, an innovative financial tool characterized by low participation rates. I focus on the choice between a research design based on randomized eligibility and a randomized encouragement design. Randomized encouragement designs offer a stronger incentive for program participation to a randomly chosen subpopulation, and then use the incentive as an instrumental variable in econometric impact evaluation. In general, the effect estimated by a randomized encouragement design will be biased relative to the true impact of a program on participants. However, bias may be offset by greater precision, resulting in an estimator with a relatively low Mean Squared Error. In addition, greater unobserved heterogeneity among the study population will not necessarily increase this bias. These conclusions depend on the nature of the program and outcomes being studies, and ought to be considered carefully by researchers weighing alternative research designs.
1 Introduction
Direct randomization into treatment and control groups is a powerful tool for the evaluation of development programs, as it will identify the distribution of treated and untreated outcomes under relatively weak assumptions (Duflo, Glennerster and Kremer 2006). However, randomizing treatments in this way is ruled out in many types of programs where participation must be voluntary. For example, in the context of financial market interventions in developing countries, households usually cannot be compelled to take out a loan from a micro-lender or be forced to purchase insurance.

Two alternative research designs are a randomization of eligibility and a randomized encouragement. The former entails randomly assigning program eligibility but allowing for voluntary participation, while the latter consists of allowing all households to be eligible but offering a randomly chosen group an extra incentive for participation that raises enrollment. A randomization of eligibility will identify the average impact of a program on participants, while the randomized encouragement will identify the average impact on participants induced to join the program by receiving the extra incentive (Imbens and Angrist 1994).

In general, these two effects will not usually coincide, because they capture effects on two different groups within the population (Heckman and Vytlacil 2007). This would seem to suggest that randomizing eligibility is the superior strategy. But when participation rates are low among eligible households not offered any extra incentive for enrollment, the identifying power of the randomization of eligibility can become very weak, and it will no longer be clear which strategy will yield the estimated effect that is closer to the truth on average.

This essay is a methodological study of the tradeoffs faced by development economists when choosing a research design for impact evaluation given low program participation rates.
individual-level heterogeneity that shapes program participation and outcomes, two characteristics that often describe interventions in financial markets in poor countries (McKenzie 2009). Poorly functioning financial markets are thought to be an important determinant of poverty, as market failures drive households to use costly alternative strategies to manage risk and maintain smooth consumption levels (Rosenzweig and Binswanger 1993; Carter, et al. 2007; Dercon 2004). Identifying effective financial market interventions is thus a worthy policy goal, but can be made exceedingly difficult by low program enrollment rates.

Low participation in financial programs might be accepted as a fact of life if it were the result of fully informed households optimizing their behavior. Researchers in this case would have to use extremely large sample sizes for econometric analysis. But the literature on financial market participation in the rich world provides evidence for the influence of factors such as trust in institutions and financial education on household decision making in this context (Guiso, Sapienza and Zingales 2008; Guiso and Jappelli 2009). These factors are likely to be even more prominent in less developed areas where households have little experience with formal finance and low levels of education, making it far from clear that a lack of program benefits is what is driving low financial program enrollment.

This suggests that it would be a worthwhile endeavor to increase participation rates in a way that allows for sound econometric analysis of program benefits. This essay uses the example of index insurance to examine the potential of a randomized encouragement design strategy to accomplish these goals. Index insurance is a financial market innovation which has been characterized by low participation rates in developing countries (Hellmuth, et al. 2009). In Section 2, I briefly summarize the literature on demand for index insurance. In Section 3, I build a theoretical model in which households are faced with the choice of using a safe technology, a
risky high-return technology, or pairing the risky high-return technology with index insurance. To place the model in context, I parameterize and simulate it using data on cotton production in the southern coast of Peru, which has been the site of an index insurance pilot project in recent years. I then demonstrate what happens when farmers lack trust in the insurer, and believe they will be cheated out of a portion of the indemnity. As a result of this lack of trust, participation falls, and households that could benefit from index insurance do not do so.

In Section 4, I link the theoretical model to econometric program evaluation, and examine the problem of measuring the impacts of index insurance in this model economy from the perspective of an analyst who cannot observe household-level heterogeneity that is driving both technology choice and insurance demand. A randomization of eligibility and a randomized encouragement design are compared in this context, bringing into sharp relief the strengths and weaknesses of each approach. If researchers focus solely on unbiasedness, then randomizing eligibility will be preferred. But when participation rates are low, randomized encouragements may be preferable to randomized eligibility when the choice of research design is based on traditional model selection criteria such as Mean Squared Error.

I also explore the effects of greater unobserved heterogeneity that is correlated with program uptake and outcomes of interest on the program effects identified by each strategy. While intuition might suggest that greater heterogeneity of this sort would result in larger differences between the program effects identified via each strategy, I demonstrate that this is not necessarily the case. Depending on the characteristics of the program and outcome variables being studied, more heterogeneity of this sort can actually bring program effects closer to one another. Section 5 demonstrates these points via a simulation, and Section 6 concludes.
2 Index insurance: an example of an innovative financial market intervention

2.1 Definition of index insurance
As summarized above, households in developing countries may adopt costly risk management strategies. This is of particular concern among rural households, given the risks associated with agriculture. Crop insurance seems like a natural response to this problem, but traditional crop insurance based on compensating farmers for losses relative to a farm-specific historical level of output or income has proven extremely expensive, due to the incentive problems introduced by covering household-specific risks without the ability to perfectly monitor behaviors (Hazell 1992).

High cost makes traditional insurance a less than desirable policy option for governments in developing countries. Index insurance is an alternative to traditional crop insurance that bases payouts on an index that is highly correlated with crop yields but beyond the control of any single household. For example, an index product that bases payouts on rainfall levels would pay households if precipitation levels were to fall below a “strike point,” with the payout increasing in the size of the shortfall. Since it does not cover all risks, index insurance will not offer protection to the same extent as traditional crop insurance, but it avoids the incentive problems which have made the latter costly and fiscally unsustainable.

2.2 Demand for index insurance
The empirical literature to date on index insurance generally reports low levels of demand. While variables such as price and access to liquidity are found to affect demand, other factors outside the scope of a model of perfectly informed optimizing households also influence uptake. Giné, Townsend, and Vickery (2007) found that being a past credit client of the institution offering a rainfall insurance product in the Indian village of Andhra Pradesh has a positive and significant
effect on demand; financial sophistication and trust in the institution could both explain this finding. Focusing on the same insurance product studied by Giné, Townsend, and Vickery, Cole et al. (2010) randomized factors that might influence demand for rainfall insurance and measured their effects using a sample drawn from a larger number of villages. Varying the price of the insurance and providing households with enough cash to buy a policy had significant effects on demand in the expected direction, but a household sales visit also had a large and positive effect.

Cai et al. (2009) measured the impact of an insurance product for sows on investment by farmers in China. In order to generate exogenous variation in demand, the authors randomly varied the incentives faced by marketing agents charged with selling the insurance policies. Living in a village served by a marketing agent facing stronger incentives had a strongly positive and significant impact on demand. The authors also found that households which were already receiving government subsidies from other programs were significantly more likely to buy insurance, and argue that receipt of subsidies is a valid proxy for trust in government programs.

Giné and Yang (2009) studied the effect of bundling in-kind loans of fertilizer and high yield maize and groundnut seeds to farmers in Malawi with a rainfall insurance product. After measuring an unexpectedly negative impact of being offered the bundled contract on credit uptake, Giné and Yang separately examined factors affecting credit demand for the subsamples offered the bundled and unbundled loans. Acceptance of the bundled loan was positively correlated with education levels, whereas this was not the case for the unbundled contract, suggesting that the cognitive burden of evaluating the complex insurance contract depressed demand.

The literature on demand for index insurance reinforces the conclusions of studies of participation in financial markets in the rich world: factors such as trust that might not be part of
a typical model of the program participation decision are important determinants of enrollment. In the next section, I model the decision to participate in an index insurance program, first assuming that households are perfectly informed and fully trust the insurance company and then assuming that households believe the insurance company will systematically underreport shortfalls of the insured risk from the strike point.

3 A model of demand for index insurance and beliefs about the insured risk

3.1 Model structure: putting the problem in context
The model consists of farmers choosing between two activities: planting cotton on a single hectare of land, or planting a subsistence crop on the same single hectare. The subsistence crop guarantees a risk free return of \( w \). Cotton is a risky technology, as output in each period is vulnerable to a covariate shock, \( \epsilon^c \), that is common to all households, and a households-specific shock, \( \epsilon^s \). In this model, I define the common shock at time \( t \), \( \epsilon^c_t \), as the deviation of average output per hectare, or area-yields, from its mean:

\[
\epsilon^c_t = \mu - \bar{q}_t \tag{1}
\]

where \( \bar{q}_t \) is area-yields at time \( t \) and \( \mu \) is the mean of \( \bar{q}_t \). The price of cotton is fixed at unity, making output and revenue identical. The cotton yield of farmer \( i \) at time \( t \) is:

\[
q_{it} = \mu_i - \beta_i (\mu - \bar{q}_t) - \epsilon^s_i = \mu_i - \beta_i \epsilon^c_i - \epsilon^s_i \tag{2}
\]

This production function is adapted from Miranda (1991). It states that in each period a farmer planting cotton receives his or her mean yield, \( \mu_i \), net of any covariate or idiosyncratic shocks, i.e., \( \epsilon^c_i \) and \( \epsilon^s_i \), respectively. Note that the shocks can be harmful or beneficial, depending on the signs of \( \epsilon^c_i \) and \( \epsilon^s_i \). The shocks \( \epsilon^c_i \) and \( \epsilon^s_i \) are assumed to be continuous, jointly independent, and
symmetrically distributed about a mean of zero. Both terms are identically distributed across
farmers, although for the present purpose this need not be the case for $\varepsilon^c$.

The effect of the covariate shock is multiplied by $\beta$, a parameter whose value varies
across farmers. Thus while the magnitude of $\varepsilon_i^c$ is the same for all households in a given period $t$,
its impact will depend on the value of $\beta_i$ for a given farm. It is assumed that $\beta_i$ is weakly greater
than zero for all farmers. Taken together, the individual $\beta_i$ parameters form the population level
distribution of $\beta$, which has a mean of one, a variance of $\sigma_{\beta}^2$, and is assumed to be truncated to
the left of zero.

To make sense of the $\beta$ parameter, one could imagine that $\beta_i$ is related to physical
characteristics of the farm that would make it more or less vulnerable to covariate shocks. For
example, in an area where agriculture is dependent upon irrigation, $\beta_i$ might capture distance
from the farms to the primary irrigation canal. Farms located close to the canal would always
have sufficient water, and therefore might have low $\beta_i$ values. Farms located further from the
canal would be more sensitive to water availability, and have higher $\beta_i$ values as a result.\footnote{1}

For simplicity, it is assumed that the mean level of output $\mu_i$ is the same across
households, so that $\mu_i = \mu$ for all $i$. This is not a realistic assumption, but adding additional
heterogeneity would distract from $\beta_i$. Differences in $\beta_i$ across farmers will drive variation in the
potential benefits to insuring against the covariate shock $\varepsilon^c$, and this is the focus of the model.

\footnote{1 In the long run, the value of $\beta$ for each farmer would likely be a choice variable, as pointed out by Chambers and Quiggin (2002). I assume this parameter is fixed.}
From equation (2), the variance of yield for a single farmer can be written as:

$$\sigma^2_{q_i} = \beta_i^2 \sigma_c^2 + \sigma_s^2$$  \hspace{1cm} (3)

Yield variance is thus the sum of a component due to variation in area-yields, $\beta_i^2 \sigma_c^2$, and another due to all other sources of risk, $\sigma_s^2$. Differences in the variance of yield across farmers are driven by heterogeneity in sensitivity to the covariate shock as captured by $\beta_i$.

### 3.2 The planting decision without index insurance

A farmer’s planting decision will be driven by his ex-ante evaluation of the benefits of planting cotton versus those of the subsistence crop. Denote by $plant_i = 1$ the decision to grow cotton by farmer $i$, and $plant_i = 0$ planting the subsistence crop. The expected utility of planting cotton is:

$$EU_{plant_i = 1} = \mu^2 - \gamma \left( \beta_i^2 \sigma_c^2 + \sigma_s^2 \right)$$  \hspace{1cm} (4)

and for the subsistence crop:

$$EU_{plant_i = 0} = w^2$$  \hspace{1cm} (5)

This mean-variance expected utility function is taken from Nelson and Escalante (2004). Squaring the mean implies that farmer preferences are characterized by constant relative risk aversion, where the coefficient of relative risk aversion is given by $\gamma$.

In the absence of insurance, farmers will plant cotton if $EU_{plant_i = 1} - EU_{plant_i = 0} > 0$.

Assuming that $\mu > w$, differences in planting decisions across farmers will be determined by heterogeneity in $\beta_i$. Specifically, farmers with high values of $\beta_i$ will deem cotton production as too risky, as their output is highly sensitive to covariate risk. Setting the difference between (4) and (5) to zero and rearranging yields a critical value of $\beta_i$, beyond which farmers plant the subsistence crop:
\[ EU_{\text{plant}=1} - EU_{\text{plant}=0} > 0 \iff \beta_1 < \sqrt{\frac{\mu^2 - \gamma \sigma_c^2 - w^2}{\gamma \sigma_c^2}} \equiv \beta\] (6)

3.3 The area-yield insurance contract

Other things being equal, farmers would be more inclined to move out of subsistence crop production if the risk associated with growing cotton could be reduced, and any such shift would increase expected output in the economy. Introducing area-yield insurance (AYI) is one way this might be accomplished. I assume that the AYI contract has an indemnity function, \( I \), which takes the following form:

\[ I_t = \max[0, \mu - q_t] = \max[0, \varepsilon_t^c] \] (7)

The indemnity pays farmers the difference between the mean of area-yields, \( \mu \), and area-yields at time \( t, \bar{q} \), when this difference is positive, and nothing otherwise. To purchase AYI, farmers must pay the premium, \( \tau \), which is equal to the expected indemnity, \( r \), plus a “loading” term, \( L \):

\[ \tau = E(I) + L = E(\varepsilon | \varepsilon > 0)P(\varepsilon > 0) + L = r + L \] (8)

Since it includes a loading term, the AYI contract is not actuarially fair.²

The expected utility of insurance purchasers will be affected through the expected value of the indemnity, \( r \), as well as the variance of the indemnity, \( \sigma_r^2 \), and its covariance with the common shock, \( \sigma_{\varepsilon, I} \). The variance of the indemnity is:

\[ \sigma_r^2 = E(\varepsilon^2 | \varepsilon > 0)P(\varepsilon > 0) + r^2 \] (9)

² “Loading” is what is added by insurance companies to cover costs of offering insurance in addition to making indemnity payments.
The covariance of the indemnity with $\varepsilon^c$ is:

$$\sigma^2_{c,j} = \left[ (\sigma^2_c | \varepsilon^c > 0) + E(\varepsilon^c | \varepsilon^c > 0) \right] P(\varepsilon^c > 0)$$ (10)

Equations (9) and (10) are derived in the appendix. Along with the value of $\beta_j$, the covariance term determines the risk reduction potential of AYI for each individual farmer. The greater the covariance, the greater is the risk reduction due to purchasing AYI.

### 3.4 The decision to buy area-yield insurance and its effect on technology choice

Letting $AYI_i = 1$ denote the decision to buy AYI and $AYI_i = 0$ opting not to do so, expected utility conditional on planting cotton and buying AYI is:

$$EU_{\text{plant}_i=1, AYI_i=1} = (\mu - L)^2 - \gamma(\beta_i^2 \sigma^2_c + \sigma^2_i)$$ (11)

Taking the difference between the expected utility of cotton with AYI and the expected utility of uninsured cotton yields an additional critical value on $\beta$:

$$EU_{\text{plant}_i=1, AYI_i=1} > EU_{\text{plant}_i=1, AYI_i=0} \iff \beta_i > \frac{(2\mu - L) + \gamma\sigma^2_i}{2\gamma\sigma^2_c \sigma^2_j} \equiv \beta^*_i$$ (12)

Conditional on planting cotton, a farmer will purchase AYI if his $\beta_i$ value is greater than or equal to $\beta^*_i$. A smaller value of $\beta_i$ would imply that yields are not sufficiently sensitive to the common shock to make purchasing AYI worthwhile.

Introducing AYI into the economy may induce some farmers to switch from planting the subsistence crop to farming cotton with insurance. Comparing the difference between the expected utility of cotton with AYI and the subsistence crop give us two new critical values on $\beta_i$: 
\[ EU_{\text{plant}_i=1, \text{AYI}_i=1} - EU_{\text{plant}_i=0} > 0 \iff \beta_2 \equiv \frac{\sigma_{c,t} - x}{\sigma_c^2} < \beta_1 \equiv \frac{\sigma_{c,t} + x}{\sigma_c^2} \equiv \beta_3 \] (13)

where \( x = \sqrt{\gamma \left( \sigma_{c,i} \right)^2 + \sigma_c^2 \left[ \left( \mu - L \right)^2 - \nu^2 - \gamma \left( \sigma_x^2 + \sigma_i^2 \right) \right]} \)

Purchasing AYI results in disutility from the variance of the indemnity, \( \sigma_i^2 \), and the decrease in expected returns caused by \( L \). In order to switch from the subsistence crop to cotton with AYI, farmers must have values of \( \beta_i \) at least as large as \( \beta_2 \), as this will guarantee a sufficient gain in expected utility via risk reduction to offset these other changes. Values of \( \beta_i \) greater than \( \beta_2 \) result in sensitivity to the covariate risk that is too large to be offset through the purchase AYI; farmers with very high values of the \( \beta_i \) parameter will therefore continue to plant the subsistence crop.

These relationships between the insurance decision, technology adoption, and the value of \( \beta_i \) for each farmer are depicted in Figure 1, which graphs the expected utility of the options discussed above as function of \( \beta_i \) under three different values of the loading parameter, \( L \):
The curves in Figure 1 are meant to represent the general case and are not based on any particular parameter values. Prior to the introduction of AYI, farmers choosing the subsistence crop will have expected utility located somewhere on the horizontal line segment labeled $EU_{plant_i=0}$, whereas those planting cotton have expected utility located on the curve $EU_{plant_i=1, AYI_i=1}$. Farmers with values of $\beta_i$ to the left of where $EU_{plant_i=0}$ and $EU_{plant_i=1, AYI_i=1}$ intersect (i.e., to the left of $\beta_0$) plant cotton prior to the introduction of insurance.

When index insurance is introduced with a loading factor of $L_0$, farmers with values of $\beta_i$ between $\beta_0$ and $\beta_i(L_0)$ purchase insurance and switch from the subsistence crop to planting cotton. Farmers with $\beta_i$ values between $\beta_i(L_0)$ and $\beta_0$ switch from uninsured to insured cotton,
while those to the left of $\beta_1(L_0)$ continue to plant cotton without buying AYI; it is the former group that drives the impact of AYI on technology adoption, as they are the group that changes behavior following the introduction of insurance. The value of $\beta_i$ where the expected utility of insured cotton is maximized is denoted by $\beta^*$. Farmers with $\beta_i$ values to the right of $\beta_3(L_0)$ continue to use the subsistence technology.

Increasing the loading factor to $L_1$ shifts the vertical intercept of $EU_{plant_i=1,AYI_i=1}$ downwards, and insurance purchasers now have $\beta_i$ values between $\beta_1(L_1)$ and $\beta_3(L_1)$; note that since the expected utility of uninsured cotton and the subsistence technology both intersect $EU_{plant_i=1,AYI_i=1}$ at the same point, $\beta_1(L_1)$ is equal to $\beta_2(L_1)$. The results of this shift are twofold: first, the proportion of farmers electing to purchase insurance decreases, since the expected utility of insured cotton has fallen relative to the other two options, and the group of insurance purchasers is now entirely comprised of farmers who are induced to switch from the safe technology to cotton farming. The average impact of purchasing index insurance on the technology choice variable $plant_i$ is a shift from 0 to 1. An additional sharp increase in the loading factor to $L_2$ moves the expected utility of insured cotton down to $EU_{plant_i=1,AYI_i=1}(L_2)$. In this last case, the expected utility of insured cotton is everywhere below that of the other two options, and no one purchases AYI.

Stated more formally, farmers purchasing AYI must have values of $\beta_i$ satisfying:

$$AYI_i = 1 \leftrightarrow \beta_3 \text{ exists and } \max[\beta_1, \beta_2] < \beta_i < \beta_3$$

(14)
The curve $EU_{plant_{1},AYI_{1}}(L_2)$ in Figure 1 corresponds to the case in which $\beta_2$ and $\beta_3$ do not exist because the expected utility of cotton with AYI is everywhere below that of the subsistence crop, while $\beta_2$ does not exist when loading is set equal to $L_0$ because of the truncation of the $\beta$ distribution. The $\beta_i$ values of farmers planting cotton following the introduction of AYI must obey:

$$\beta_i < \beta_0 \text{ or } \max[\beta_1, \beta_2] < \beta_i < \beta_3$$ (15)

The proportion of farmers falling within these different bounds on $\beta_i$ will depend on the distribution of $\beta$ within the population. Figure 2 below depicts this distribution and the critical values of $\beta_i$ for the case of loading equal $L_0$ depicted in Figure 1; i.e., non-zero demand for AYI, with a mix of farmers who always plant cotton and farmers induced to adopt cotton by AYI electing to buy index insurance:
Figure 2 depicts $\beta$ as a truncated normally distributed random variable with a mean of 1 and a variance of 0.25. Note that $\beta_2$ is negative in this case, and as a result is not depicted. Farmers planting cotton prior to the introduction of AYI have $\beta_i$ values to the left of $\beta_0$. Once AYI is introduced, farmers with $\beta_i$ values between $\beta_0$ and $\beta_3$ are induced to switch to cotton. Insurance purchasers for whom technology adoption is unaffected have $\beta_i$ values between $\beta_1$ and $\beta_0$. Given the positions of the different critical values of $\beta_i$, the spread of the $\beta$ distribution, $\sigma_\beta^2$, will determine the proportion of farmers who purchase AYI and the share of farmers that are induced to switch to cotton farming following the introduction of index insurance.
3.5 Lack of trust in the insurer and demand for area-yield insurance

If farmers know the different mean, variance, and covariance terms detailed above, then any farmer who is better off with AYI will purchase it. Suppose, however, that farmers believe the insurance company will “cheat” them in the sense that it will always underreport the size of the shortfall of area-yields from $\mu$ by some fixed proportion of $\mu$, which we will label $g$. It is assumed for simplicity that $g$ is identical across all farmers. For example, the insurance company would likely carry out a survey of yields in the region where the AYI product is sold at the end of each harvest, in order to determine whether a payout has been triggered. Rather than taking them at their word, farmers believe that the insurer always adds $g\mu$ to the measured area-yields; this is the most the insurers are believed to be able to “get away with” without drawing attention from regulatory bodies. It is easy to think of similar examples in the context of other types of index insurance, e.g., farmers might believe an insurer would manipulate the rain gauge so as to avoid paying out claims for an index product based on rainfall.

The result of this perception is that farmers will no longer view the indemnity function as being based on the distribution of $\overline{q}_i$, but on $\tilde{q}_i$, where the latter is equal to:

$$
\tilde{q}_i = q_i + g\mu 
$$

Farmer perceptions, whether correct or not, cause a location shift in the perceived distribution of area-yields. The expected value of this subjective distribution of area-yields is:

$$
E[\tilde{q}_i] = E[q_i + g\mu] = \tilde{\mu} 
$$

Note that this change in perception does not alter the distribution of the common shock, $\varepsilon^c$:

$$
E[\tilde{\varepsilon}^c] = E[\tilde{\mu} - \tilde{q}_i] = 0 
$$

and
\[ E\left[\left(\hat{e}_c^e\right)^2\right] = E\left[\left((\mu + g\mu) - (\tilde{q}_i + g\mu)\right)^2\right] = \sigma_c^2 \]  

(19)

Since the common shock under the perception of a dishonest insurer is equal to the shock under the scenario of no perceived cheating, we can use the latter going forward rather than having to work with a new distribution.

What the perception of cheating does do is alter the perceived definition of the indemnity function as viewed by farmers. The subjective indemnity function is:

\[ \tilde{I}_t = \max\left[0, e_t^c - g\mu\right] \]

(20)

The strike point under this new indemnity function is closer to the right tail of the \( e_c^c \) distribution than under the true distribution, lowering the subjective probability of receiving an indemnity payout. This in turn leads farmers to perceive a larger difference between the premium and the expected indemnity; in effect, farmers view the contract as having a higher loading term.

The impacts of the perception of cheating are summarized graphically in Figure 3:
Figure 3: Perception of cheating, area-yields, and the indemnity.

Panel (a) depicts the perceived location shift of area-yields caused by the perception among farmers that the insurer systematically exaggerates area-yields in each time period by an amount \( g \mu \). Panel (b) shows the effect on the indemnity. Both \( I \) and \( \tilde{I} \) are evaluated over the distribution of \( \varepsilon^c \), but the latter is shifted downward by the amount \( g \mu \) at every level of the common shock that generates a payout.

While the impact of \( g \) on the expected returns to the AYI contract is clear, the perception of cheating does not unequivocally reduce the risk-reduction potential of AYI. As shown in the appendix, this ambiguity stems from the fact that while the higher strike point reduces the covariance of the indemnity and the covariate shock, it may also reduce the variance of indemnity. The net effect of the decrease in expected returns and the change in total variance due to purchasing AYI given the subjective mean of area-yields cannot be signed without choosing...
explicit parameter values. To clear up this ambiguity and set the stage for linking the analytical model to the discussion of econometric methodology, this model is simulated in the following section using data on cotton farming in the southern coast of Peru. The results support the intuition: a higher $g$ has a strongly negative effect on demand for AYI.

### 3.6 Assigning parameter values to the model

The model is parameterized using data from the Pisco Valley in Peru, which has been the site of an AYI pilot project for the past several years.\(^3\) The Pisco Valley is home to around 5,000 farmers, approximately half to three quarters of which farm cotton in any given year with maize and beans comprising the other major crops. Farms are small, with an average size of around 5 hectares, and about 20 percent of farmers have access to credit from formal financial institutions. Land is productive in Pisco, but agriculture is subject to idiosyncratic risk in the form of localized flooding from breakdown of irrigation infrastructure, and systemic shocks from El Niño, which can cause flooding and pest outbreaks that can devastate the usually robust cotton plant.

Focus group discussions with farmers in Pisco suggest a strong disconnect between the distribution of area-yields as estimated using data from the Ministry of Agriculture in Peru, which were used to price the contract, and farmer perceptions of area-yields. The causes of this dissonance between the data and farmer beliefs are more heterogeneous than what has been modeled in this essay. For example, some farmers have a low opinion of the Ministry of Agriculture and thus may see the data as unreliable, while others (particularly highly productive

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\(^3\) For additional details on this project, see Boucher and Mullally (2010).
farmers) have a difficult time believing that area-yields will ever drop below the strike point. But in one sense the consequences are the same, as farmers see AYI as too expensive given the likelihood of the insurer reporting a level of $\bar{q}_i$ that would trigger a substantial payout.

Parameters values used in the simulations are given in Table 1 below:

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</tbody>
</table>

The values of $\mu$ and $\sigma^2_c$ were taken from a 20 year time series of cotton yields in the Pisco Valley, provided by the Ministry of Agriculture. The data were de-trended using a linear model. A normal distribution was fit to the yield data using the estimates for $\mu$ and $\sigma^2_c$ and then truncated at zero. Parameters related to the AYI contract, $r$, $\sigma^2_i$, $\sigma^2_{c,i}$, and $L$, were generated using the truncated normal distribution of area-yields while setting the strike point equal to zero (i.e., payouts replace 100 percent of shortfalls from $\mu$) and loading $L$ equal to 15 percent of the expected indemnity. In lieu of a time series of yields from agricultural households, the idiosyncratic risk parameter, $\sigma^2_s$, was set equal to the variance of the covariate shock, $\sigma^2_c$. The remaining parameters $w$ and $\gamma$ were chosen to generate a proportion of participation in cotton farming similar to what is usually observed in Pisco.

3.7 Simulation results
Simulation results depicting how perceived cheating by the insurer affects AYI demand and technology adoption are shown in Figure 4.
Along the horizontal axis is \( g \), corresponding to farmer’s beliefs about the percent by which insurers overstate area-yields as a proportion of \( \mu \). The middle dashed line, \( \beta_0 \), shows the critical value of \( \beta_i \) below which farmer’s plant cotton prior to the introduction of AYI. The uppermost dashed line depicts \( \beta_3 \), gives the new upper bound on \( \beta_i \) for cotton planters once AYI is available and the lower dashed line is the minimum level of \( \beta_i \) for cotton planters, \( \beta_1 \). For a given value of \( g \), farmers with values of \( \beta_i \) between \( \beta_0 \) and \( \beta_3 \) are induced to switch technologies from the safe activity to cotton farming, while those between \( \beta_1 \) and \( \beta_0 \) plant cotton regardless. The effect of increasing \( g \) is to narrow the range of \( \beta_i \) values falling between both sets of bounds, thus lowering overall demand for AYI and the proportion of farmers who are induced to plant cotton by
purchasing insurance. Beyond a value of $g$ equal to about 7 percent of $\mu$, demand for AYI falls to zero.

### 3.8 Model implications
If the insurer is indeed cheating the farmers who purchase AYI in this model economy, then farmers who are dissuaded from buying it under this scenario but would do so if the insurer were honest are behaving optimally. But as described in Section 2, the literature on index insurance and other areas of finance shows that variation in the degree of trust in presumably honest institutions can have a strong effect on demand. This variation is all about perception and may have little to do with the true benefits a household can expect to receive from participating in index insurance or any other financial market innovation.

At the same time, little is known empirically about the nature and magnitude of the impacts of many different financial innovations in developing countries. The ideal scenario would therefore appear to be to offer farmers the incentives necessary to experiment with new financial market interventions, allowing them to learn for themselves the benefits of these programs, and do so in a way that allows for the measurement of impacts, i.e., the incentive must not directly affect outcomes of interest. Whether households can be convinced to try new programs is not a foregone conclusion. However, the studies summarized in Section 2 have had some success identifying variables that can significantly raise demand for index insurance. It therefore does appear possibly to raise uptake even in programs that have been strongly affected by low participation rates.

Introducing extra incentives for participation will alter the program effect that is ultimately estimated, leaving researchers with difficult choices. Assume that households must make a binary program participation decision. If program impacts vary across households, and
the incentive shifts program participation from some positive percentage of households under no extra incentive to a higher proportion, then the resulting estimate will be a Local Average Treatment Effect (LATE). The LATE will capture the average impact of the program on those that were induced to enroll by the extra incentive (Imbens and Angrist 1994).

4 Increasing demand for index insurance in the context of program evaluation

4.1 Tying the model to econometrics
Suppose we would like to estimate the average impact of AYI on technology adoption among insurance purchasers in the economy described in Section 3, and assume that the individual $\beta_i$ parameters are not observed. In other words, $\beta_i$ is an unobserved source of heterogeneity across farmers that will affect both the decision to participate in index insurance and whether to engage in the risky activity of cotton farming. A simple comparison of average outcomes among AYI purchasers and non-purchasers will not yield an unbiased estimate of average program impacts on participants, because as the theory from the previous sections demonstrated, the distribution of $\beta$ will be systematically different across the two groups. Estimation of impacts will require an additional, observed source of variation in AYI demand that is uncorrelated with technology adoption.

4.2 A randomization of eligibility and a randomized encouragement design
One method that can yield unbiased estimates of average treatment effects is directly randomizing individuals into and out of the index insurance program. This approach may be problematic, given individual cannot usually be compelled to participate in an insurance program. Alternatively, one could randomize a variable that affects demand for index insurance without affecting the variable to be used as the outcome. Comparisons of the groups formed by this randomization can yield unbiased estimates of average treatment effects. However, the
average treatment effect that is estimated may vary depending on the randomization strategy used. This is because the randomization strategy chosen will affect the composition of the comparison groups. Unless impacts are the same for all individuals or vary randomly in a way that is not correlated with the decision to buy insurance, the average outcome in each group will depend on group composition (Heckman and Vytlacil, 2007).

For example, consider a randomization of eligibility to purchase index insurance. Conceptually, this is perhaps the simplest randomization of this sort, although it may be one of the most difficult to implement in practice; for example, it might require convincing a private insurer to deny coverage to potential clients. At the individual level, randomizing eligibility would require some sort of mechanism by which randomly chosen persons are not allowed to purchase index insurance, while others are left to purchase it as they please. This is what was done by Giné and Yang (2009) in their study of demand for loans bundled with rainfall insurance in Malawi.

Denote by \( z_i = 1 \) random assignment of farmer \( i \) to the eligible group, i.e., those who can purchase AYI at the market price \( \tau = (r + L) \), and \( z_i = 0 \) random assignment to the ineligible group. Suppose that a proportion \( \rho_{z_i=1} \) of the eligible group buys index insurance. Using technology choice \( plant_i \) as the outcome, the following average treatment effect could be estimated, using data on technology adoption and demand for insurance among the eligible and ineligible groups:

\[
\frac{P(plant_i = 1|z_i = 1) - P(plant_i = 1|z_i = 0)}{P(AYI_i = 1|z_i = 1) - P(AYI_i = 1|z_i = 0)} = P_{z_i=1} - P_{z_i=0} \\
\frac{P(plant_i = 1|z_i = 1) - P(plant_i = 1|z_i = 0)}{P(AYI_i = 1|z_i = 1)} = \frac{P_{z_i=1} - P_{z_i=0}}{\rho_{z_i=1}} \tag{21}
\]
\[ P(AYI_i = 1 \mid z_i = 1) = P_{z_i = 1} \]

is the proportion of eligible farmers planting cotton, also equal to the expected value of \( plant \) in the AYI-eligible subpopulation.

Exploiting the fact that \( z_i \) is randomly assigned and that it does not directly affect the outcome \( plant \), it can be shown that the expression given in (21) is equivalent to:

\[
P\left( plant_{i,AYI_i = 1} = 1 \mid AYI_i = 1 \right) - P\left( plant_{i,AYI_i = 0} = 1 \mid AYI_i = 1 \right) = \\
1 - P\left( plant_{i,AYI_i = 0} = 1 \mid AYI_i = 1 \right)
\]  

(22)

Note that the potential outcomes \( plant_{i,AYI_i = 1} \) and \( plant_{i,AYI_i = 0} \) are used in (22), i.e., what farmer \( i \) would do if he were to purchase AYI, rather than the observed outcome \( plant_i \). Correspondingly, \( P\left( plant_{i,AYI_i = 1} = 1 \mid AYI_i = 1 \right) \) is the proportion of farmers who would plant cotton if they were to buy index insurance, conditional on being among the farmers that elect to buy index insurance, and \( P\left( plant_{i,AYI_i = 0} = 1 \mid AYI_i = 1 \right) \) is the proportion of farmers in this same group who would plant cotton without index insurance.

The expression in (22) answers the following question: What is the causal effect of purchasing index insurance at the price \( \tau \) on the cotton adoption rate among insurance purchasers? In the vocabulary of program evaluation, this is an example of the “Average Treatment on the Treated,” or ATT. This particular average treatment effect may be of greatest relevance to policymaking, for two reasons. Firstly, it captures impacts on program participants, rather than some other sub-group. Secondly, one could also argue that the group of insurance purchasers under the randomization of eligibility ought to strongly resemble the group of farmers that will buy index insurance when it is made widely available, assuming insurance contract parameters will be the same, lending the effect estimated using the randomization of eligibility
some degree of external validity. For these reasons this average treatment effect will be referred to in what follows as the “Policy Relevant Treatment Effect,” or PRTE, using the terminology of Heckman and Vytlacil (2007).

Now consider a “randomized encouragement design” or allowing all households to purchase AYI but encouraging a randomly selected group to do so by offering them an extra incentive for enrollment. An example of a randomized encouragement design would be a voucher program that reduces the cost of participation in a social program. Here I will use a randomized encouragement design that gives a randomly chosen subset of farmers a discount “coupon” enabling each to pay a lower price for index insurance. Denote by $c_i = 1$ assignment of farmer $i$ to the encouraged group, and $c_i = 0$ if farmer $i$ is not picked to receive a coupon. Suppose further that a proportion $\rho_{c_i=1}$ of the coupon group participates, while a share $\rho_{c_i=0}$ of farmers without coupons buys index insurance. This randomized encouragement yields the following estimator:

$$
\frac{P(\text{plant}_{i} = 1|c_i = 1) - P(\text{plant}_{i} = 1|c_i = 0)}{P(\text{AYI}_{i} = 1|c_i = 1) - P(\text{AYI}_{i} = 1|c_i = 0)} = \frac{P_{c_i=1} - P_{c_i=0}}{\rho_{c_i=1} - \rho_{c_i=0}}
$$

This is a “Local Average Treatment Effect” (LATE). It can be shown that this expression is equivalent to:

\[ \text{...} \]

---

4 McKenzie (2009) offers some examples of encouragement designs in development.

5 For this to represent an LATE, the encouragement must satisfy the “monotonicity” assumption (Imbens and Angrist 1994). In the case of the coupon, this assumption would require that the lower premium either encourages or has no effect on the AYI purchase decision; it cannot persuade some farmers to buy insurance and dissuade others. One could imagine some encouragement designs where satisfaction of this assumption would not be obvious. For example, exposing farmers to information about sound risk management and the role of insurance as a risk management tool might boost insurance demand, but could also directly affect technology adoption.
\[ P\left( \text{plant}_{i, \text{AYI}_{i}=1} = 1 \mid \text{AYI}_{i} = 1 \leftrightarrow c_{i} = 1 \right) - P\left( \text{plant}_{i, \text{AYI}_{i}=0} = 1 \mid \text{AYI}_{i} = 1 \leftrightarrow c_{i} = 1 \right) \]  

(24)

where the expression \( \text{AYI}_{i} = 1 \leftrightarrow c_{i} = 1 \) should be read as “buys area-yield insurance if and only if given a coupon.” In other words, equation (24) is the change in the cotton adoption rate due to having AYI among the group of farmers that would purchase AYI if they were to receive a coupon, but would otherwise not purchase it. This group is known as the “compliers” in the program evaluation literature.

4.3 Choosing between alternative research designs: Mean Squared Error

The fact that the LATE only captures average impacts on individuals induced to participate by the encouragement is a limitation; if individuals are heterogeneous with respect to the potential risk reduction offered by AYI, then each different possible encouragement could yield a new LATE, making interpretation of these effects difficult (Heckman and Vytlacil 2007). A corollary to this is that the effect estimated by the randomized encouragement design and that captured using the randomization of eligibility will not in general coincide. A randomized encouragement design can generate an unbiased estimate of the average impact of AYI on the group whose insurance purchase decision is determined by the encouragement, but it is a biased estimate of the PRTE when individuals heterogeneous in ways that affect program enrollment and benefits.

Bias should not be the only consideration when weighing alternative research designs, however. Changing the pool of participants via a randomized encouragement will allow households to update information with respect to the gains from participation. It seems reasonable to assume that the most effective means of allowing households to decide if they can benefit from financial market interventions is to let them experiment for themselves, or learn from the experiences of others. Randomized encouragement designs, carried out over multiple
years, might accomplish this while enabling researchers to determine if the intervention has a beneficial effect on the subpopulation of compliers. This dynamic aspect of randomized encouragements is beyond the scope of this essay but represents a possible area of future research.

Another consideration is the Mean Squared Error (MSE) of the estimator based on a randomization of eligibility versus that of the estimator based on a randomized encouragement design. The MSE of an estimator $\hat{\theta}$ is equal to the square its bias plus its variance:

$$MSE(\hat{\theta}) = E(\hat{\theta} - \theta)^2 = \left[ Bias(\hat{\theta}) \right]^2 + Var(\hat{\theta})$$ (25)

As a model selection criterion MSE takes bias and variance into account. In the present context, bias is the difference between the average treatment effect and the true PRTE. The estimator generated by a randomized encouragement design will likely be a biased with respect to the PRTE. But low participation among eligible farmers who have not been given extra incentive to purchase AYI may cause estimates under a randomization of eligibility to be so imprecise that the randomized encouragement design yields the preferred estimator when judging each by its respective MSE.

Suppose we draw a sample of $n$ farmers, a proportion $\pi$ of which are assigned to the eligible group. The MSE for the estimate of the estimator based on randomized eligibility is:

$$\frac{(1-\pi)P_{z_i=1}(1-P_{z_i=1}) + \pi P_{z_i=0}(1-P_{z_i=0})}{n\pi(1-\pi)} \frac{1}{\rho_{z_i=1}^2}$$ (26)
Since the estimator based on the randomization of eligibility is unbiased with respect to the PRTE, the MSE given in (26) is equal to the variance of the estimator; the formula for the variance follows from the binary nature of $plant_i$.\(^6\) Low program participation affects the MSE through its direct effect on participation rates, and via its effect on the variance of the outcome $plant_i$ among eligible farmers. Consider the derivative of (26) with respect to $g$:

$$\left[ \frac{\partial P_{z_i=1}}{\partial g} (1-\pi) (1-2P_{z_i=1} \rho_{z_i=1}) \right] - \left[ \frac{\partial \rho_{z_i=1}}{\partial g} \frac{2}{n \pi (1-\pi) \rho_{z_i=1}^3} \right]$$

Assuming that demand for AYI falls as $g$ increases, the second term of this expression is always positive while the sign of the first term is ambiguous; the latter depends on the proportion of farmers eligible to purchase AYI that elect to plant cotton, i.e., $P_{z_i=1}$. This ambiguity makes it impossible to sign (27). What can be said is that the MSE of the estimator based on a randomization of eligibility is an increasing function of $g$ for all values of $P_{z_i=1}$ greater than 0.25.\(^7\)

Now consider the MSE for the estimator relying on a randomized encouragement design. This will be equal to the square of its bias (the difference between the LATE and the PRTE) and its variance. Note that the proportion of farmers without coupons in the encouragement design

---

\(^6\) Although I assume away the possibility here, the estimate of the PRTE based on the randomization of eligibility can exhibit finite sample bias if participation among eligible farmers is sufficiently low (Bound, Jaeger and Baker 1995).

\(^7\) The derivative is positive if the second term is larger in absolute value than the first. $\frac{\partial P_{z_i=1}}{\partial g}$ is weakly greater in absolute value than $\frac{\partial \rho_{z_i=1}}{\partial g}$; at most 100 percent of the cotton farmers who switch from purchasing AYI to not doing so due to an increase in $g$ also switch from cotton to the subsistence crop. Since $\rho_{z_i=1}$ is less than 1 and $\pi P_{z_i=0} (1-P_{z_i=0})$ is positive, $2P_{z_i=1} (1-P_{z_i=1})$ is greater than 1. Thus, $2P_{z_i=1} (1-P_{z_i=1})$ is sufficient for the second term to be greater than the first. This is true for all $P_{z_i=1}$ greater than 0.25.
who elect to buy AYI is equal in expectation to the share of eligible farmers who purchase AYI in the randomization of eligibility. As a result, the fraction of farmers planting cotton when not given a coupon in the encouragement design is equal in expectation to the proportion of eligible farmers planting cotton in the randomization of eligibility. Using these facts and rearranging terms, the MSE for the estimator based on the randomized encouragement design can be expressed as:

$$\left[ \rho_{z=1} \left( P_{c=1} - P_{z=0} \right) - \rho_{c=1} \left( P_{z=1} - P_{z=0} \right) \right]^2 + \rho_{z=1} \left( \rho_{c=1} - \rho_{z=1} \right) \left( 1 - \omega \right) P_{c=1} \left( 1 - P_{c=1} \right) + \omega P_{z=1} \left( 1 - P_{z=1} \right) \left( \frac{1}{n \omega (1 - \omega)} \left( \rho_{c=1} - \rho_{z=1} \right)^2 \right) \tag{28}$$

The $\omega$ parameter is the proportion of farmers in the sample assigned coupons. The first line is the square of (LATE - PRTE). Here we see that the bias grows as the difference in the proportion of coupon holders planting cotton increases relative to the proportion of AYI-eligible farmers planting cotton in the randomization of eligibility. This difference is driven by the impact of the incentive offered by the coupon on demand for AYI. The stronger the incentive offered by the encouragement design, the greater the change in the pool of insurance purchasers induced by the encouragement relative to the mix of farmers who would purchase AYI under a randomization of eligibility, and the bigger the absolute difference between the LATE and the PRTE.

The fact that the bias changes with the size of the coupon can be made explicit by examining the derivative of (LATE – PRTE) with respect to the size of the coupon:

$$\frac{\partial P_{c=1}}{\partial \text{coupon}} \frac{1}{\rho_{c=1} - \rho_{z=1}} - \frac{\partial \rho_{c=1}}{\partial \text{coupon}} \left( P_{c=1} - P_{c=0} \right) \tag{29}$$

This is equal to zero if:
The left-hand side is the marginal change in the share of farmers planting cotton due to a small change in the proportion of farmers with AYI. The right-hand side is the average change in cotton farming due to a change in AYI demand, where the average is evaluated over the range of AYI participation rates falling between \( \rho_{c_i} = 1 \) and \( \rho_{Z_i} = 1 \). The two sides of (30) will only coincide if the average impact of purchasing AYI on the outcome is constant over some range of participation rates in the insurance program. This will not be the case in general and the absolute value of the bias of the LATE as an estimator of the PRTE will grow as the value of the coupon increases.

The second term of the MSE given in (28) is the variance of the estimator based on the randomized encouragement design. The impact of the coupon on the variance of this estimator is the mirror image of the impact of \( g \) on the precision of the estimator based on a randomization of eligibility. The derivative of the second term in (28) with respect to the size of the coupon is:

\[
\frac{\partial P_{c_i=1}}{\partial \text{coupon}} - \frac{1}{n \omega (1-\omega) (\rho_{c_i=1} - \rho_{Z_i=1})^3} \left[ \omega (1-2P_{c_i=1})(\rho_{c_i=1} - \rho_{Z_i=1}) \right] - \frac{2}{n \omega (1-\omega) (\rho_{c_i=1} - \rho_{Z_i=1})^3} \left[ \omega P_{c_i=1} (1-P_{c_i=1}) + (1-\omega) P_{c_i=0} (1-P_{c_i=0}) \right]
\]

(31)

Using reasoning similar to that employed above with respect to the change in the variance of the estimator based on a randomization of eligibility, it can be shown that (31) is positive if \( P_{c_i=1} \) is greater than 0.25. A necessary and sufficient condition for this is that at least a quarter of farmers eligible to buy AYI but not given coupons elect to plant cotton, i.e., \( P_{c_i=0} = P_{Z_i=1} \geq 0.25 \), as farmers
with coupons will be at least as likely to purchase AYI and plant cotton as farmers paying the market price for index insurance. The effect of this greater precision on the MSE of the estimator will be tempered by an increase in bias.

Note that the ambiguity of the change in the MSE of each estimator with respect to varying the incentives for participation in the AYI program stems from the fact that the variance of \( \text{plant} \) depends on \( z_i \). This will always be the case with binary outcomes when the instrument has identifying power. If the variance of the outcome of interest did not depend on \( z_i \), which might be the case with many continuous outcome variables, the MSE of the estimator based on randomized eligibility would be strictly increasing with respect to \( g \) and the variance of the estimator based on the randomized encouragement design would always decrease with the size of the coupon; changes in precision would only occur through the \( \rho_{c=1} \) and \( \rho_{z=1} \) terms. The tradeoff between increased bias and greater precision captured by the MSE of the randomized encouragement design estimator would still be present, however. This suggests that in many applications, the bias-precision tradeoff offered by a randomized encouragement design would exist at all values of the outcome of interest.

5 A randomization of eligibility and a randomized encouragement design: simulating the tradeoffs

5.1 A randomization of eligibility

Using the parameter values given in Table 1, the PRTE and its 95 percent confidence interval as estimated using randomized eligibility are drawn as functions of \( g \) in Figure 5:
Figure 5: Magnitude and precision of the PRTE with perceived insurer cheating.

Figure 5 was drawn assuming a sample size of 1,000 farmers, half of which were assigned to the eligible group. Once \( g \) passes 6.7 percent of \( \mu \), the estimate of the PRTE becomes statistically insignificant. This is despite the fact that the PRTE is increasing monotonically with \( g \) until hitting a value of 1, the largest possible value it can take. The impact of \( g \) on precision of estimation limits what can be stated about the effects of AYI on technology adoption among insurance purchasers in this case.

The sharp increase in the confidence bounds around the PRTE reflects the fact that the impact of changes in participation rates on the precision of the estimator is nonlinear, and that as participation falls, the variance of the estimator increases at an increasing rate. This is in contrast to the effect of greater sample size. The inverse of the participation rate is squared in the formula
for the variance of the estimator given in (26), whereas the inverse of the sample size enters as a linear term.

The PRTE is increasing with respect to $g$ in Figure 5, but this is not true in general. The derivative of the estimator of the PRTE with respect to $g$ is:

$$
\frac{1}{\rho_{z=1}} \left[ \frac{\partial P_{z=1}}{\partial g} - \text{PRTE} \frac{\partial \rho_{z=1}}{\partial g} \right]
$$

While the second derivative in brackets will be greater than the first in absolute value, the PRTE is weakly less than one, and the expression cannot be signed. Increasing $g$ tightens the upper and lower bounds on $\beta_i$ for insurance purchasers as shown in Figure 4. Tightening the lower bound increases the PRTE, as this pushes farmers out of the insurance market who would plant cotton regardless of having purchased AYI, whereas tightening the upper bound has the opposite effect. The number of farmers of each type pushed away from buying insurance by a larger $g$ will depend on the position of these upper and lower bounds in the $\beta$ distribution, which in turn will be determined by the values of the parameters in the model.

### 5.2 A randomized encouragement design

Now consider a randomized coupon scheme. Figure 6 graphs the LATE as calculated using equation (23), its 95 percent confidence interval, and the difference between the PRTE and the LATE as a function of the size of the coupon. The cheating parameter $g$ is fixed at 6.7 percent of $\mu$.
When the coupon reaches a value of 4.55, or approximately 2.6 percent of the total premium of 176 (including loading \( L \)), the estimated LATE becomes statistically significant. In reality we would expect that a substantially larger coupon would be needed to drive up participation rates, but the mechanics would be similar to those depicted in Figure 6.

Although it is difficult to see, the LATE is decreasing monotonically with the size of the coupon. The mechanics behind this are the mirror image of the impact of the \( g \) on the magnitude of the PRTE. Recall that the LATE measures the impact of AYI on compliers, i.e., farmers who are induced to purchase AYI by receiving a coupon. Farmers who switch from not purchasing AYI to doing so by an increase in the size of the coupon all belong to the group of compliers at the new coupon value. If a larger coupon brings in relatively more farmers for whom technology
adoption is unaffected by having insurance, then the LATE will fall. This is what we observe in Figure 6.

Larger coupons also result in a greater difference between the LATE and the PRTE. When $g$ is at 6.7 percent of $\mu$, the PRTE is 1; increasing the size of $g$ has resulted in the scenario that parallels that of setting the loading factor equal to $L_i$ in Figure 1, i.e., the entire group of insurance purchasers if comprised of farmers induced to switch technologies from the safe crop to cotton. The LATE shrinks from 0.872 at a coupon of 1 to 0.621 when the coupon is equal to 10. The absolute bias of the LATE is initially very small, as nearly all additional farmers brought into the insurance market by the coupon only plant cotton when purchasing insurance, but increases quickly with larger coupons. In other words, increasing the value of the coupon changes the composition of the compliers to include fewer farmers whose technology adoption decision is affected by having insurance, and this composition effect grows with the size of the coupon.

5.3 *Simulating the Mean Squared Error*  
As was stated earlier, a more complete picture of how close one can expect the estimators generated by these two competing research designs to come to the truth is given by the MSE of each. These are depicted below as a function of coupon size in Figure 7:
Here $g$ has been fixed at 6 percent of $\mu$, rather than the 6.7 percent level used in Figure 6. The increase in the variance of the randomization of eligibility estimator was so sharp at $g = 0.067$ that comparing the MSE of the two estimators at $g = 0.06$ seemed more reasonable. The MSE of the estimator based on a randomization of eligibility does not change with the value of the coupon, and it is shown by the flat line located just below 0.05. At small coupon values, the impact of the encouragement on demand for AYI is very small, and as a result the MSE of the estimator for the encouragement design as depicted by the dashed line in Figure 7 is quite high. Once the coupon reaches a value of 8.35, the MSE of the encouragement design estimator drops below that of the randomization of eligibility estimator, and continues to fall until the coupon amount reaches 19.61; this is the size of the encouragement that minimizes the MSE in this case.
Beyond this coupon value, the MSE of the estimator based on the randomized encouragement begins to increase, as gains in precision diminish while bias with respect to the PRTE continues to grow.

When the MSE is used as the model choice criterion, a strong enough encouragement can make the randomized encouragement explored here preferable to a randomization of eligibility when participation is low. All of the above simulation results were generated by holding unobserved heterogeneity as represented by the spread of the $\beta$ distribution, $\sigma_\beta^2$, constant at 0.25. A question one might ask is to what extent the degree of unobserved heterogeneity present in the population will determine the potential of a randomized encouragement design to improve upon a randomization of eligibility with respect to MSE. Under the scenario presented here, the implications of greater unobserved heterogeneity for the relative precision of estimators based on these two research designs are straightforward; a larger $\sigma_\beta^2$ will mean that the expansion of the critical values on $\beta_i$ for insurance purchasers will capture less mass of the $\beta$ distribution. If participation is very low under the randomization of eligibility, then the encouragement design may still always be more precise. But it will take a stronger encouragement to generate statistically significant estimates of average treatment effects. This conclusion is, however, dependent upon the positions of the different critical values on $\beta_i$. If all were to to one side of the mean of $\beta$, for example, then a greater spread of $\beta$ could increase the mass held between the critical values of $\beta_i$ for insurance purchasers. Participation in index insurance would be higher under both randomization strategies.

The impact of greater unobserved heterogeneity on the difference between the estimated PRTE and the estimated LATE are less obvious. I simulate the effect of an increase in $\sigma_\beta^2$ on the
MSE of each estimator in Figure 8 below, and then use the assumptions made about the distribution of $\beta$ to gain some insight into the changes that are observed.

![Figure 8: MSE and its components as functions of $\sigma_\beta^2$.](image)

Figure 8 was generated by varying $\sigma_\beta^2$ while holding $g$ fixed at 0.06, the value of the coupon at 8.35 (the value that drove the MSE of the encouragement design estimator below that of the randomization of eligibility estimator when $\sigma_\beta^2 = 0.25$), and all other parameters fixed at their levels given in . As shown in the left panel, the MSE of the estimator based on a randomization of eligibility increases steadily while that of the randomized encouragement design is minimized at $\sigma_\beta^2 = 0.09$. While the variance of each estimator grows as the spread of the $\beta$ distribution
increases, the change in the MSE of the randomized encouragement design estimator is offset by a reduction in the square of its bias.

The mechanics behind why the square of the bias is decreasing can be gleaned by examining the panels of Figure 9:
\[
\text{PRTE} = \frac{B}{A + B}
\]
\[
\text{LATE} = \frac{D}{C + D}
\]

Figure 9: Change in LATE, PRTE and $\sigma^2_{\beta}$. 
Panel (a) shows the areas under the curve of the density function of $\beta$ that comprise the different average treatment effects. Farmers who purchase index insurance at the market price when eligible have values of $\beta_i$ between $\beta_1$ and $\beta_3$, while insurance purchasers induced to plant cotton are between $\beta_0$ and $\beta_3$. The PRTE is equal to the ratio of the proportion of farmers falling into the latter group to the proportion who are part of the former group, or $B/(A+B)$ in Panel (a). The critical values on $\beta_i$ for insurance purchasers are initially quite narrow due to the cheating parameter $g$, but are pushed out to $\beta'_1$ and $\beta'_3$ by the encouragement. The LATE is given by $D/[C+D]$, which is the proportion of compliers who are induced to plant cotton by having insurance.

Panel (b) shows the distribution of $\beta$ under different values of the spread of the distribution; the density with the highest peak corresponds to the distribution in Panel (a). In general, increasing the value of $\sigma^2_{\beta}$ decreases the areas contained by A, B, C, and D, respectively. The one exception is D when the spread of $\beta$ is very small, because of its location near the right hand tail; increasing $\sigma^2_{\beta}$ initially increases D before decreasing it. Areas holding more mass shrink more quickly, and as a result the labeled areas begin to resemble one another in size. In other words, the components of each average treatment effect are becoming more similar, and the difference in the LATE and the PRTE is diminished as a result.

This change is shown in Panel (c), which maps the change in each of the components of the LATE and PRTE against the spread of the $\beta$ distribution; the masses of the different areas approach one another before their slopes level off. The face that areas holding more mass diminish more quickly is shown in Panel (d), which graphs the change in each area against $\sigma_{\beta}$. 
For example, the numerator of the LATE given by area $C$ is initially larger than all other areas, but it also has the most negative slope with respect to $\sigma_p$. As a result, its mass begins to more closely resemble that of the numerator of the PRTE, $B$, as $\sigma_p$ increases. This is an interesting case in that it shows how a randomized encouragement design will not necessarily lead one further astray when the degree of unobserved heterogeneity in the population affecting program participation and outcomes is high. While the conclusions are specific to the model, it adds more support to the notion of randomized encouragement designs as an alternative to other strategies, or as a complement to them in the case of experiments with multiple arms.

5.4 Simulation implications
Several implications can be gleaned from the above simulation results. Firstly, larger coupons mean greater bias, and the bias-precision tradeoff involved with selecting the strength of the incentive offered by a randomized encouragement design cannot be avoided. However, bias does not necessarily translate into getting further away from the truth on average, as shown by the impact of larger coupons on the MSE in Figure 7. Randomized encouragement designs and other LATE estimators should not be dismissed out of hand because they are biased, particularly if the alternative is a research design that cannot produce much in the way of statistical precision.

Secondly, the effects of greater unobserved heterogeneity on the MSE of each estimator are context specific. Intuition might say that if a population has a great deal of underlying heterogeneity that affects both program participation and outcomes, any estimator that gets its identifying power by changing the pool of participants will be strongly biased relative to the true impact on participants. The example presented here showed that reality will often be more complex.
6 Conclusion
This paper explored the choice between two different research designs in the context of a program affected by low participation rates. As development economists focus more on evaluating complex interventions, low participation by households in the programs being analyzed has become more common, the result of which will often be estimates so imprecise that it is impossible to distinguish what works from what does not. While low participation could be the result of optimizing households making the decision that yields the greatest level of welfare, it is not at all clear that this is the case in this context. In the example of index insurance, properly evaluating the benefits of participation requires accurate knowledge of the insured risk and how fluctuations in this risk affect household welfare. This may necessitate a high level of sophistication by potential participants. It is unrealistic to expect this from poor households with little education interacting in markets with minimal history in the area being studied.

Against this backdrop, I compared the potential of a randomization of eligibility and a randomized encouragement design to serve as the basis for an impact evaluation of an index insurance product. A randomization of eligibility can yield unbiased estimates of the impact of a program on participants, which may be the effect of greatest interest from the perspective of both research and policymaking. A randomized encouragement design featuring a suitably strong incentive will improve upon the precision of the randomization of eligibility. By changing the pool of participants in the insurance program, the average treatment effect identified by a randomized encouragement will differ from that of the randomization of eligibility. The presence of bias does not necessarily mean that randomized encouragement designs should be ruled out, however. When equal weight is given to bias and precision when selecting between these two research designs, as is done by a comparison of Mean Squared Error, using a randomized
encouragement can indeed be preferable to a randomization of eligibility when low participation is an issue. Low participation in the absence of additional incentives is not sufficient reason to abandon the effort to measure program impacts.
Appendix: AYI contract parameters and comparative statics

**A1.1 Demand for AYI and the parameters of the AYI contract**

Recall that farmers who purchase AYI must have values of $\beta_i$ lying within the following interval:

$$\max[\beta_1, \beta_2] < \beta_i < \beta_3$$

(A.1)

or more explicitly:

$$\max \left[ \frac{(2\mu - l)I + \gamma \sigma^2_i}{2\gamma \sigma_{i,e}}, \frac{\sigma_{i,e} - x}{\sigma^2_{i,e}} \right] < \beta_i < \frac{\sigma_{i,e} + x}{\sigma^2_{i,e}}$$

(A.2)

Where $x = \sqrt{\gamma (\sigma_{c,i})^2 + \sigma^2_c \left[ (\mu - L)^2 - w^2 - \gamma (\sigma^2_c + \sigma^2_i) \right]}$. Assuming $x$ is positive, which is quite reasonable given that the squared $\sigma_{c,i}$ term will be extremely large, the derivative of the upper bound of (A.2) with respect to $\sigma_{c,i}$ is:

$$1 + \frac{\gamma \sigma_{c,i}}{\sigma^2_c} > 0$$

(A.3)

Depending on which lower bound is utilized, its derivative is either:

$$1 - \frac{\gamma \sigma_{c,i}}{\sigma^2_c} < 0$$

(A.4)

which has an indeterminate sign, or assuming $\mu > L$:

$$-\frac{-((2\mu - L)L + \gamma \sigma^2_i)}{2\gamma (\sigma_{c,i})^2} < 0$$

(A.5)
If the derivative is (A.4), the lower bound may be increasing, but at a rate slower than that of the upper bound; $\beta_3$ is always greater than $\beta_1$. Increasing the risk reduction potential of AYI via a higher value of $\sigma_{c,I}$ will weakly increase demand for insurance.

The derivative of the upper bound with respect to the variance of the indemnity, $\sigma_i^2$, is:

$$\frac{\gamma}{-2x} < 0 \quad (A.6)$$

The derivative of the lower bound with respect to $\sigma_i^2$ is then:

$$\frac{\gamma}{2x} > 0 \quad (A.7)$$

or

$$\frac{1}{2\sigma_{i.e}} > 0 \quad (A.8)$$

depending on which lower bound is used. The size interval is therefore decreasing with respect to $\sigma_i^2$.

Lastly, we have the loading term $L$ and its impact on the interval given in (A.2). The derivative of the upper bound with respect to $L$ is:

$$\frac{-(\mu - L)}{x} < 0 \quad (A.9)$$

While the derivative of the lower bound with respect to $L$ is:

$$\frac{(\mu - L)}{x} > 0 \quad (A.10)$$

or

$$\frac{\mu - L}{\gamma \sigma_{c,I}} > 0 \quad (A.11)$$
depending on which lower bound is used. The size of the interval is a decreasing function of the loading term, $L$.

### A1.2 Perceived cheating and the AYI contract

Suppose that farmers believe that the insurer exaggerates measured area-yields in every time period by an amount $g \mu$. Given $g$, the perceived covariate shock is $\tilde{\epsilon}^c = \tilde{\mu} - \tilde{\theta}$. The AYI contract that replaces shortfalls in area-yields from $\mu$ is perceived as:

$$\tilde{I}_i = \max \left[ 0, \tilde{\epsilon}^c - g \mu \right]$$  \hspace{1cm} (A.12)

while the true contract is:

$$I_i = \max \left[ 0, \epsilon^c \right]$$  \hspace{1cm} (A.13)

The variance of area-yields given $g$ is equall to that of true area-yields. Since the common shock is symmetric, this implies that the distributions of $\epsilon^c$ and $\tilde{\epsilon}^c$ are identical. When we evaluate the moments of $\tilde{\epsilon}^c$ as they pertain to the AYI contract, we can use the distribution of $\epsilon^c$ as long as we account for the fact that $g$ will leads farmers to view the strike point of the contract as higher than it actually is.

While it is proved in the main text that incorrectly perceiving the mean of area-yields leads farmers to view AYI as more costly, we cannot unequivocally say that it leads AYI to be viewed as less risk-reducing. The true change in the variance of output due to purchasing AYI is:

$$\sigma_i^2 - 2 \beta \sigma_{c,i}$$  \hspace{1cm} (A.14)

Replacing $I$ and $\epsilon^c$ with $\tilde{I}$ and $\tilde{\epsilon}^c$ yields the perceived change in variance. Consider the covariance between the indemnity and the covariate shock. The true value of this parameter is:
\[ \sigma_{c,t} = E \left( \max \left[ 0, \varepsilon_i^c - g \mu \right] \varepsilon^c \right) - E \left( \max \left[ 0, \varepsilon_i^c \right] \right) E \left( \varepsilon^c \right) = \]
\[ E \left[ \left( \varepsilon^c \right)^2 | \varepsilon^c > 0 \right] * P \left( \varepsilon^c > 0 \right) = \]
\[ \frac{\int_0^\pi \left( \varepsilon^c \right)^2 f \left( \varepsilon^c \right) d\varepsilon^c}{P \left( \varepsilon^c > 0 \right)} P \left( \varepsilon^c > 0 \right) = \]
\[ \int_0^\pi \left( \varepsilon^c \right)^2 f \left( \varepsilon^c \right) d\varepsilon^c \]  

(A.15)

Under the subjective distribution of area-yields, the covariance term is:

\[ \sigma_{c,t} = E \left( \max \left[ 0, \varepsilon_i^c - g \mu \right] \varepsilon^c \right) - E \left( \max \left[ 0, \varepsilon_i^c - \mu g \right] \right) E \left( \varepsilon^c \right) = \]
\[ \left( E \left[ \left( \varepsilon^c \right)^2 | \varepsilon^c > g \mu \right] - g \mu E \left[ \varepsilon^c | \varepsilon^c > g \mu \right] \right) P \left( \varepsilon^c > 0 \right) = \]
\[ \frac{\int_{g \mu}^\pi \left( \varepsilon^c \right)^2 f \left( \varepsilon^c \right) d\varepsilon^c}{P \left( \varepsilon^c > g \mu \right)} P \left( \varepsilon^c > g \mu \right) - g \mu \int_{g \mu}^\pi \varepsilon^c f \left( \varepsilon^c \right) d\varepsilon^c = \]
\[ \int_{g \mu}^\pi \left( \varepsilon^c \right)^2 f \left( \varepsilon^c \right) d\varepsilon^c - g \mu \int_{g \mu}^\pi \varepsilon^c f \left( \varepsilon^c \right) d\varepsilon^c \]  

(A.16)

The last line of (A.15) is larger than the last line of (A.16); i.e., the true covariance is larger than the covariance given the incorrectly perceived mean.

Ambiguity with respect to the risk reduction potential of AYI comes from the impact of \( g \) on the variance of the indemnity. The true variance of the indemnity function is equal to:
\[ \sigma_c^2 = E(I^2) - E(I)^2 = E(I^2) - r^2 = \]
\[ E \left[ \max \left[ 0, \epsilon_c^2 \right] \max \left[ 0, \epsilon_c^2 \right] \right] - r^2 = \]
\[ E \left[ (\epsilon^c)^2 \mid \epsilon^c > 0 \right] P(\epsilon^c > 0) - E \left[ \epsilon^c \mid \epsilon^c > 0 \right]^2 P(\epsilon^c > 0)^2 = \]
\[ \left[ (\sigma_c^2 \mid \epsilon^c > 0) + E \left( \epsilon^c \mid \epsilon^c > 0 \right)^2 \right] P(\epsilon^c > 0) - E \left( \epsilon^c \mid \epsilon^c > 0 \right)^2 P(\epsilon^c > 0)^2 = \]
\[ (\sigma_c^2 \mid \epsilon^c > 0) P(\epsilon^c > 0) + [1 - P(\epsilon^c > 0)] E \left( \epsilon^c \mid \epsilon^c > 0 \right)^2 P(\epsilon^c > 0) = \]
\[ (\sigma_c^2 \mid \epsilon^c > 0) P(\epsilon^c > 0) + \frac{P(\epsilon^c < 0) E \left( \epsilon^c \mid \epsilon^c > 0 \right)^2 P(\epsilon^c > 0)^2}{P(\epsilon^c > 0)} = \]
\[ (\sigma_c^2 \mid \epsilon^c > 0) P(\epsilon^c > 0) + E \left( \epsilon^c \mid \epsilon^c > 0 \right)^2 P(\epsilon^c > 0)^2 \]
\[ (\sigma_c^2 \mid \epsilon^c > 0) P(\epsilon^c > 0) + r^2 \]

The second to last line follows from the symmetry of \( \epsilon^c \), i.e., \( P(\epsilon^c > 0) = P(\epsilon^c < 0) \).

Using similar reasoning, it can be shown that given perceived cheating by the insurer of size \( g \mu \), the variance of the indemnity is:

\[ (\sigma_c^2 \mid \epsilon^c > g \mu) P(\epsilon^c > g \mu) + \frac{(\tilde{\tau})^2 P(\epsilon^c < g \mu)}{P(\epsilon^c > g \mu)} \]  \( \text{(A.18)} \)

Since \( P(\epsilon^c < g \mu) > P(\epsilon^c > g \mu) \), the second term in the right hand side of (A.18) may increase the variance of the indemnity, even though \( \tilde{\tau}^2 < \tau^2 \). However, raising the truncation point of a distribution decreases the truncated variance, and as a result,

\[ (\sigma_c^2 \mid \epsilon^c > 0) P(\epsilon^c > 0) > (\sigma_c^2 \mid \epsilon^c > g \mu) P(\epsilon^c > g \mu) \).

The perception of cheating by the insurer unequivocally lowers the covariance between the indemnity and the covariate shock, but it may also decrease the variance of the indemnity.
A1.3 Perceived cheating by the insurer and demand for AYI

The impact of perceived cheating $g$ on demand will depend on the derivatives of the bounds of the set of $\beta_i$ values given in (A.2) for which purchasing AYI is optimal with respect to $g$. The derivative of the first possible lower bound of this set with respect to $g$ is:

$$
\sigma_{c,i} \left( 2 \frac{\partial h}{\partial g} \left( \mu - (L + h) \right) + \gamma \frac{\partial \sigma_i^2}{\partial g} \right) - \frac{\partial \sigma_{c,i}}{\partial g} \left( \frac{1}{2} \left( 2 \mu - (L + h) \right) (L + h) + \gamma \sigma_i^2 \right) 
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