University of Alberta

In Kind and Cash Payments in Experiments: Farmer Valuation of Seeds with Decreased Variance in Orissa, India

by

Fiona Ila Hossack

A thesis submitted to the Faculty of Graduate Studies and Research
in partial fulfillment of the requirements for the degree of

Master of Science
in
Agricultural and Resource Economics

Department of Resource Economics and Environmental Sociology

Spring 2013
Edmonton, Alberta

Abstract

Evaluating the ex-ante willingness to pay (WTP) and adoption rates for pro-poor technologies is necessary to inform development interventions, the distribution and pricing of new technologies, and research and development. Unfortunately, a common practice of economic experiments that elicit value, paying participants in cash, is not always amenable to partner organizations in developing countries. Using a framed field experiment, an alternative in kind payment is explored and its effect on the valuation of yield stabilizing seed traits compared to that of cash. I find participants who are paid in kind with commonly bought household goods are willing to pay about 7% more than when they are paid in cash, suggesting that in kind remuneration may result in the overestimation of WTP. In addition, for a 6.9 INR reduction in the standard deviation, WTP was approximately 8% higher, suggesting that farmers value yield stabilizing seed traits like pest or drought resistance.

Acknowledgements

This dissertation would not have been possible without the contributions of a multitude of people, both acknowledged and unacknowledged, known and unknown; however, I will attempt here to recognize a few:

Dr. Henry An, my supervisor, for his guidance and infectious enthusiasm for economics and research. Thanks also for the contributions of Dr. Peter Boxall, Dr. Wiktor Adamowicz, Dr. James Rude, Dr. Sandeep Mohapatra, Dr. Brent Swallow, MSSRF Jeypore (in particular Seema Tigga and Chaudhury Shripati Mishra), MSSRF Chennai (in particular Nita Selena), as well as other MSSRF and REES staff.

Simrat Minhas, whose raw sex appeal brought me to school day after day despite my sometimes faltering motivation, and whose indefatigable friendship prevented me from dropping out, provided me with pictures of kittens, and took me out drinking, at all of the appropriate (and inappropriate) junctures to ensure my sanity.

Jeremy Lee Davis Taylor for his infallible support and the patient provision of an endless supply of snack items and tea. Thanks also to my other roommate, Bingley (Felis catus), for her constancy in cuddles and cute.

My parents (Margaret and Nick Hossack) for a lifetime of encouragement, particularly academically. My brother for his rambunctious approach to everything and unerring generosity of spirit.

Dr. Patti Laboucane-Benson for her role in getting me into graduate school and her infinite supply of nerdishness (which feeds my own).

My friends within the department for Royveloping and dividing our trousers by numbers we know (and then knowing).

Finally, my friends outside the department who learned not to ask when I would be graduating and who provided ample distraction and a plethora of encouragement, sympathy, or outrage as required.

Contents

Tables

Figures

1. Introduction

1.1 Motivation and Background

Poverty is often characterized by extreme vulnerability (Banerjee, 2004). According to the World Bank (2008), 75% of the world's poor[1] live rurally, and most depend on agriculture. Consequently, reducing agricultural vulnerability is critical in the effort to reduce poverty. For people practicing subsistence agriculture, stochastic crop yields are of principal concern, as losses from pests, drought, or floods can have a catastrophic impact on income, nutrition, food security, and well-being. The poor are not only less likely to receive the best information on rain and pest forecasts, they are also less likely to be able to afford either adjustment by replanting or preventative measures, such as pesticides and irrigation (Sinha and Lipton, 1999; Banerjee, 2004). In addition, they are less likely to have access to resources that could mitigate some of this risk, such as insurance or personal assets that can be sold (Sinha and Lipton, 1999; Banerjee, 2004). Together, these factors leave them acutely vulnerable.

Fortunately, recent developments in biotechnology and conventional breeding have attempted to mitigate some of this vulnerability through the creation and improvement of drought, flood, and pest resistant varieties of staple crops. Recent years have seen farmer trials and the distribution of varieties of disease resistant banana, disease and pest resistant sugar cane, disease resistant sweet potato, and

[1] Earning less than 1 USD a day.

drought and flood resistant rice (Wambugu, 1999; The Economist, 2003; Ornstein, 2009; Pandey, 2009).

Traits such as drought or pest resistance are considered yield stabilizing traits, because in years of low rainfall or high pest load they offer protection against total crop failure. It is thought that yield stabilizing traits are “pro-poor”, as the stability they offer should appeal to the most impoverished and vulnerable farmers who cannot afford to take risks. However, the traits are new technologies with uncertain adoption patterns. It is unclear whether farmers will purchase these seeds and whether, as may be required, they will pay a premium for the technology. The adoption of yield stabilizing seeds is particularly uncertain, because the benefit of such seeds is only realized in years with flood, drought, or infestation[2], making their valuation difficult for farmers to discern (Lybbert and Bell, 2010). In addition, farmers who pay a premium could lose money in years without stressors, contributing to the riskiness of adoption (Lybbert, 2006).

Given the uncertainty involved, researchers are keen to estimate ex-ante adoption rates and willingness to pay (WTP) for new varieties in order to estimate benefits and inform research and development decisions (see for example Zimmermann and Qaim, 2002; Lybbert, 2006; Kolady and Lesser, 2006; Moyo et al., 2007; Krishna and Qaim, 2008; Napasintuwong and Traxler, 2009; Labarta, 2009; Corrigan et al, 2009; Dalton et al., 2011). In addition, there is an increased interest from agribusiness in WTP techniques and studies, as they search for more

[2] In years of extreme drought or infestation, however, even seeds with resistance fail.

information on consumer demand for novel traits or varieties for which no markets currently exist (Lusk and Hudson, 2004). Some of these studies (Lybbert, 2006; Corrigan et al., 2009) use experimental economics, an increasingly popular approach to studying economic problems. Unfortunately, one common practice of economic experiments, the use of cash payments, has given rise to a potential conflict between researchers and local institutions involved in development research.

Generally, economic experiments pay participants in cash to mimic real incentives and because everyone values it equally[3] (Croson, 2005). However, it may not always be possible or agreeable to provide participants with cash payments. In developing countries, experimental work can be particularly sensitive, and particular attention needs to be paid to the existing relationships between local institutions and populations. These relationships may be based on reciprocity through gifts, or on partnerships in development, rather than on cash compensation. Under such conditions, the introduction of cash payments could be seen to set an unsustainable precedent for these organizations, could be offensive to local leaders, and could negatively impact the nature of existing relationships – all considerations that could make local research partners uncomfortable with cash payments. This was the position of our partner in India, the M.S. Swaminathan Research Foundation, and a review of the literature suggests that I was not the first to encounter such apprehension. Cook et al. (2011) were also asked by their local research partner not to use cash in their experiments

[3] Though different agents could have different marginal utilities of income.

measuring risk aversion in Kolkata, India[4]. While such concerns are valid, it does pose a problem for economic experiments: how should participants in experiments be remunerated when cash payment is not feasible?

Cook et al.'s (2011) solution was to pay participants in kind with a prize voucher system. Vouchers could be redeemed at a "store" offering popular household items (soap, spices, etc.) that the researchers set up in the back of the room in which the experiments were held. Unfortunately, their study was unable to establish what, if any, difference this method of payment may have had on participants' bidding behaviour. Theory suggests that while cash transfers increase the budget constraint and move the individual to a higher indifference curve, in kind transfers restrict consumption choices and may leave the individual at a lower indifference curve than a cash transfer of equal value (see Cunha (2011) for one application). If this is the case, participants' behaviour in experiments may well depend on the method of remuneration, possibly resulting in inaccurate estimates of WTP or adoption rates that could affect the pricing, marketing and distribution of new technologies.

1.2 Research Questions and Objectives

Using an experimental approach developed by Lybbert (2006), this paper attempts to determine (1) if farmers in an impoverished area of India are willing to pay more for yield stabilizing seed traits by assessing farmers' valuation of the

[4] The reason for the request is not specified in their paper.

variance of several experimental seed distributions; and (2) if paying participants in kind versus in cash leads to different bidding behaviour. By determining if there is a statistically significant difference between these two payment systems and assessing how closely they align, I hope to comment on the feasibility of in kind payment as incentives in economic experiments. I also hope to report an initial inflation (deflation) factor against which other researchers may compare their findings, with the ultimate goal of being able to correct for possible systematic over (under) bidding when participants' are paid in kind.

1.3 Outline

The rest of this paper will be organized as follows: Chapter 2 reviews relevant literature on cash and in kind payment, valuation, WTP, and field experiments. Chapter 3 explains the conceptual model. Chapter 4 provides a description of the experimental design, study site, and data. Chapter 5 describes some econometric considerations and the empirical approach. Chapter 6 presents and discusses the results, and Chapter 7 offers a summary and directions for future research.

2. Literature Review

This literature review is divided into three sections. The first examines empirical work comparing cash and in kind payments. The second discusses valuation and willingness to pay, including methodology and empirical work on the willingness to pay for technology in India and its determinants. The last section provides an overview of field experiments in development economics.

2.1 Comparing Cash and In Kind Payment

To the best of my knowledge, there are no studies that discuss the differences between cash and in kind payments in experiments. Probably the most relevant literature is that which compares cash and in kind transfers, and it is to this literature that I turn first. Following that is a discussion of Heyman and Ariely's theory of two markets (social and monetary). Lastly, work on the value of gifts is considered.

2.1.1 Empirical work comparing cash and in kind transfers

Empirically, there is little and mixed evidence on the differential effects of providing in kind or cash transfers. Early studies examine in kind food transfers in the form of food stamps. For instance, a study by Moffitt (1989) found that there was no discernible difference in food expenditures before and after the food stamps program in US Puerto Rico was converted to a cash transfer. He argues that the stamps were infra-marginal (the transfer amount is less than the original amount consumed) for most households, and, for those for whom it was extra-marginal (the transfer amount is more than the original amount consumed), there was resale. Hoynes and Schanzenbach (2009) find similar results for the introduction of food stamp programs in the US proper, suggesting that there is little evidence for the paternalistic argument for in kind transfers. Contradictorily, Fraker, Martini, and Ohls (1995) find that in four instances of cashing out food stamps in the US, food expenditure decreased. All of the aforementioned studies employ non-experimental data, taking advantage of changes in food stamp program policies, and the results may be affected by the unobservable characteristics of program participants as compared to non-participants. Whitmore (2002), on the other hand, uses a field experiment to examine the differences between food stamps and the cash equivalent and finds that nutrition levels and food expenditures are similar for those randomly assigned to receiving cash benefits.

Using data from a randomized field experiment on a cash and food transfer program in Mexico called PAL (*Programa Apoyo Alimentario* or Food Support Program**)**, Cunha (2011) found that the effect of in kind transfers of food was not significantly different than a cash transfer[5] of equal value after two years when measured by child health outcomes such as calorie intake, anthropometric measures, or incidences of illness. Increases in food consumption were not significantly different between the transfer types, but consumption of certain

[5] The cash transfers are meant to be conditional on attending nutritional education classes; however, in practice, they are unconditional (Cunha, 2011).

foods did differ, resulting in some distinction in micronutrient intake. However, this difference was not the result of households in the cash transfer group spending money on "vices" or making poor food choices; indeed, they spent most of the money on nutritious fruits and vegetables. Instead, the difference is attributed to the fortified milk supplied in the food baskets. Cunha argues that these results suggest that there is little evidence for paternalistic arguments for in kind transfers, especially considering the increased cost associated with administering in kind transfers.

Using the same data, Skoufias, Unar, and Gonzalez-Cossio (2008) found no significant difference in total consumption or reduction in poverty between the two transfer types either. They also found no difference between changes in labour supply between the two transfers; both transfers tended to shift labour supply from agricultural to non-agricultural activities but did not affect overall participation in the labour market.

Again from the PAL data, Skoufias, Unar, and Gonzalez-Cossio (2008) report[6] that a study by Gonzalez-Cossio et al. (2006) found that cash transfers had a larger effect on child height for age z-scores than the equivalently valued food transfers. This contradicts Cunha's finding that there was no significant difference between transfer types on the same measure for children of the same age; however, it is not mentioned in Skoufias, Unar, and Gonzalez-Cossio's (2008) summary whether the larger effect was statistically different.

[6] The original study is unavailable to this researcher.

There have also been other studies comparing conditional cash transfers to in kind transfers; however, Cunha (2011) points out that these studies have confounding interventions, such as school attendance (see Rawlings and Rubio (2005) for a review). The results of such studies are not reviewed here, as this study is concerned with unconditional cash transfers.

2.1.2 The theory of two markets: Social and monetary markets

The idea that the introduction of cash payments into an otherwise non-monetary relationship can change the nature of the interaction has some precedent in the idea of the "two markets". Heyman and Ariely (2004) posit that the form of compensation offered can in fact induce the receiver to frame the exchange as happening in one of two conceptually separate markets: the *monetary market* or the *social market*. They hypothesized that in the monetary market, effort is positively related to compensation level and focused on reciprocity, but in a social market, effort is expended altruistically and is not related to compensation level. As such, they also predicted that effort would be greater when no payment is offered, as offering even a low payment shifts the receiver from the social market, where effort is generally high, into the monetary market, where effort depends on the level of payment, which in this case is low.

They confirmed their hypotheses using a series of experiments. First, they asked students to rate how likely other students would be to help someone move a couch into a van if (1) there was no payment, (2) there was cash payment, either low or medium, (3) there was in kind compensation of candies, equivalent in value to the cash payment (low or medium), or (4) there was in kind compensation of candies, but their value was mentioned. The results showed that the expected willingness to help was higher for the no payment condition than for the low cash payment condition, but that there was no difference between the no payment condition and the low candy payment condition. They also confirmed that expected willingness to help increased with the level of cash compensation, but not with the level of candy compensation. When the value of the candies was mentioned, the participants seemed to view the transaction as occurring in the monetary market, and the results mimicked the cash payment condition. All of these results held when they offered students identical payment conditions for actual effort (either for dragging as many digital balls across a computer screen to specified locations as they could within three minutes or for time expended trying to solve an impossible math problem).

The theory of the two markets suggests that it may be possible to mimic the results of cash payment in an experiment even without paying cash simply by stating the monetary value of the in kind compensation. A caveat, of course, is that Heymen and Ariely's (2004) study examines situations in which payment is not contingent upon the participant's decisions in the experiment. In economic experiments, payment is often directly linked to decisions made within the experiment. This may result in no difference between in kind and monetized in kind payment (where the value of the goods is disclosed).

2.1.3 The value of gifts

The only other potentially relevant empirical work is that on the value of gifts. In a rather infamous article, Waldfogel (1993) tried to estimate the deadweight loss of Christmas. The argument for the potential deadweight loss of gifts is the same is that for cash versus in kind transfers: constricting bundles will leave some people worse off than they could have been with an equivalent amount of cash. Waldfogel estimated that 13% of what is spent is lost, though this figure has been criticized by many. An initial replication of Waldfogel's survey by Solnick and Hemenway (1996) resulted in an estimated 214% of actual value, though some of this discrepancy can be attributed to differences in the wording of the valuation question (Ruffle and Tykocinski, 2000). List and Shogren (1998) also criticize Waldfogel (1993) and Solnick and Hemenway's (1996) studies for being hypothetical and not using demand-revealing mechanisms, making the incentives for revealing true valuations very weak. This work also ignores the social and cultural significance of gift giving.[7]

2.2 Valuation and Willingness to Pay

2.2.1 Defining and eliciting value

Brown (1984) argues that the value of a good from a set of resources is determined by individuals' preferences rather than an intrinsic value, thereby varying across individuals and time. The economic value of a good can be

[7] Even though Waldfogel allows for the giver to receive welfare, the welfare from the increased social bond (assuming the gift is not offensive!) and that of practicing cultural rites is ignored.

measured either by the maximum amount someone is willing to pay (WTP) or the minimum they are willing to accept (WTA) in exchange for the good (Adamowicz, 1991). Typically, these measures are used to assign value to goods for which no well-developed markets exist, such as environmental amenities or new goods or attributes that do not yet exist in the marketplace. This second purpose, the valuation of new goods and attributes, is of particular interest to agribusiness and biotechnology research, as it can help to predict the demand and adoption of new technologies (Lusk and Hudson, 2004).

There are many approaches to eliciting value, beginning with the choice between WTP and WTA. Over the past few decades, a persistent disparity between these measures has been established: people require more to forego a good than they are willing to pay to obtain it (see, for instance, Knetsch and Sinden, 1984; Brookshire and Coursey, 1987). Several hypotheses have been put forward to try to explain this inconsistency. Hanemann (1991) argues that the degree of substitutability of the good in question can determine the gap between WTP and WTA, and postulates that only for goods with high substitutability will the measures converge, and vice versa. Shogren et al. (1994) found evidence in support of this in an auction experiment that compared the WTP and WTA for a number of highly substitutable (chocolate bars, coffee mugs) and less substitutable (reduced health risk) goods. The other oft cited explanation is that of reference dependence and loss aversion from Kahneman and Tversky's (1979) prospect theory, which theorizes that people view losses differently than gains and, consequently, have utility functions that distinguish between the two. Evidence of this has been found in experiments by Kahneman and Tversky (1981) and in experiments on the endowment effect (see Kahneman, Knetsch, and Thaler, 1990; List, 2003; and Plott and Zeiler, 2005). Recent work has shown that the WTP/WTA gap is diminished by market experience (List, 2003; Shogren et al., 1994).

Value can also be determined using either indirect (revealed preference) or direct (stated preference) measures. Indirect measures attempt to determine the value of a good from actual behaviour, using methods like hedonic pricing and derived demand functions. Direct (stated preference) methods rely on asking people directly about value and include methods such as contingent valuation (CV), contingent behaviour (CB), and choice experiments (CE). Both methods have limitations. Direct methods can suffer from hypothetical bias, starting point bias, strategic behaviour, warm glow, and yeasaying, though researchers have developed tools to mitigate some of these problems (Andreoni, 1990; Blamey, Bennett and Morrison, 1999; Cummings and Taylor, 1999; List and Gallet, 2001; Carson and Groves, 2007). Indirect methods, on the other hand, can suffer from a lack of applicable data (nothing in which to see price embedded) and strong assumptions (such as weak complementarity) (Adamowicz, 1991).

In this study, I employ an open-ended question for eliciting WTP, which is a direct method that simply asks respondents for a monetary value. Using open-ended questions to elicit WTP has been criticized for not mimicking a reality that participants can relate to, as consumption decisions are typically framed as either a dichotomous choice of whether to purchase (yes or no) or as a selection from a set of goods (which one of the three available) (Haab and McConnell, 2002). Opponents argue that this disconnect from familiar decision making processes results in participants responding with "protest zeros" and unreasonably high values (Haab and McConnell, 2002), patterns that I will look for in the data. On the other hand, open-ended elicitation does not suffer from anchoring or starting point bias, and there is some evidence that it may elicit a valuation closer to the true value when compared to dichotomous choice questions (Balistreri et al., 2001).

2.2.2 WTP for agricultural technology and its determinants in India

Literature on the WTP and the determinants of WTP for agricultural technology in India is scant. Generally, studies have found that even the poorest farmers are willing to pay something for services that are relevant to them and offer the potential to improve their livelihoods, such as extension services, health insurance, crop insurance, and veterinarian services (Mathiyazhagan, 1998; Ajuha and Sen, 2006; Ozor, Garforth, and Madukwe, 2011; Kakumanu et al. 2012). I will now discuss some of the determinants that WTP studies have found to be relevant in India. As specific evidence is scarce, the review will be supplemented with some evidence from studies performed in other developing areas as well.

Studies by Ajuha and Sen (2006) and Kakumanu et al. (2012) show education to be positively related to WTP for veterinary services in Orissa, India and weather based crop insurance in Andhra Pradesh, India, respectively. Ozor, Garforth, and Madukwe (2011) found, however, that the number of years of schooling for farmers in Nigeria was negatively related to WTP for extension services, which they argue could be a result of the increased number of employment opportunities available to respondents with more schooling.

The relationship between income and WTP is more complex. Some studies find that as income increases, WTP also increases (Mathiyazhagan, 1998; Kakumanu et al., 2012); however, others demonstrate that the relationship between WTP and income is dependent on the good being valued. To illustrate, Ajuha and Sen (2006) found that income (as measured by a wealth index) was either negatively or positively related to WTP for veterinary services in Orissa, India, depending on where the services were offered. Income was negatively related to the WTP for veterinarian services offered in-centre but positively related to services offered at home, because in-centre services are substituted for more convenient at-home services for those with higher incomes. Aspects of income other than level are also important. For instance, Mathiyazhagan (1998) discovered that receiving a regular (daily or weekly) income was positively related to WTP for a health insurance scheme in Karnataka, India, suggesting that income certainty can be a factor as well.

Four other factors may also be significant. First, family size has been found to positively influence WTP for health insurance (Mathiyazhagan, 1998). Similarly, an increase in household size increases WTP for seed related information in Nigeria and Benin (Horna, Smale, von Oppen, 2005). Second, the literature also suggests that for India, an increase in age is associated with a decrease in WTP for health and crop insurance (Mathiyazhagan, 1998; Kakumanu et al., 2012). Third, caste was a significant determinant of WTP for health insurance (Mathiyazhagan, 1998). Finally, farm size is also positively related to WTP for crop insurance (Kakumanu et al., 2012). Similarly, the number of animals owned was positively related to WTP for veterinary services (Ajuha and Sen, 2006).

2.2.3 Determinants of agricultural technology adoption in developing countries

Other determinants, from the literature on agricultural technology adoption, should also be considered, as the propensity to adopt new technology is closely related to the WTP for it. I will now provide a summary of some of the major determinants of technology adoption.

Profitability, and in the case of seeds, yield, are fundamental determinants of any agricultural technology adoption (Feder, Just and Zilberman, 1985). Risk is also of particular import, especially to the poorest farmers (Binswanger et al., 1980; Rosenzweig and Binswanger, 1993; Moser and Barrett, 2006; Cole et al., 2009). Land tenure arrangements, such as sharecropping, can also profoundly affect adoption decisions (Newbery, 1975; Feder, Just and Zilberman, 1985).

In developing countries, constraints are common and loom large in adoption decisions. Supply constraints, particularly of complementary goods like fertilizer, are typical and can prevent the adoption and diffusion of new varieties (McGuirk and Mundlak, 1991; Kohli and Singh, 1997). Likewise, an adequate supply of labour is vital and must be well timed to coincide with critical stages in agricultural production (Nordman, 1969; Harriss, 1972). Credit and liquidity constraints, either from the lack of institutions or assets to offer as collateral, also have a powerful influence on the decision to adopt new technologies (Bhalla, 1979; Moser and Barrett, 2006; Cole et al., 2010). Finally, Beke (2011) finds significant evidence that the lack of cost effective transportation infrastructure can influence adoption as well.

As acquiring information about new technologies is critical, both learning-by-doing (O'Mara, 1971; Linder et al., 1979; Stoneman, 1981) and learning from others (social learning) (Foster and Rosenzweig, 1995; Munshi, 2004; Dupas, 2009; Conley and Udry, 2010) are also important determinants of adoption. For the same reason, human capital and education can be influential (Gerhart, 1975; Rosenzweig, 1978; Jamison and Lau, 1982; Skinner and Staiger, 2005).

More recently, non-pecuniary determinants, such as stigma, prestige and social norms have also been shown to have an effect on adoption (Moser and Barrett, 2006; Herberich et al., 2011). In addition, researchers are now examining how behavioural anomalies, such as reference price, sunk cost effects, and framing may influence adoption decisions, though the evidence so far is nascent (Cole et al., 2009; Ashraf et al., 2010; Cohen and Dupas, 2010).

2.2.4 Valuation of seed traits

Regarding the valuation of specific seed traits, again, the literature is scarce. Dalton (2004) showed the importance of consumption and non-yield production characteristics using a hedonic model to determine households' WTP for rice traits in West Africa. Plant cycle length, plant height, grain colour, elongation/swelling and tenderness all explained farmers' WTP for new rice varieties. Yield was not significant in Dalton's analysis.

Using experimental seed distributions, Lybbert (2006) found that farmers in Tamil Nadu, India were willing to pay more for seeds with a higher expected yield. He also found limited evidence that farmers would pay more for highly skewed distributions with no downside risk and no evidence that farmers would pay for yield stabilizing distributions with a smaller standard deviation, the latter of which is the focus of this study. Lybbert (2006) only had farmers value a seed with lower standard deviation of yield distributions than the baseline, however, and did not include a seed with a higher standard deviation of yield. In this study, I include both a decrease and an increase in standard deviation of yield as a check for understanding and consistency in results.

2.3 Field Experiments in Development Economics

A search for new methodologies spurred by anxieties about "the reliable identification of program effects in the face of complex and multiple channels of causality" has led to the increased popularity of experimental economics over the last few decades (Banerjee and Duflo, 2008). In addition to traditional laboratory experiments, field experiments have also arisen as a valuable tool by which to test hypotheses, especially within development economics (see Duflo, 2006 for a review). According to Harrison and List's (2004) taxonomy, there are three types of field experiments. First, *artefactual field experiments* differ from conventional lab experiments, because they use *non-standard subject pools* (Harrison and List, 2004). This means that instead of a convenience sample of university undergraduates, the researchers employ more socio-demographically diverse participants, or, most appealingly, subjects from the population of interest. Second, *framed field experiments* also use non-standard subject pools, but they also employ a field context in either the commodity, information set or task (Harrison and List, 2004). Finally, *natural field experiments* employ non-standard subject pools and employ a field context similar to framed field experiments, but differ because participants have no knowledge of their participation (Harrison and List, 2004). Table 2-1 provides a summary of this nomenclature.

Experiment Type	*Nonstandard subject pool*	*Field context in either the commodity, information set or task*	*Subjects have knowledge of participation*
Conventional Lab			X
Artefactual Field	X		X
Framed Field	X	X	X
Natural Field	X	X	

Table 2-1: Summary of Harrison and List's (2004) proposed taxonomy for field experiments.

Field experiments offer some advantages over both traditional laboratory experiments and naturally occurring data, as they are by nature a mixture of the control of the former and realism of the latter. Indeed, laboratory experiments have been criticized for being "environmentally dependent" (lacking external validity) for the very reason they offer such robust results: their controlled environment produces results that perhaps cannot be generalized to reality (Carpenter, Harrison, List, 2004; Banerjee and Duflo, 2008). Field experiments offer an opportunity to see whether people in the real world really do behave as laboratory experiments might predict (Carpenter, Harrison, List, 2004). In addition, Harrison and List (2003) argue that the strong abstraction from reality that creates such high experimental control may actually encourage participants to import external frames with which to interpret the experiment, resulting in a net loss of control. Field experiments, on the other hand, discourage this behaviour, by providing a frame and context upfront – one that is constant across participants. Field experiments (and experiments in general) are also argued to have an advantage over naturally occurring data for interpreting causality (Banerjee and Duflo, 2008). However, they are hardly a panacea. Field experiments suffer from problems of external validity too, as frames and context can become so site-specific that the problem of generalization is essentially reversed (Banerjee and Duflo, 2008). In addition, Heckman (1992) points out that field experiments can suffer from self-selection problems at the level of the organization, as organizations that agree to partnerships with researchers may differ from those that decline such offers. Finally, field experiments also present unique ethical considerations, some of which have been neglected and have resulted in gross ethical oversights[8] (see Barrett and Carter, 2010).

Finally, and quite relevant to this study, Levitt and List (2009) discuss the possible consequences of partnerships for field experiments and the “limits to cooperation” that can ensue. While their discussion considers the effect of collaboration with private industry, NGOs also hold independent agendas that could just as easily result in partners being at cross purposes with researchers. They argue that cooperation can skew research agendas or, by providing low social but high private returns, create suboptimal research output. Most seriously, perhaps, they write that researchers may not be permitted to pursue all treatments or publish negative findings, resulting in partial research and publication bias.

[8] Barrett and Carter (2010) discuss a study by Bertrand et al. (2007) that offered incentives for people in India to obtain driver’s licenses. Their experiment resulted in their subjects bribing officials for licenses rather than obtaining the required training and practice, putting both themselves and those around them at great risk.

3. Conceptual Model

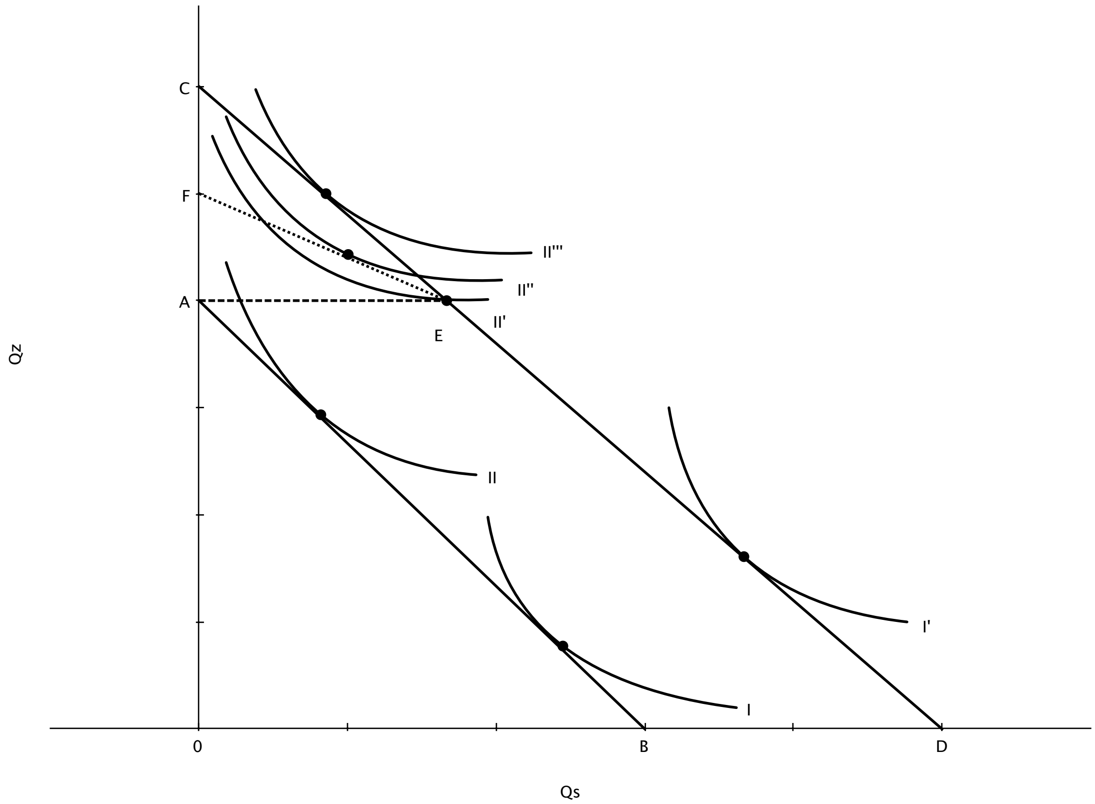
Typically, researchers modeling the decision-making of households that practice mainly subsistence agriculture use either household models that incorporate both production and consumption directly in decision making (see Singh, Squire and Strauss (1986) for a review of studies that use household models) or a utility maximization framework that allows for farmers to select crops "with traits that are desirable for on-farm consumption as well as production and sales" (Horna, Smale, and von Oppen, 2005). I will engage the second approach and assume that farmers maximize utility. Two additional considerations will inform the conceptual model: (1) how payment type may affect utility, and (2) how uncertainty may affect utility.

3.1 Utility and Payment Type

As in the literature review, the most relevant literature compares the possible effects on utility between cash and in kind transfers. In this section, I will review the theoretical differences between these two forms of payment, summarize the main arguments for the use of in kind transfers, and discuss how this may be extended to WTP.

3.1.1 Theoretical differences between in kind and cash transfers

From an example adapted from Cunha (2011), assume that a household consumes two goods, soap (Q_s) and a composite good (Q_z). The initial budget constraint (line AB in Figure 3-1) by which the household maximizes utility $U(Q_s, Q_z)$ is $P_sQ_s+P_zQ_z=Y$, where P_s is the price of soap, P_z the price of the composite, and Y is the household budget constraint. A cash transfer of C increases household income to Y+C, shifting the budget constraint out to $P_sQ_s+P_zQ_z=Y+C$ (line DC). An equivalently valued in kind transfer of soap, however, creates a kinked budget line, the exact slope of which depends on the resale price of soap. If there is no resale, the budget line will be flat from the initial budget line's maximum quantity of the composite good, as it is not possible to increase the consumption of it (AED). If resale is possible, but there are transaction costs such that the resale price (P'_s) is less than P_s, the budget line will be kinked and the shift will be between the initial budget line's maximum quantity of the composite good and the maximum quantity of the composite possible after the cash transfer (FED). If resale is frictionless, the in kind transfer will be equivalent to a cash transfer, and the budget lines will be parallel but shifted outward (CD).



3-1: Comparing the effect of in kind and cash transfers on choice of consumption bundles for extra- and infra-marginal households

Within this setup, it is possible to illustrate the difference between two types of transfers: infra-marginal and extra-marginal. Infra-marginal transfers occur if the amount transferred is smaller than the quantity consumed prior to the transfer (Skoufias, Unar, and Gonzalez-Cossio, 2008). Infra-marginal transfers are equivalent to an equally valued cash transfer, as the household will not be forced to over-consume soap according to its preferences and compared to what would be consumed under an equivalently valued cash transfer. On the other hand, households that were initially consuming less soap than the transfer provides, or for whom the transfer is extra-marginal, may be forced to over-consume soap according to their preferences and depending on the resale price of soap. If resale is anything less than frictionless, households for whom the transfer is extra-marginal will find themselves at a lower indifference curve than if they had received an equal value cash transfer, because the choice of bundles has been restricted. In Figure 3-1, the transfer is infra-marginal for household I but extra-marginal for household II, as the indifference curve I′ denotes a higher level of utility than the curve I, and utility increases from indifference curve II to II‴ (ie. U(II)<U(II′)<U(II″)<U(II‴)). However, transfers must be binding to create distorting effects on consumption. A transfer is considered binding if there is no opportunity for resale, such that the quantity consumed is the same as the quantity that was transferred.

3.1.2 Theoretical arguments for in kind transfers

Despite the possible dead-weight loss of welfare for households receiving in kind transfers and the additional administrative expense that the provision of in kind transfers entails, the majority of transfers in both developed and developing countries are made in kind through the public provision of health services, housing, food, and child care (Cunha, 2011). This section will review some of the theoretical arguments in favour of in kind transfers, as elucidated by Currie and Gahvira (2007).

The most common justification is *paternalism*. Paternalism assumes that donors receive some welfare from, and have preferences about, recipients' consumption behaviour, so societal welfare may be increased by consumption decisions that would not maximize the utility of recipients themselves (Currie and Gahvira, 2007). For example, society may wish households to over-consume goods such as nutritious vegetables, in which case the extra-marginal provision of vegetables is ideal. When society has preferences not about a household's consumption in general, but rather for specific goods, such as food, housing, and health services, it is a form of paternalism that Tobin (1970) described as *specific egalitarianism*. In either case, the outcome of such interdependent preferences is that in kind transfers provide greater welfare than cash transfers in some cases.

Next, *self-targeting* assumes that the government has imperfect information about those for which it wishes to provide certain goods or services, such as the poor (Currie and Gahvira, 2007). As such, they must set up a program of in kind transfers in such a way that the good or service appeals only to the target group, the poor. Such targeting would be impossible with cash transfers, but the provision of low income housing, for instance, would not hold universal appeal as those who are not poor would have preferences for, and the means to attain, larger houses with more features in more fashionable locations.

The *Samaritan's dilemma* is another possible argument for in kind transfers (Currie and Gahvira, 2007). As many transfer programs may depend on qualifications such as low income, they provide a moral-hazard problem for participants: participants who successfully increase their income with the provision of the transfers will no longer receive the transfers. In kind transfers of goods such as education or child health services, however, may avoid this problem, by focusing on building human capital.

Finally, *household redistribution* is another consideration (Currie and Gahvira, 2007). In countries where women hold little decision-making power within the household, it may be difficult to improve the livelihoods of women with cash transfers. A similar argument can be made for the utility of children, who usually hold no decision-making power. In kind transfers of goods or services directed at women or children can potentially address this problem.

To Currie and Gahvira's (2007) list, I can add considerations for *partnerships in development*. As was mentioned previously, the introduction of cash payments into a relationship not based on cash remuneration can have a number of undesirable effects. It could set an unsustainable precedent of future cash payment. It could also offend local leaders and populaces who see the relationship as a partnership and see the introduction of cash payment as altering the power dynamics of the relationship to something akin to an employer/employee relationship. These considerations suggest that the social capital of the partnership is more valuable than the potential loss of welfare, and, possibly, the changes in experimental results, created by in kind remuneration.

3.1.3 Effects of in kind payment on WTP

Given the lack of relevant data and theory, it is difficult to predict what effect, if any, in kind remuneration will have on WTP when compared to cash. From the theory on cash versus in kind transfers as discussed above, if the in kind payment is extra-marginal for a participant, it is possible that she will behave differently than if she had been paid in cash, as the utility from the in kind payment would not be equivalent. Since she would be receiving less utility, she may be willing to pay more within the experimental set up, as the additional risk (from the increased likelihood of adoption) may be more acceptable under a payment regime that offers lower utility. Indeed, if we consider lower utility to be akin to a lowering of the stakes of the experiment, evidence from Holt and Laury (2005) and Harrison et al. (2005) on risk aversion supports the idea that participants make riskier decisions when they face lower stakes. As such, we could postulate that paying in kind will have a positive effect on WTP.

It is also possible that, on average, farmers value the goods less than the equivalent value of cash. The implications are the same; again, the stakes are perceived as lower, and WTP will be higher.

3.2 Utility under Uncertainty in Agriculture

Uncertainty in agriculture has generally been modeled as either price uncertainty (such as Sandmo, 1971) arising from the time gap between making production decisions and the sale of the output, or production uncertainty (such as Just and Pope, 1978), arising from the inherent uncertainty of production technology due to factors such as weather, pests, disease, etc. Given the motivation for this experiment, I am concerned with the latter and will now turn to a model of production uncertainty.

Moschini and Hennessy's (2001) model of utility under production uncertainty assumes that farmers maximize expected utility from initial wealth, w_o, and profits, π:

$$\max E(U(w_o, \pi)) \tag{3-1}$$

$$where\, \pi = pG(x,e) - rx - K \tag{3-2}$$

For the above equations, *p* is the output price, *r* is the input price, *K* represents the fixed costs, and *G(x,e)* is the production function whereby output depends on a vector of inputs, *x*, and a random element, *e*. In this model, profits are stochastic, because they are dependent on production, which is also stochastic.

3.3 Adaption of Moschini and Hennessy

Given the experimental design in this study, there is only one input: the seed, *s*. Output is already given in monetary terms, so there is no output price. There are also no fixed costs. This reduces Moschini and Hennessy's (2001) model to:

$$\pi = G(s,e) - r \quad (3\text{-}3)$$

However, in this study, profits are also dependent upon the adoption and planting of the seed. To account for this, we will multiply the profit equation by *d*, an indicator variable, which will be equal to 1 if the seed is adopted and will be equal to 0 otherwise. The seed is adopted if *WTP*, an individual's maximum WTP, is greater than or equal to the actual seed price, *r*. The model is now:

$$\pi = d(G(s,e) - r) \quad (3\text{-}4)$$

$$where \;\; d = \begin{cases} 1 \; if \; WTP \geq r \\ 0 \; if \; WTP < r \end{cases} \quad (3\text{-}5)$$

Of interest to this study are the determinants of WTP, which will govern adoption, profits, and utility. From Haab and McConnell (2002), an open-ended WTP question may be modeled as:

$$WTP_i = f(\mathbf{z}_i, \varepsilon_i) \quad (3\text{-}6)$$

where $\mathbf{z}_i$ is a vector of individual covariates and ε_i is a random component.

Incorporating the experimental design and treatments of interest, the basic model will be:

$$wtp_{is} = f\left(\boldsymbol{d}, \boldsymbol{t}, \boldsymbol{g}, \boldsymbol{f}\right) + \varepsilon_{is} \quad (3\text{-}7)$$

where wtp_{is} is the WTP of individual i for seed distribution s; $\boldsymbol{d}$, $\boldsymbol{t}$, $\boldsymbol{g}$, *and* $\boldsymbol{f}$ are vectors of seed dummies, treatment dummies (one of which will be payment type), game effects, and farmer characteristics, respectively; and ε_{is} is an error term. Since the only difference between the seeds is experimentally controlled to be the standard deviations of their yield distributions, the effect of the seed dummies on wtp_{is} is equivalent to the effect of changes in the standard deviation on wtp_{is}.

4. Experimental Design, Project Site Description and Data

4.1 Experimental Design

The experiment consisted of three parts:

(1) *Risk Preference Elicitation*: Participants chose a gamble to elicit their risk preferences.

(2) *Seed Valuation Game*: Participants played a seed valuation game to determine their willingness-to-pay (WTP) for yield stabilizing seed traits by valuing seeds with the same expected yield but difference variances. Seeds with lower variances are considered yield stabilizing seeds. The farmers were either paid in cash or in kind for their earnings.

(3) *Survey*: Participants responded to a short questionnaire that collected demographic data, risk exposure perceptions, and information on farm management priorities.

4.1.1 Seed valuation game

This experiment was based on the methodology developed by Lybbert (2006). I asked farmers to make planting decisions for a series of hypothetical farming seasons. Each season, farmers were offered a chance to purchase a seed with a known payoff distribution in Indian rupees (Rs). The distributions were represented using an opaque cloth bag with ten chips inside. Chips were green, black, or red. Each colour corresponded to a harvest level, which in turn corresponded to a payoff in rupees: green for a bad harvest, which paid Rs. 0;

black for an average harvest, which paid Rs. 20; and red for a high harvest, which paid Rs. 40. The share of each colour of chips in the bag was varied to create the different seed distributions. At the end of each season, a harvest was realized by drawing a chip from the bag with the seed distribution. Large posters depicting the distributions (the number of each colour of chips in the bag) were used alongside the actual bag and chips to aid understanding (see Figure 4-1).

After the distribution was explained, the Becker-DeGroot-Marschak (BDM) mechanism (1964) was used to elicit participants' WTP for each seed. Participants were told that the price of the seed could vary within a discrete uniform distribution of prices and that the price would be selected randomly by drawing a price from a bag after they had stated their WTP. Farmers who were willing to pay more than the price that was drawn bought, planted, and realized a harvest for the seed in that season. Net earnings were calculated as the harvest payoff minus the seed cost. Yields were described in monetary outcomes (Rs.) rather than another form of measurement for a two reasons: (1) measurement units are not uniform in the region (one bag vs. one quintal) and (2) to facilitate the simplicity of the game by avoiding another step converting the yield to a payoff. Those who did not buy (adopt) the seed did not realize a harvest, but they had the opportunity to learn from the outcomes of others (social learning) and were protected from potential losses.

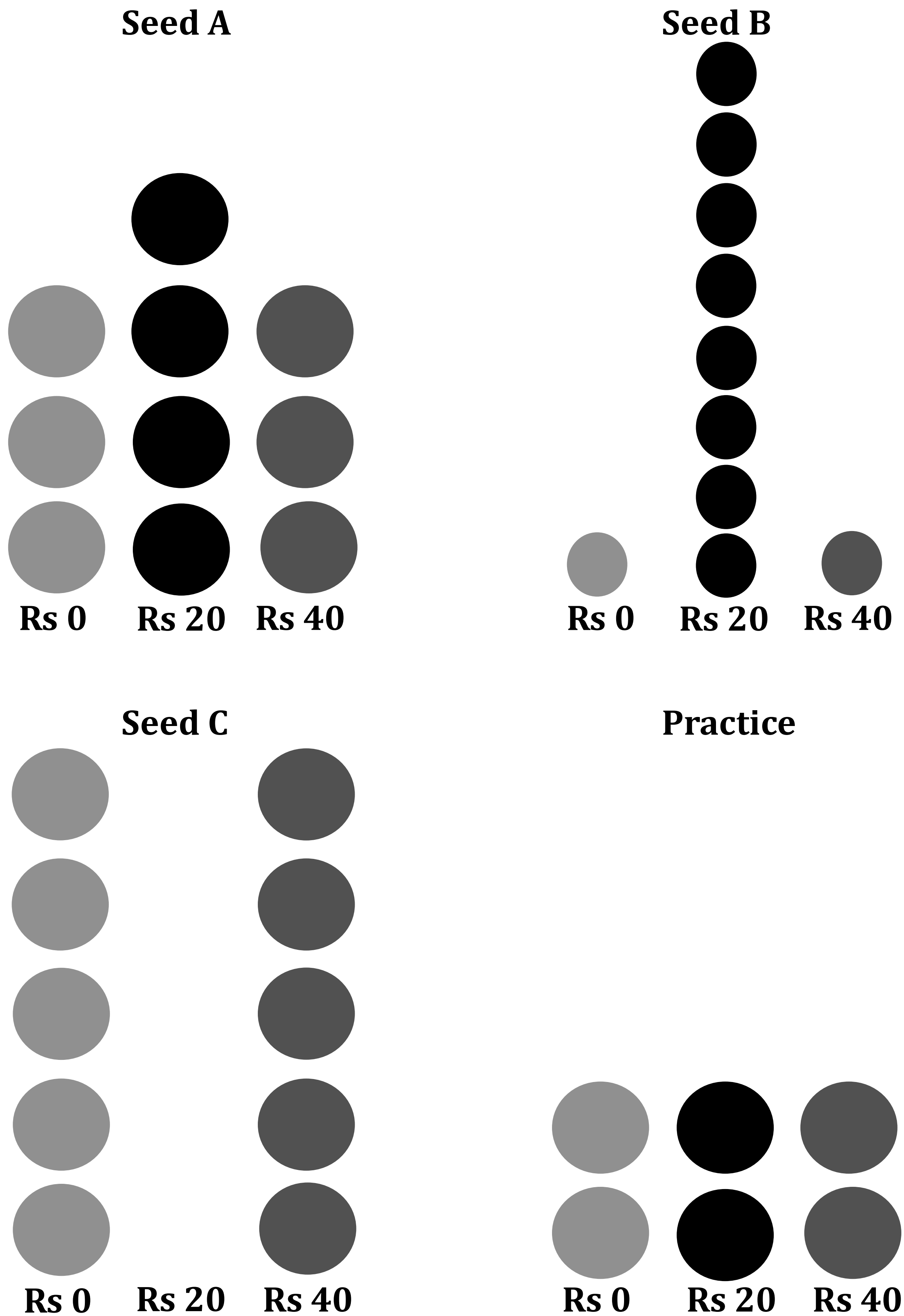


Figure 4-1: Scaled down depiction of posters shown to participants. Posters were originally in colour; the colour of the circles matched the colour of chips.

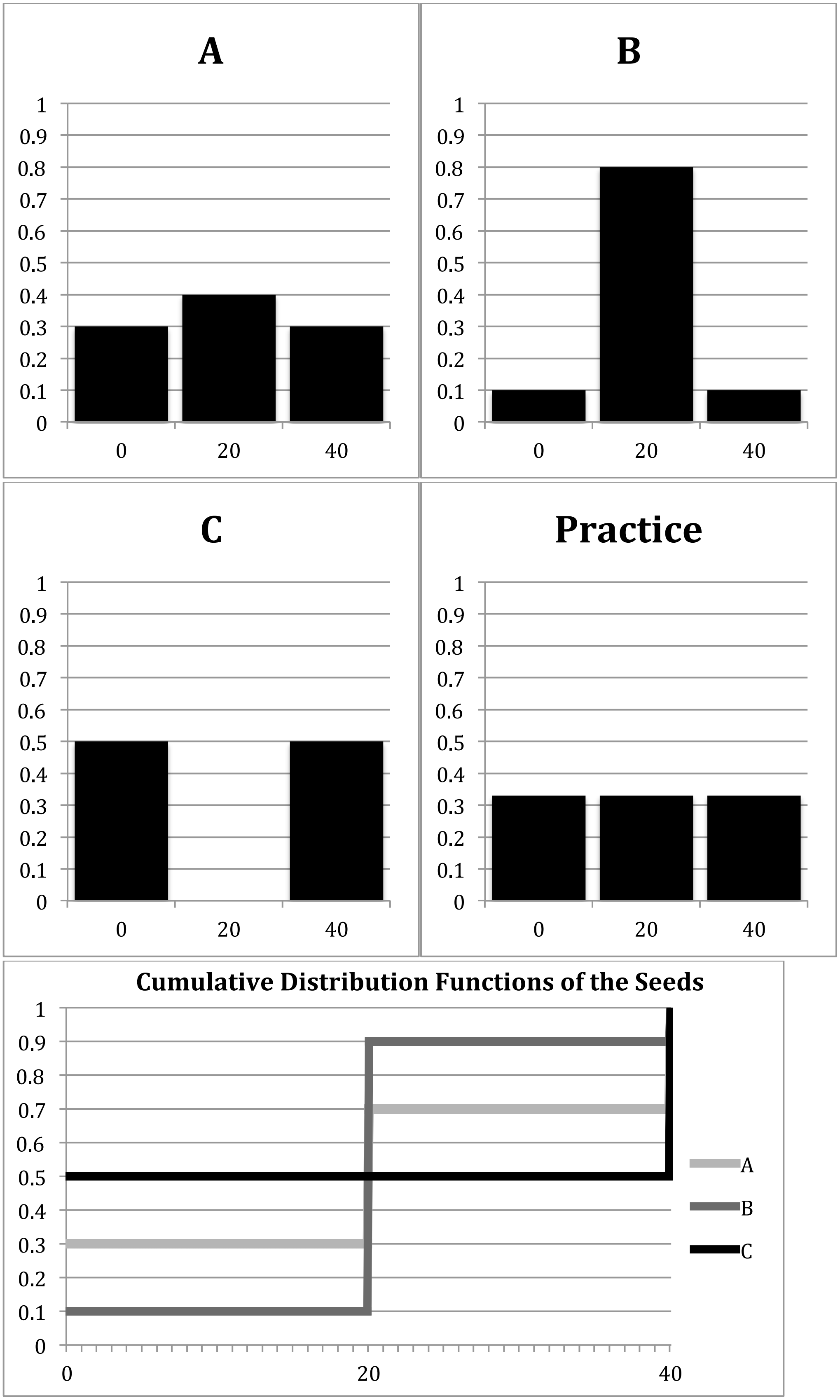


Figure 4-2: Probability and cumulative distribution functions for all seeds

For each seed, this process was repeated for four unpaid practice rounds and one paid “real” round. Each participant valued three seed distributions, for a total of fifteen rounds. To test for order effects, the distributions were presented in two sequences: ABC or ACB. Since Seed A is meant to provide a baseline by which to measure changes in the farmers' WTP, it was always first in the sequence.

To determine the farmers’ valuation of yield stabilizing seed traits, the three distributions were different only in their variance. Figure 4-2 compares the distributions of the seeds. Seed A was designed as a baseline, offering intermediate variance (standard deviation of 16.33) and intermediate chances of both a good and bad harvest. In comparison, Seed B offers a much smaller variance (standard deviation 9.43) (stabilized yield), but also only a very small chance of a good or a bad harvest. Seed C, on the other hand, offers a much higher variance (standard deviation 21.08) (destabilized yield), but also a much greater chance of a good or bad harvest. The expected value of all three distributions was the same (Rs. 20).

Prior to these distributions, a practice seed was valued. To counteract potential anchoring and imprinting effects, the practice seed was of a uniform distribution with two chips of each colour, for a total of six chips (rather than the ten chips of the three experimental distributions of interest). During the rounds with the practice seed, the enumerators asked the participants questions to ensure they understood the procedures of the experiment.

While participants could lose money in any particular round, everyone was guaranteed a minimum of Rs. 40 for their participation. It was possible to win up to Rs. 125, depending on the participant's decisions, the harvests, and the outcome of the gamble (see the section on Risk Preference Elicitation). This was a nontrivial incentive, as the daily wage in the area is Rs. 50[9].

At the end of the experiment, participants were either paid in cash or kind for their earnings. Due to the logistical difficulties of obtaining an adequate supply of small change in the study area, cash payments were rounded up to the nearest Rs. 10. This approach was used for two reasons: (1) participants were not made aware of this "bonus" in advance, so it could not have affected their behaviour in the experiment, and (2) a participant could only stand to increase their earnings. Unfortunately, a drawback from this approach was that those who earned, for instance, 41, as opposed to 49, ended up with a larger bonus. While the enumerators did not report any signs of discontentedness from participants on this score, it was a concern.

Following the approach of Cook et al. (2011), participants who were paid in kind were given vouchers that could be redeemed at the small "shop" we set up at the back of the room in which the experiment was held. Vouchers were similar in shape and size to real rupee notes, though not in colour or design (see Figure 4-3). Discussions with the farmers confirmed that most of them understood both the purpose and value of the vouchers. Our shop offered a variety of commonly

[9] Personal communication with farmer participants.

purchased household goods at two price levels: Rs. 10 or Rs. 5. In order of popularity, the items for sale were laundry detergent, soap, biscuits, and pens. Soap and laundry detergent were both exchanged for Rs. 10 vouchers, and biscuits and pens for Rs. 5. Candies, worth Rs. 1, were given as change for anything smaller than Rs. 5 increments, which is common practice at shops in the area.

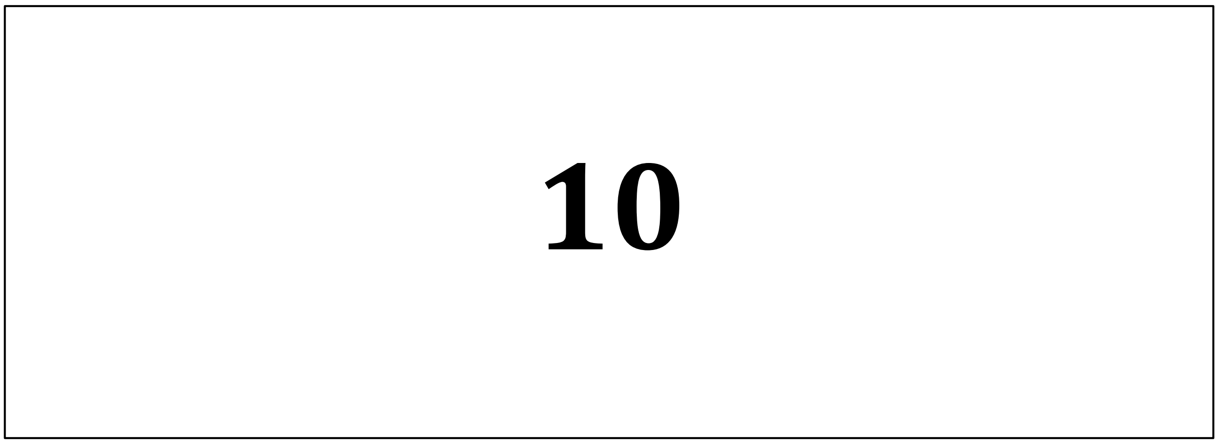


Figure 4-3: Approximate replication of vouchers presented to participants in the in kind payment scenario

To avoid setting the precedent of paying cash for participation in the project villages, which could affect the ongoing work on the larger project, as well as the work of other NGOs in the future, the experiments were held at the MSSRF regional centre in Jeypore. MSSRF felt that holding the experiments at a central location away from the villages would differentiate the experience enough to avoid creating an expectation of payment amongst the villagers in the project area. The centre was approximately 20 kilometres outside of the project area. Villagers who agreed[10] were taken in groups from their villages to the centre by four-wheel

[10] The enumerators recruited participants by asking all adults within a particular village, or section of a village. There was no difficulty in recruiting participants for the experiment, and all

drive vehicles. The experiment took approximately two hours to complete, including the instructions, practice seed, and a follow-up survey that was answered individually with the enumerators following game play. Two groups of 9-14 farmers (male and female) played the game each day, and lunch was provided for the participants.

4.1.2 Framing in seed valuation game

The design of this experiment diverges most significantly from Lybbert's (2006) setup in its approach to abstractness. Harrison and List (2004) argue that experiments that abstract strongly from reality (in information, procedures, tasks, instructions, or commodities) may encourage participants to import external frames with which to interpret the experiment. If this happens, there can be as many "uncontrolled" frames as there are participants. Without information on these uncontrolled frames, the experimenter does not gain, and, in fact, loses, control by increased abstractness. To reduce the likelihood of such uncontrolled framing, my experiment used a less abstract approach.

First, I set the experiment in the context of rice farming. Second, by providing examples that differentiated the seeds by their responsiveness to abiotic stressors, I framed the reduced (increased) variance of the distributions in the context of yield stabilization (destabilization). To illustrate, the enumerators explained to

---

participants who were asked to participate did so. Due to the random nature of the village selection, and the fact that all those who were asked participated, I feel that problems associated with self-selection are minimal. During the time of the experiment, it was the agricultural off-season, and this, in addition to the lunch and remuneration provided, we assume, offered an adequate opportunity and incentive to participant.

participants that Seed B offered a much higher chance of an average harvest, because it is not as sensitive to the conditions in which it is grown. They then gave the example of rainfall, explaining that Seed B would not be as sensitive to seasons with drought. On the other hand, Seed C was described as being very sensitive to the conditions under which it is grown. Again, the rainfall example was used, and it was explained that in years with low or late rainfall, Seed C would perform very poorly.

These frames provide a relevant local context to both seeds. Seed B was framed as a type of drought resistant seed. Though there are currently no drought resistant varieties of rice available in this area, traditional varieties of rice are more resistant to drought than the new hybrid seeds that have been widely adopted in the last four to five years. Many farmers made the connection between the resilience traits of traditional varieties and Seed B; the possible consequences of these connections are discussed later in this section.

Similarly, Seed C was framed somewhat like a hybrid seed. Hybrid seeds offer a higher yield, but they require an intensive fertilizer, pesticide, and labour regime, resulting in much higher costs and cutting into potential profit margins.[11] They are very sensitive to both this input regime and rainfall, and they perform very poorly in drought years. Just as with Seed B, many farmers made the connection between the rainfall sensitivity traits of hybrid seeds and Seed C. Overall, in discussions following the experiment farmers reported that they related very strongly to the

[11] Personal communication with farmer participants.

frames provided, and it is likely that this contributed to their understanding of the yield stabilization differences between the three distributions.

Of course, the downfall of providing this additional context was the likelihood of the farmers importing too much context and attributing other characteristics of the traditional and hybrid seeds to the experimental seeds, such as the higher yield offered by hybrid seeds. To combat this tendency, no specific seed varieties were mentioned, and the enumerators were careful to communicate that the only difference between the seeds in the experiment was their distributions. In practice, they repeated that the time to maturity, labour usage, etc. for all of the seeds were the same, and they asked the participants questions about the differences between the experimental seeds. Discussions with the farmers confirmed that they were concentrating on the distributional differences between the seeds, and, more specifically, the difference in their risk levels and chances of a high harvest, when they made their WTP decision.

4.1.3 Risk preference elicitation

Prior to the seed game, risk preference was revealed using an Eckel-Grossman (EG) gamble selection (Eckel and Grossman, 2002). The EG approach was selected over alternatives, such as Holt and Laury's (2002) series of choices between two gambles, for its simplicity given the low levels of education and familiarity with experiments amongst our population.

Participants were asked to select one of six gambles that each offers a 50% chance of a high payoff and a 50% chance of a low payoff. The gambles increase in both risk (standard deviation) and expected payoff. Participants were given a visual representation of this choice (see Figure 4-4) to accompany the verbal explanation with the bags and chips. Once the participants have chosen a gamble to play, the outcome was decided by placing two chips, one white and one black, in a bag and drawing one of the two chips. The black chip represented a high payoff, and the white chip represented a low payoff. Participants were paid based on the outcome of the gamble they chose. For instance, if the participants were more risk loving and chose number 6 (see Figure 4-4), they would be paid Rs. 35 if the chip was black and Rs. 1 if the chip was white. On the other hand, if the participants were more risk averse and chose gamble 1, they could win Rs. 14 regardless of the chip that was drawn.

After pretesting and some discussion with the farmers, the draw for the EG gamble was made prior to the commencement of the seed game rather than after its completion. When the draw was made prior, farmers had a much easier time understanding the real incentives behind their selection. They also reported that they had an easier time understanding the concept of randomness and its relation to drawing from the bag when they had a chance to see it in this 50/50 context first, which they said increased their level of comfort with the draws for the seed valuation game. As such, it was decided that the gain in understanding from this order of events outweighed the effect that luck (getting the high payment) in the

EG gamble would have on WTP in the seed game. This design effect is controlled and tested for in the analysis that follows.

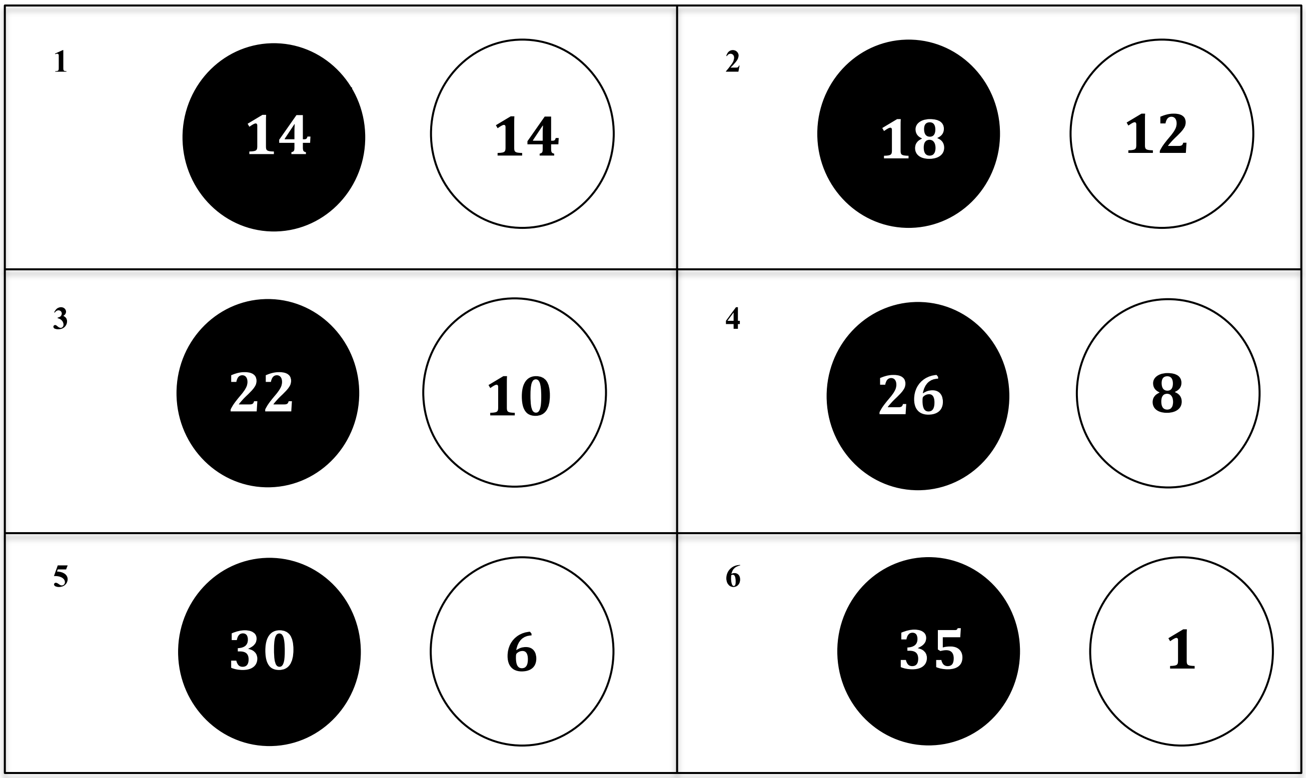


Figure 4-4: Visual representation of Eckel-Grossman gamble options given to participants

4.1.4 Game effects

There are three possible game effects: (1) *wanting to play*: farmers who did not buy the seed in a given round because the price was too high increased their WTP in the next round to increase their chance of purchasing the seed and "playing" the harvest, (2) *preceding earnings*: farmers who had a low (high) harvest in a given round decreased (increased) their WTP in the next round, and (3) *gamble luck*: farmers who were "lucky" and drew a high payment in the EG gamble bid higher in the seed valuation game. All of these possible game effects are considered in the estimation in later chapters.

4.2 Project Site Description

Data for this project was collected as part of the larger Alleviating Poverty and Malnutrition in Agrobiodiversity Hotspots Project, a partnership between the University of Alberta and the M.S. Swaminathan Research Foundation (MSSRF). The project operates in three field sites in southern India, one of which is in Koraput district, Odisha (formerly Orissa), India. Within Koraput, the project operates in 32 intervention villages from the Dangarpaunsi, Banuaguda, and Mosigam *Gram panchayats* (a local level of government) in Kundra block (see Figure 4-5 for a map of the project area). 10 of these villages were randomly selected for this experiment.

Koraput district is located in the Eastern Ghats mountain range in the southwestern part (17°40′ - 20°7′ N and 81°24′ - 84°2′ E) of Odisha. In 2001, the population of Koraput was 1,180,637, with a sex ratio (females per 1000 males) of 999 for all ages and 983 for children 0 to 6 years of age (Orissa Review, 2010). Due to its mountainous geography, it has remained relatively isolated, thus preserving its unique agrobiodiversity and the culture of its aboriginal populations (Shah et al., 2008). Koraput also features one of the highest incidences of poverty in Odisha, which itself is one of the poorest states in India (Shah et al., 2008; Satapathy, 2012). Moreover, Koraput is home to a large proportion of scheduled caste and tribal populations, both of which are populaces that are considerably more likely to be impoverished and have not experienced any of the decline in poverty that has occurred in other parts of the state since the 1990s (Shah et al., 2008; Satapathy, 2012). Koraput's literacy rate is approximately half that of the state's (35%, compared to 63%), as is the Human Development Index (HDI) value (0.236 versus 0.404) (Shah et al., 2008). The district depends heavily on agriculture, but the small scale, marginalized agriculture that is practised provides only a precarious living. For instance, in 1995, land productivity was 1,477 Rs/ha in Koraput compared to 6,317 Rs/ha for the state (Shah et al., 2008). Some of this difference is likely due to the frequent droughts that exacerbate the destitution and uncertainty in the district (Shah et al., 2008).

The major crop in the project villages is rice. According to personal communications with farmers and researchers at MSSRF Jeypore, in the last four to five years, rice cultivation has undergone a huge shift with the introduction of hybrid varieties. Traditional varieties, which contributed to the FAO's designation

Figure 4-5: Map of the project area: Koraput district, Orissa, India

of Koraput as a Globally Important Agricultural Heritage System (GIAHS), are disappearing, though some are still cultivated for personal consumption and for use in festivals and ceremonies[12] (Food and Agriculture Organization, 2011). Farmers report that, although it was always present, uncertainty has become an increasing concern in rice cultivation, because traditional varieties offer much greater protection against drought and climate than the new hybrid seeds, which perform poorly in drought years and are more sensitive to pests and the timing of rainfall. In addition, farmers report that hybrid seeds require large labour, pesticide, and fertilizer inputs, all of which require optimal timing, increase outlay, and depend on availability. Despite this, farmers have widely adopted hybrid seeds, because the hybrid seeds offer a much larger yield under the right conditions.

4.3 Data

4.3.1 Overview

Two enumerators collected data from 185 participants. Because of the panel nature of the data, there are n=555 (185*3) observations. However, of the 555, 138 observations were discarded because of inconsistencies in collection methods that would result in measurement error. Of the remaining 417 observations, 159 (38%) were male and 258 (62%) female, compared to 228 (41%) male and 327 (59%) female before culling. Table 4-1 provides a breakdown of the observations by treatment before and after observations were discarded. Table 4-2 provides

[12] Personal communication with farmers.

descriptive statistics for demographics and variables of interest.

	BEFORE				AFTER		
	In Kind	*Cash*	*Total*		*In Kind*	*Cash*	*Total*
ACB	141	120	261	*ACB*	102	93	195
ABC	135	159	294	*ABC*	99	123	222
Total	276	279	555	*Total*	201	216	417

Table 4-1: Number of observations by payment type and order before and after discarding observations

Of the females, 53% reported that they were not the primary decision maker for what is planted on their farm, compared to only 11% of the males[13]. The mean age is 40 years[14]. On average, a household has 1.6 children and 3.2 adults[15]. The majority (61%) of participants are illiterate. This, along with the restricted size of the room we were able to use, made collecting answers privately from each group member impossible. As such, peer effects are likely, and group effects will be considered in the analysis that follows.[16]

[13] According to personal communications with the farmers, this is a new phenomenon in the region. Men became the primary planting decision makers with the advent of hybrid seeds, as the men visit the larger farmers in the area to learn about hybrid varieties. Previously, with traditional rice varieties, the women would save a portion of the seeds at the end of each season, so they would also determine what seed variety would germinate best at the start of the next.
[14] Most participants (65%), especially elder ones, reported their age in five year increments.
[15] Children are defined as less than 14 years of age, adults 14 or older.
[16] While Lybbert (2006) had farmers participate individually with no communication, he does point out that farmer decisions are often a collective process and that farmers will "interact extensively with other farmers, agro-services dealers and family members as they formulate their input purchasing decisions." In our experiment, we have to consider peer effects in decision making, but the group dynamic may in fact be a process that is more realistic and familiar to the farmers.

	Median	Mean	Std. Dev.	Min	Max
Gender (1=female)	1	0.619	0.487	0	1
Age	38	39.993	13.139	15	65
Adults in household	3	3.237	1.458	1	10
Children in household	1	1.576	1.611	0	7
Land					
Total cultivated	1.5	2.135	1.797	0	10
Owned	1	1.873	1.710	0	10
Lease out	0	0.001	0.008	0	0.1
Lease in	0	0.263	0.767	0	6
Grow an improved variety (1=yes)	1	0.986	0.120	0	1
Primary decision maker (1=yes)	1	0.623	0.486	0	1
of females	0	0.465	0.502	0	1
of males	1	0.885	0.322	0	1
EG gamble selection (6=riskiest)	4	4.022	1.608	1	6
Farm Management Goals (rank)					
increase yield	1	1.230	0.745	1	6
stabilize yield	5	4.640	0.771	2	6
decrease costs	2	2.518	0.837	1	5
use fewer inputs	4	4.453	0.714	2	6
increase quality of produce	6	5.647	0.859	2	6
avoid losses	3	2.518	0.736	1	4
Risk exposure (1=most exposure)					
to price uncertainty	1	1.345	0.678	1	3
to drought	2	1.691	0.769	1	3
to pests	1	1.633	0.744	1	3
	Freq.	**%**			
Most important Crop					
By land area planted					
Rice	399	97.08			
Corn	6	1.46			
millet	6	1.46			
other	0	0.00			
By income generated					
Rice	345	83.33			
Corn	51	12.32			
millet	15	3.62			
other	3	0.72			
Understood the experiment					
Well	330	79.14			
somewhat	75	17.99			
not well	12	2.88			

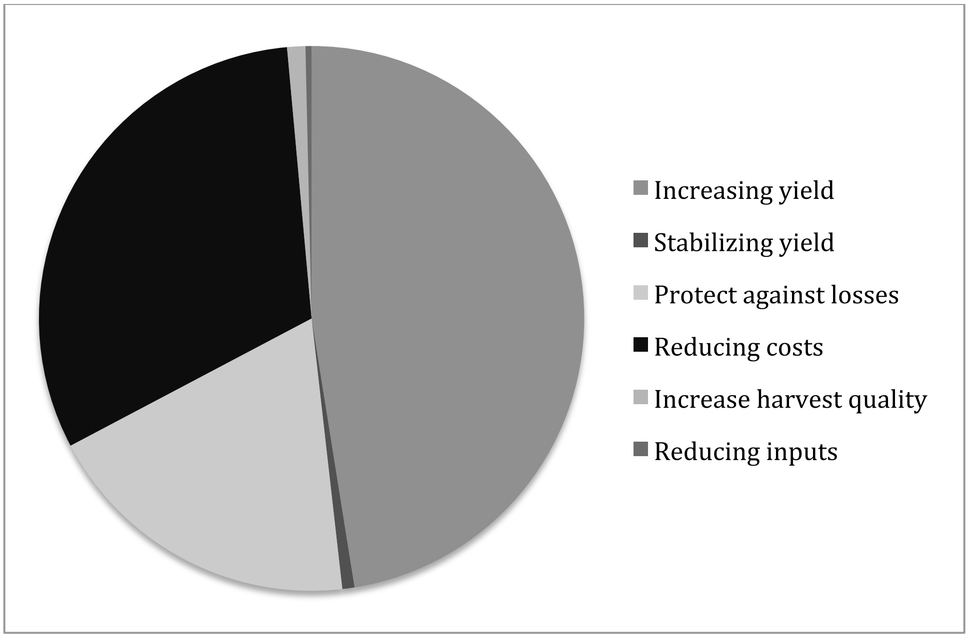
Self reported risk preference		
Avoid risks	192	46.04
Neither seek nor avoid risks	219	52.52
Seek risks	6	1.44
Education		
Illiterate	84	60.87
Some primary	34	24.64
Some high primary	11	7.97
Some secondary	6	4.35
Some high secondary	2	1.45
Degree	1	0.72

Table 4-2: Descriptive statistics for all variables

The most important crop by land area is rice. Rice is also the most important crop for income generation for most farmers, though 17% reported that corn, ragi (millet), or eucalyptus was more important. Every participant grew rice on at least some part of their land[17], and 98.5% grow a hybrid variety of rice. The average farmer cultivates 2.1 acres of land and owns 1.9 himself or herself. 17% of farmers leased additional land to farm, but only one farmer leased land out.

87% ranked increasing yield as their most important farm management goal (Figure 4-6). The most popular second and third choices were either avoiding losses or decreasing costs. Stabilizing yield from season to season was ranked fifth of six choices by most farmers (Figure 4-7).

[17] Except for one farmer who is landless. He/she leases land most seasons but was not able to in the season prior to the experiment.



4-6: Most important farm management goals: proportion of farmers ranking goals as either most or second most important

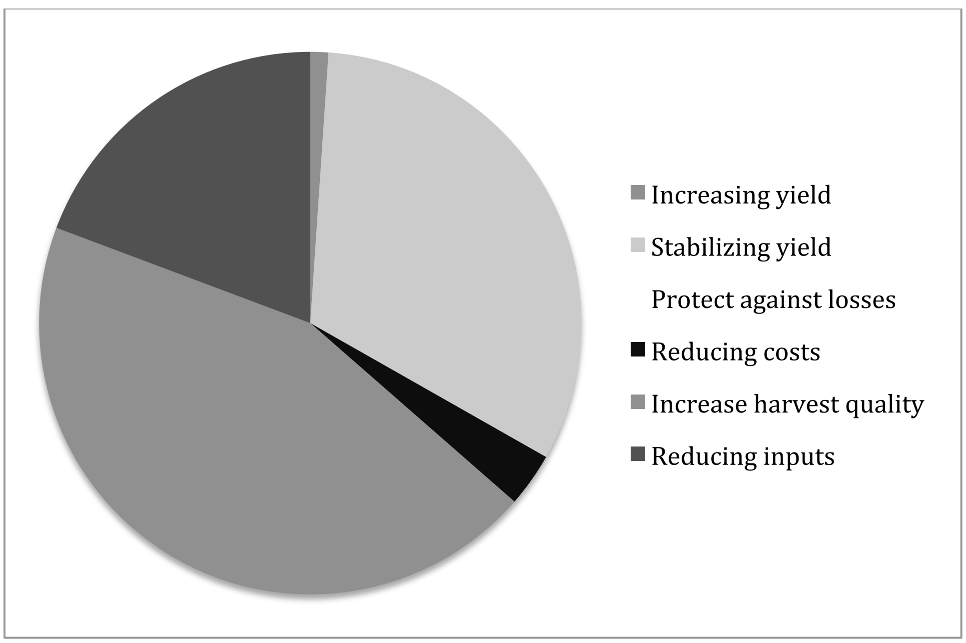


Figure 4-7: Least important farm management goals: proportion of farmers ranking goals as either least or second least important

Most farmers reported that they were either risk averse or risk neutral in comparison to other farmers. On the other hand, the selection of the EG gamble showed a diverse set of risk preferences, with a tendency towards more risk loving gambles: 24% of participants chose the most risky gamble (

4-8).

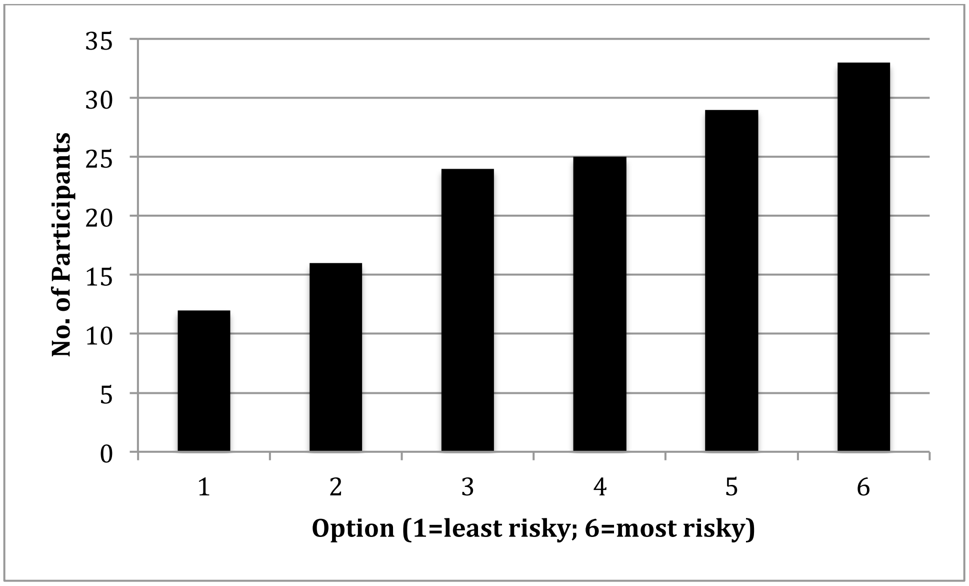


Figure 4-8: Frequency chart for participants' Eckel-Grossman gamble choice, N=139

77% of farmers reported that they were at risk of uncertainty of the price at which they could sell their produce "most of the time". From conversations with the farmers, much of this was a result of a government purchase program. If farmers can sell their produce to the government, they almost always do, because usually the price paid to them is much higher. However, the government purchasing is only done on fixed dates, so if the harvest is late, the farmers miss this opportunity and instead sell to smaller stores or buyers who usually offer lower prices. Pests and drought were also a pressing concern, with about 50% of farmers reporting that these were problems most of the time as well.

Finally, 79% of participants professed to understand the experiment 'well', 18%

‘somewhat’, and only 3% ‘not well’. This is supported by the enumerators' observations of the participants' quickness at entering into the calculations of the seed valuation game and ability to answer questions regarding game design during the valuation of the practice seed.

4.3.2 Summary statistics: WTP

WTP ranges from Rs. 10 to Rs. 20. The values taper off as would be expected on the left side towards 10, but they increase in density towards 20. This is because WTP was top-coded at 20. There are no zero bids in the data, as may be expected from an open-ended elicitation of WTP (Haab and McConnell, 2002). There are s explanation for this: (1) as in Lybbert (2006), when we asked farmers about a zero bid, they reported that they must plant something every year or they will not get a harvest, so being unwilling to pay anything seemed illogical to them, and (2) prices for subsidized seeds available through the *panchayat* (government) office start at Rs. 10, so a price lower than this was inconceivable. Figures 4-9 and 4-10 show a histogram of WTP for all seeds and then by seed, respectively.

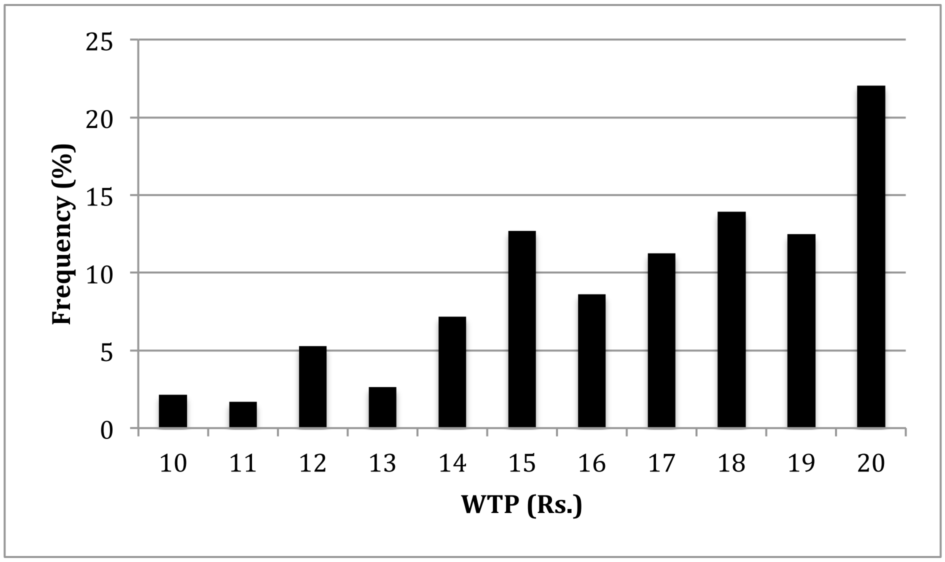


Figure 4-9: Participants' willingness to pay (WTP) for all seeds

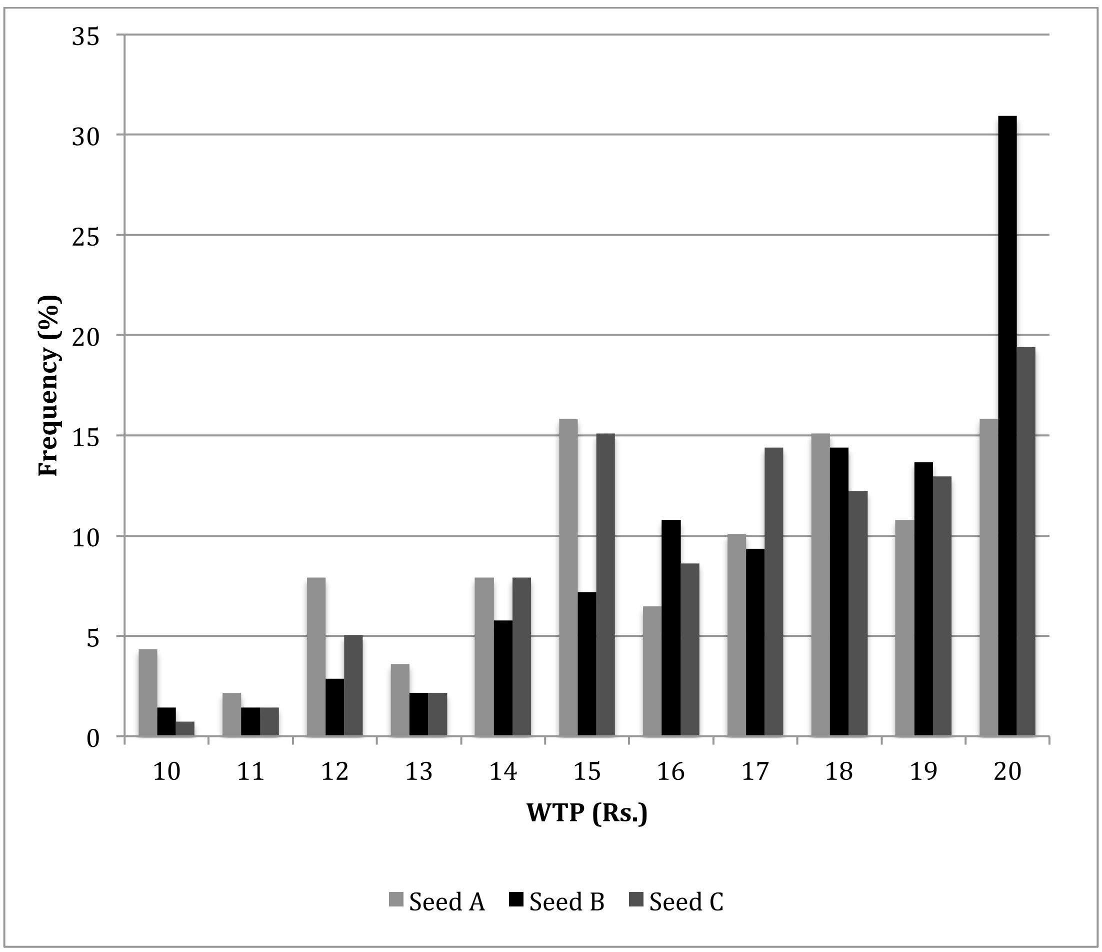


Figure 4-10: Participants' willingness to pay (WTP) by seed

Table 4-3 provides some additional summary statistics for WTP by seed, gender, and treatment. Mean WTP for both Seed B and Seed C are significantly different from Seed A (pvalues of 0.0001 and 0.0604, respectively), suggesting that the seed distributions did affect WTP. Females were willing to pay more for the seeds (pvalue=0.0177). Order did not have an effect on WTP for Seed B or Seed C, but, inexplicably, the WTP for Seed A is higher for participants in ACB than in ABC (Rs. 16.9 versus 15.7; pvalue=0.0129). Finally, WTP is higher when participants were paid in kind (pvalue=0.0001).

	Obs	Mean WTP	Std. Dev.	Min	Max
Seed					
A	139	16.252	2.892	10	20
B	139	17.504	2.543	10	20
C	139	16.863	2.509	10	20
Gender					
female	258	17.124	2.575	10	20
male	159	16.465	2.844	10	20
Order					
ABC	222	16.635	2.857	10	20
ACB	195	17.144	2.481	10	20
Payment					
in kind	201	17.398	2.498	10	20
cash	216	16.384	2.786	10	20

Table 4-3: Participants' willingness to pay (WTP) by seed, gender, order, and payment

5. Empirical Strategy

5.1 Econometric Issues

Before estimating, several econometric issues had to be considered, including censoring, panel data, the combination of panel data models and censoring, and clustered errors.

5.1.1 Censoring and the Tobit model

Censoring occurs when some values are not known exactly but are known to be above or below a certain value[18] (Wooldridge, 2002). For instance, in a survey, income may be right censored due to top coding, which happens when income is recorded as an exact value only up to a certain cutoff, say $500,000. After $500,000, the exact value is not recorded, and it is only known from the data that income for some values is equal to or above $500,000. This implies that the distribution function consists of both continuous and discrete portions (Greene, 2003). Because the distribution of the dependent variable is no longer continuous, ordinary least squares (OLS) is inconsistent (Wooldridge, 2002). Instead, a censored regression model, such as a Tobit model, should be used (Wooldridge, 2002).

[18] This is distinct from truncation, which occurs when some values are never observed within a range. This can happen because of a sampling strategy that targets only a subset of the population, such as only examining houses below a certain income level (Wooldridge, 2006).

The Tobit model, first proposed by Tobin (1958), is based on the idea that the censored variable is only equivalent to the actual, or latent, variable for the uncensored range of values (Wooldridge, 2002). Using the example above, let y_i be the censored variable observed income for individual *i* and y_i* be the uncensored latent variable actual income. Then, the standard censored Tobit model would be:

$$y_i* = \begin{cases} y_i \ if \ y_i* < 500000 \\ 500000 \ if \ y_i* \geq 500000 \end{cases} \quad (5\text{-}1)$$

$$where \ y_i* = \mathbf{B}\mathbf{x_i} + \varepsilon_i \quad (5\text{-}2)$$

Here, **B** is a vector of coefficients, $\mathbf{x_i}$ is a vector of regressors, and ε_i is an error term.

This model assumes homoscedasticity and normality of the errors. As Wooldridge (2006) points out, these restrictions showcase the costliness of censoring, as consistent estimators can be found to account for heteroscedasticity and nonnormality if the data is uncensored. When appropriate, the tobit model will account for non-linearity and produce consistent estimates with maximum likelihood estimation (Tobin, 1958). However, if the data is uncensored and a tobit model is used, the estimation will be misspecified and suffer from the restrictiveness of the tobit assumptions, not allowing for estimation under heteroscedasticity or nonnormality, with no gain in efficiency.

5.1.2 Panel data: fixed and random effects models

Traditionally, panel data consists of repeated observations for the same individual or geographical region over a number of different time periods (Wooldridge, 2006). In this experiment, each individual valued three distributions, resulting in panel data consisting of repeated observations for the same individual for a number of different seed distributions. Panel data is advantageous because it can control for omitted variable bias, reduce multicollinearity, and generally allow for more complicated analysis by differentiating between inter- and intra-individual differences (Hsiao, 2001). On the other hand, it can usually no longer be assumed that the error terms are uncorrelated, and this must be accounted for in the estimation (Verbeek, 2008). For instance, in my data, it would be reasonable to expect that the error terms associated with each observation of WTP are not independent within individuals, but that they are independent across individuals.

Two common panel data models are fixed effects and random effects (Wooldridge, 2006). A fixed effects model includes a dummy variable for each individual in order to estimate individual-specific intercept terms that will control for individual specific unobserved heterogeneity (Wooldridge, 2002). This approach assumes that individual specific effects are fixed values and can only control for unobserved factors that are constant over the three seed distributions (Wooldridge, 2002). As such, one of the drawbacks of fixed effects models is that regressors that do not vary over time, such as education or gender, cannot be estimated, as the estimation procedure differences them out. The second drawback is that estimating so many dummy variables entails the loss of degrees of freedom, reducing the efficiency of the estimation (Kennedy, 2008)

Alternatively, a random effects model assumes that individual specific effects are random values with a distribution (Wooldridge, 2002). Consequently, they can be thought of as part of a composite error term that comprises both the traditional random error component and the random unobserved heterogeneity (Wooldridge, 2002). Unlike fixed effects, random effects estimation does not use differencing, so the effect of time-invariant regressors can be estimated. It also does not entail the loss of so many degrees of freedom, making it a more efficient estimator (Kennedy, 2008). However, for random effects to be consistent, the individual specific effects must be uncorrelated with any of the other regressors, which is a very restrictive condition (Kennedy, 2008). Since the fixed effects approach is always consistent, even if it is not the most efficient, fixed effects are generally thought to be "a more convincing tool for estimating ceteris paribus effects" (Wooldridge, 2006).

To help determine if a random effects model is appropriate, a Hausman test can be conducted to test whether the non-correlation condition holds (Wooldridge, 2002). The test compares the estimates of the two approaches. Because the random effects model is inconsistent when the individual specific effects are correlated with the regressors but fixed effects is not, a significant difference between the estimates suggests that this assumption does not hold (Wooldridge, 2002). Under the null hypothesis, both fixed and random effects are consistent, and a rejection of the null suggests that only the fixed effects estimator is consistent (Wooldridge, 2002). However, if there is large sampling variation in the fixed effects estimates, the Hausman test is weak (Wooldridge, 2006).

5.1.3 Censoring and panel data

Though Honore (1992) has developed a semiparametric estimator for fixed effects tobit models, a parametric conditional fixed effects model is not possible, as there is still no sufficient statistic that can condition fixed effects out of the likelihood function (Statacorp, 2009). In addition, unconditional fixed effect tobit models, though estimable, are biased, making fixed effects tobit models unattractive (Statacorp, 2009). Random effects tobit models can be used to account for censoring in panel data; however, just as in a random effects model without panel datavvv, its appropriateness is dependent on the assumption that the individual specific effects are uncorrelated with the regressors.

5.1.4 Clustered errors

In the discussion on panel data, it was mentioned that it would be reasonable to expect that errors be correlated within, but not across, individuals in panel data. In both the fixed effects and random effects models, this correlation was called the unobserved individual specific effects and was accounted for in the estimation.

Correlated errors may apply in other cases as well, often as a result of sampling strategy or experimental design (Wooldridge, 2006). In this experiment, farmers participated in groups, which may have resulted in peer effects within experimental groups. Additionally, most groups were made up of farmers from the same or neighbouring villages. As such, farmers who played the game in the same experimental group are likely to have errors correlated with other members of their group, but not necessarily with members of other groups. This group level clustering of errors violates the independent and identically distributed (iid) assumption and causes the standard errors, and, consequently, any inferences made, to be incorrect (Wooldridge, 2002). Fortunately, cluster robust standard errors can be calculated to allow for inference accounting for the within group correlation (Wooldridge, 2002). The final model is estimated using cluster robust standard errors.

5.2 Estimation

5.2.1 Model 1: Abosolute WTP

Based on the discussion in the last section, the data is analyzed using a tobit model to account for right censoring (Wooldridge, 2002). Since the data was top coded at a WTP of 20, the observable variable *wtp* is equal to the latent variable *wtp** whenever it is below 20 and equal to twenty otherwise. The model is specified as:

$$wtp = \begin{cases} wtp^* \; if \; wtp^* < 20 \\ 20 \; if \; wtp^* \geq 20 \end{cases} \tag{5-3}$$

$$where \quad wtp^*_{is} = f\left(\boldsymbol{d}, \boldsymbol{t}, \boldsymbol{g}, \boldsymbol{f}\right) + \varepsilon_{is} \tag{5-4}$$

wtp^*_{is} is the WTP of individual *i* for seed distribution *s;* ***d, t, g,*** *and* ***f*** are vectors of seed dummies, treatment dummies, game effects, and farmer characteristics, respectively; and ε_{is} is an error term.

From this generic model, three different regressions were estimated, each including progressively more explanatory variables. Model 1-1 includes only the seed dummies (*sB, sC*), treatment dummies (*cash, abc*), and game effects (*play, prearn, gamluck*, *group*)[19]:

Model 1-1

$$wtp^*_{is} = \beta_0 + \beta_B sB_s + \beta_c sC_s + \beta_1 cash_i + \beta_2 abc_i + \beta_3 play_{is} + \beta_4 prearn_{is} + \beta_5 gamluck_i + \beta_6 group_i + \varepsilon_{is} \tag{5-5}$$

Model 1-2 comprises Model 1-1, as well as demographic variables (*female, age, education*), a measure of risk exposure (*riskexp*)[20], and indicators of risk preference (*EG*)[21] and wealth (*land*):

[19] See the chapter on Experimental Design for more information on treatments and possible game effects.
[20] The risk exposure was a very simplified version of Lybbert's (2006). Participants were asked to rate, on a three-point scale, how often they faced three types of risk: drought, pest, and market (price for their output). This was then aggregated to make a single measure of risk exposure from 1 to 7, with 7 being the most risk exposed.
[21] See the chapter on Experimental Design for more information on the Eckel-Grossman (2002) gamble as a measure of risk preference.

Model 1-2

$$wtp^*_{is} = \beta_0 + \beta_B sB_s + \beta_c sC_s + \beta_1 cash_i + \beta_2 abc_i + \beta_3 play_{is} + \beta_4 prearn_{is} + \beta_5 gamluck_i + \beta_6 group_i + \beta_7 female_i + \beta_8 age_i + \beta_9 education_i + \beta_{10} riskexp_i + \beta_{11} EG_i + \beta_{12} land_i + \varepsilon_{is} \quad (5\text{-}6)$$

Model 1-3 comprises Model 1-1 and a number of interaction terms (*sBfem, sCfem, sBEG, sCEG, sBrisk, sCrisk, sBland, sCland*). Table 5-1 provides a complete list and description of variables included in the models, as well as details as to the regressors included in each of the three specifications of Model 1.

Model 1-3

$$wtp^*_{is} = \beta_0 + \beta_B sB_s + \beta_c sC_s + \beta_1 cash_i + \beta_2 abc_i + \beta_3 play_{is} + \beta_4 prearn_{is} + \beta_5 gamluck_i + \beta_6 group_i + \beta_7 sBfem_i + \beta_8 sCfem_i + \beta_9 sBEG_i + \beta_{10} sCEG_i + \beta_{11} sBrisk_i + \beta_{12} sCrisk_i + \beta_{13} sBland_i + \beta_{14} sCland_i + \varepsilon_{is} \quad (5\text{-}7)$$

Variable	Type	Model 1-1	Model 1-2	Model 1-3	Description
sB	Dummy	*	*	*	Equal to 1 if the distribution valued was Seed B, 0 else.
sC	Dummy	*	*	*	Equal to 1 if the distribution valued was Seed C, 0 else.
cash	Dummy	*	*	*	Equal to 1 if the participant was paid in cash. Equal to 0 if the participant was paid in kind.
abc	Dummy	*	*	*	Equal to 1 if the order ABC was used, 0 else.
play	Dummy	*	*	*	Equal to 1 if the participant adopted in the preceding round, 0 else.
prearn	Continuous	*	*	*	Earnings for the preceding round in rupees.
gamluck	Dummy	*	*	*	Equal to 1 if the participant's draw for the payment of the EG gamble was high, 0 else.
group	Categorical	*	*	*	Indicates participant's group number in the experiment.
female	Dummy		*		Equal to 1 if participant is female, 0 else.
age	Continuous		*		Participant's age in years
education	Categorical		*		Participant's education level, from 1 to 7, see Data section for a more detailed breakdown.
riskexp	Categorical		*		Index of the participant's level of risk exposure, from 1 to 7, where 7 indicates the highest risk exposure.
EG	Categorical		*		Participant's selection of EG gamble, from 1 to 6, where 6 is the riskiest option (indication of risk preferences).

land	Continuous	*		Amount of land owned by the participant in acres.
sBfem	Interaction		*	sB*female
sCfem	Interaction		*	sC*female
sBEG	Interaction		*	sB*EG
sCEG	Interaction		*	sC*EG
sBrisk	Interaction		*	sB*riskexp
sCrisk	Interaction		*	sC*riskexp
sBland	Interaction		*	sB*land
sCland	Interaction		*	sC*land

Table 5-1: Summary and description of variables for all models

The experimental design provides panel data that can be analyzed using a random effects tobit model to correct the test statistics for individual specific effects. However, a Hausman test ignoring the censoring results in a rejection of null hypothesis (p=0.0002), suggesting that the random effects may be correlated with other regressors and, therefore, a random effects specification may be inappropriate (Verbeek, 2008). Consequently, the data is pooled and two more regressions were estimated for specification of model 1: (1) a pooled tobit, and (2) a pooled tobit with standard errors adjusted for clustering at the group level. Given the possibility of peer effects within experimental groups, the pooled tobit with cluster adjusted standard errors is probably the most appropriate model. The results of a random effects tobit regression are still estimated for comparison but should be considered with caution.

5.2.2 Model 2: Incremental WTP

As the experiment is set up to examine the incremental valuation of changes to the standard deviation of the yield distributions, I also estimate a model with incremental WTP as the dependent variable:

$$\Delta wtp_{is} = f(\boldsymbol{d}, \boldsymbol{g}, \boldsymbol{f}) + \varepsilon_{is} \tag{5-8}$$

where Δwtp_{is} is the incremental WTP of individual *i* for seed *s*, calculated as the difference between the WTP for the variance reduced (seed B) or increased (seed C) distribution and the baseline distribution (seed A). ***d*** and ***g*** are vectors of seed dummies and game effects, respectively, as before; ***f*** is a vector of farmer dummies (equal to 1 for farmer *i*) to control for individual specific effects, and ε_{is} is an error term. The model is specified as:

Model 2-1

$$\Delta wtp_{is} = \alpha_B + \alpha_c sC_s + \alpha_1 play_{is} + \alpha_2 prearn_{is} + \delta_3 ' \boldsymbol{f}_i + \varepsilon_{is} \tag{5-9}$$

In this model, the constant is the incremental WTP for seed B. As the incremental WTP is calculated without accounting for censoring, any effect of the censoring on the model is lost. However, without censoring, it is possible to estimate this model using a fixed effects regression. Consequently, there is only one specification for Model 2, as the effect of time invariant regressors, such as demographic variables and risk preference, are not estimated using fixed effects.

As before, the model is estimated adjusting the standard errors for group clustering. The results of a fixed effects regression with unadjusted standard errors and an OLS regression with adjusted standard errors are also included for comparison.

6. Results

Data analysis for all estimations was performed using the software package STATA.

6.1 Model 1: Absolute WTP

6.1.1 Model 1-1

wtp	**Pooled tobit, SE adjusted for group level clusters**			**Pooled tobit**			**Random effects tobit**		
	Coef.	*Std. Err.*		*Coef.*[22]	*Std. Err.*		*Coef.*	*Std. Err.*	
constant	17.572	0.512	***	17.867	1.144	***	17.789	1.236	***
sB	1.530	0.559	***	1.530	0.393	***	1.552	0.378	***
sC	0.806	0.767		0.805	0.387	**	0.801	0.372	**
cash	-1.175	0.471	**	-1.381	0.816	*	-1.357	0.885	
abc	-0.416	0.514		-0.419	0.326		-0.421	0.353	
play	-0.844	0.479	*	-0.846	0.381	**	-0.679	0.392	*
prearn	-0.011	0.027		-0.011	0.019		-0.0129	0.019	
gamluck	0.134	0.421		0.117	0.348		0.086	0.376	
group	---	---		-0.022	0.078		-0.018	0.085	
No. of obs.	417			417			417		
Right censored obs.	92			92			92		
Number of clusters	12			---			---		
Log likelihood	-922.869			-922.830			-921.551		
Psuedo R^2	0.022			0.022			---		

Table 6-1: Regression results from Model 1-1 using different estimation approaches (significant at the 1% (***), 5% (**), and 10% (*) level)

In the pooled tobit with standard errors adjusted for group clustering, the coefficient for *sB* is positive and significant (p<0.01), indicating that farmers were willing to pay a premium of Rs. 1.530, or about 9%, for the yield stabilized

[22] Note that the coefficients estimated for the pooled tobit are not equivalent to the pooled tobit with adjusted standards errors because the variable *group* is included as an explanatory variable when it is not used for clustering errors.

distribution. This is contrary to Lybbert's (2006) findings[23] that there was no statistical difference between WTP for the baseline and stabilized distributions.

Again looking at the pooled tobit with adjusted standard errors, farmers were willing to pay less when they were paid cash than when they were paid in kind ($p<0.10$). If we assume that the valuation elicited under cash compensation is the "true" valuation, this means that in kind remuneration could result in overestimation. Bids were higher by Rs. 1.175 when farmers were paid in kind, corresponding to about a 7% higher valuation.

Finally, again for the pooled tobit with adjusted errors, the game effect *play* was significant and negative, suggesting that farmers were also willing to pay less if they had adopted in the preceding round. This negative effect may also be explained in part by the possibility of real losses in the fifth round of the game. It is also possible that the consequences of the real losses may have been more salient to the adopters of the preceding round, as they had just realized a harvest.

Leaving the standard errors of the pooled tobit unadjusted for clustering only changes the significance of the coefficient for *sC*, making it positive and significant. This would suggest that farmers were willing to pay a premium for seeds that increased yield variance, as well as seeds that reduced yield variance. In the random effects tobit estimation, *sC* is also positive and significant and *cash* is

[23] Note that Lybbert did not include a distribution with increased variance, akin to Seed C, in his study.

no longer significant. All other results are the same, allowing for slight changes of statistical significance (from 5% to 10%, for instance).

6.1.2 Model 1-2

wtp	**Pooled tobit, SE adjusted for group level clusters**			**Pooled tobit**			**Random effects tobit**		
	Coef.	*Std. Err.*		*Coef.*[24]	*Std. Err.*		*Coef.*	*Std. Err.*	
constant	16.990	1.254	***	17.859	1.491	***	17.807	1.598	***
sB	1.483	0.558	***	1.482	0.390	***	1.500	0.377	***
sC	0.748	0.760		0.747	0.385	*	0.744	0.371	**
cash	-1.060	0.476	**	-1.829	0.841	**	-1.810	0.903	**
abc	-0.310	0.459		-0.300	0.352		-0.304	0.377	
play	-0.809	0.476	*	-0.808	0.384	*	-0.676	0.392	*
prearn	-0.009	0.025		-0.009	0.020		-0.010	0.020	
gamluck	0.241	0.507		0.225	0.402		0.206	0.431	
group	---	---		-0.084	0.084		-0.081	0.090	
female	0.498	0.494		0.558	0.383		0.561	0.412	
age	-0.009	0.010		-0.010	0.013		-0.010	0.014	
education	-0.014	0.065		-0.002	0.068		-0.004	0.073	
riskexp	0.116	0.099		0.138	0.107		0.143	0.115	
EG	0.006	0.136		0.003	0.105		0.002	0.113	
land	-0.051	0.113		-0.034	0.103		-0.037	0.111	
No. of obs.	414			414			414		
Right censored obs.	90			90			90		
Number of clusters	12			---			---		
Log likelihood	-915.451			-914.949			-913.979		
Psuedo R^2	0.024			0.024			---		

Table 6-2: Regression results from Model 1-2 using different estimation approaches (significant at the 1% (***), 5% (**), and 10% (*) level)

Additional demographic, wealth, or risk related terms do not seem to improve the model. None of the added coefficients in Model 1-2 are significant in any of the regressions. The coefficients on *sB*, *cash*, and *play* remain statistically significant and have the same sign as in the estimation for Model 1-1.

[24] See note 22.

6.1.3 Model 1-3

wtp	**Pooled tobit, SE adjusted for group level clusters**			**Pooled tobit**			**Random effects tobit**		
	Coef.	*Std. Err.*		*Coef.*[25]	*Std. Err.*		*Coef.*	*Std. Err.*	
constant	17.429	0.559	***	18.294	1.155	***	18.206	1.246	***
sB	1.383	1.330		1.240	1.276		1.425	1.276	
sC	-0.715	1.469		-0.848	1.263		-0.776	1.257	
cash	-1.088	0.493	**	-1.695	0.824	**	-1.657	0.892	*
abc	-0.374	0.504		-0.375	0.331		-0.399	0.358	
play	-0.839	0.457	*	-0.845	0.381	**	-0.689	0.391	*
prearn	-0.010	0.028		-0.010	0.019		-0.012	0.020	
gamluck	0.277	0.555		0.246	0.375		0.218	0.403	
group	---	---		-0.065	0.081		-0.060	0.087	
sBfem	1.037	0.776		1.105	0.615	*	1.067	0.615	
sCfem	0.384	0.503		0.449	0.605		0.427	0.605	
sBEG	0.022	0.189		0.038	0.167		0.018	0.167	
sCEG	0.147	0.186		0.161	0.162		0.149	0.161	
sBrisk	-0.142	0.215		-0.143	0.179		-0.160	0.179	
sCrisk	0.015	0.173		0.013	0.177		0.001	0.177	
sBland	-0.019	0.130		-0.008	0.170		0.011	0.170	
sCland	0.239	0.079	***	0.251	0.178		0.277	0.179	
No. of obs.	417			417			417		
Right censored obs.	92			92			92		
Number of clusters	12			---			---		
Log likelihood	-919.534			-919.207			-917.956		
Psuedo R^2	0.025			0.026			---		

Table 6-3: Regression results from Model 1-3 using different estimation approaches (significant at the 1% (***), 5% (**), and 10% (*) level)

Additional interactions terms do not appear to improve the model either. Of all the interactions terms, only *sCland* is significant in the regression with adjusted standard errors. It is positive, suggesting that farmers with more land are willing to pay more for the riskier seed (increased variance). This is in accordance with previous findings that farm size is positively related to the intensity of the

[25] See note 22.

adoption of new technologies under uncertainty (Feder, Just, and Zilberman, 1985).

In the pooled tobit with unadjusted standard errors, *sBfem* is positive and significant, suggesting that females are willing to pay more for reduced variance. This agrees with previous findings that women are more risk averse and less likely to adopt new technologies (Fletschner, Anderson, and Cullen, 2010; Croson and Gneezy, 2009).

6.2 Model 2: Incremental WTP

6.2.1 Model 2-1

Δwtp	**Fixed effects, SE adjusted for group level clusters**			**Fixed effects**			**OLS, SE adjusted for group level clusters**		
	Coef.	*Std. Err.*		*Coef.*	*Std. Err.*		*Coef.*	*Std. Err.*	
constant[26]	1.122	0.271	***	1.122	0.227	***	1.167	1.041	
sC	-0.749	0.397	*	-0.749	0.298	**	-0.829	0.431	*
play	0.526	0.430		0.526	0.522		1.447	0.846	
prearn	0.005	0.023		0.005	0.024		-0.067	0.028	**
cash	---	---		---	---		-0.723	1.103	
abc	---	---		---	---		1.344	0.975	
gamluck	---	---		---	---		-0.808	1.151	
No. of obs.	278			278			278		
Number of clusters	12			---			12		
R^2	---			---			0.0668		

Table 6-4: Regression results from Model 2-1 using different estimation approaches (significant at the 1% (***), 5% (**), and 10% (*) level)

Table 6-4 provides a summary of the fixed effects estimations for Model 2-1. The farmer dummies are jointly significant (p=0.000), suggesting that the addition of

[26] The constant is the incremental willingness to pay for Seed B.

farmer fixed effects is beneficial and the individual specific effects are significant. In the fixed effects regression with cluster adjusted standard errors, we see from the significant and negative coefficient on *sC* that farmers are willing to pay less by Rs. 0.749 when yield variance is increased. Correspondingly, the constant (the incremental WTP for seed B) is positive and significant, suggesting that the farmers are again willing to pay a premium (Rs. 1.122) for a reduction in yield variance. This is the reverse of Lybbert's (2006) findings[27] that the incremental WTP for a stabilized seed was significantly negative. In this model, the results do not change appreciably when standard errors remain unadjusted to clustering.

6.3 Summary of Results

Generally, farmers were willing to pay more for decreased variance in yield. For a Rs. 6.9 reduction in the standard deviation, the premium ranged from Rs. 1.530 (9% of total WTP) to 1.383 (8%) in the absolute WTP models and was Rs. 1.122 in the incremental WTP models.

Farmers were willing to pay more when they were paid in kind than when paid in cash. If we assume that the WTP elicited using cash payment to be the true valuation, this implies that participants may overbid when they are paid in kind by a factor of about 7%.

[27] See note 7.

In the absolute WTP models, regressors that consistently explained WTP were *cash*, *sB* and *play*. Demographic variables, indicators of wealth, and indicators of risk preference did not help to explain WTP. *sC* was significant in a few of the regressions and suggested, along with the coefficient on sB, that farmers were simultaneously willing to pay more for increased and decreased variance. This is somewhat perplexing; however, all of the estimations in which *sC* is statistically significant may suffer from misspecification, either by failing to correct the standard errors for clustering or by inappropriately applying a random effects model.

In the incremental WTP models, both *sB* and *sC* are consistently significant. In this case, the coefficient on *sC* is negative, suggesting that farmers were willing to pay less for seeds with increased variance. This was in accordance with the finding that farmers in these models were willing to pay more for seeds with decreased variance.

7. Discussion and Conclusion

7.1 Summary and Implications

Adapting an experimental approach developed by Lybbert (2006), I conducted a study eliciting farmers' WTP for yield stabilizing seed traits in Koraput, Orissa, India. The results are summarized below.

7.1.1 Comparing in kind and cash payment

To the best of my knowledge, in this study I present the first results comparing the effects of in kind and cash payment on participants' WTP in an experimental setting. Using a prize voucher system based on Cook et al.'s (2011) study, I find that participants who are paid in kind with commonly bought household goods (soap, laundry detergent, biscuits, etc.) are willing to pay about 7% more than when they are paid in cash, suggesting that in kind remuneration may result in the overestimation of WTP. While 7% may seem like a small difference in absolute terms, it is not necessarily insignificant, as farmers may be operating on very small profit margins.

There are a few possible explanations for the difference in WTP between payment types. First, the in kind transfers that we offered may have been extra-marginal. Second, the goods we offered were household goods that would likely be shared amongst family members, which, if we assume the cash would not as likely be shared, may have led to different incentives. Relatedly, another possible explanation is that participants valued the goods less than an equivalent value of cash, resulting in a perceived lowering of the game stakes. However, perceived differences in value would likely be minimal as the original value of the goods is already fairly low (Rs. 10 maximum) and their value well known (ie. the goods are familiar).

The results imply that researchers working with partners in developing countries who are asked not to use cash payments should consider the effects of in kind payment and the possibility of overestimation carefully, as it may affect the interpretation and comparability of their results. This will be true not only for WTP studies, but all economics experiments in which remuneration is offered, such as dictator or trust games, and studies from beyond the field of economics as well.

7.1.2 Farmer valuation of yield stabilizing seed traits

Farmers were willing to pay significantly more for seeds that had decreased variance. For a Rs. 6.9 reduction in the standard deviation, WTP was approximately 8% higher. This implies that even with a price premium, there is a possible demand for yield stabilizing seed traits and suggests that farmers in the project villages would likely adopt, and benefit from the distribution of, drought or pest resistant varieties. It also indicates that the farmers were capable of recognizing the value of the yield stabilizing traits, despite the stochastic nature of their benefits. While I did not model farmers' learning, after only five rounds (four practice and one real) with each seed, the value of the yield stabilization was evident to them, which provides hope for the possibilities of learning about stochastic benefits.

I did not find any significant demographic, wealth or risk preference variables that helped to explain WTP. This is similar to Lybbert's (2006) findings that only a few farmer traits provided even statistically weak predictions of WTP for seed traits. As an explanation, Lybbert writes, "Farmers' constraint sets (e.g., access to credit and inputs) may determine their adoption of pro-poor seeds that reduce risk more than their risk preference-based valuation of these seeds." I offer more evidence in support of this possibility.

In conclusion, anecdotally, there was one group who played the game while continually joking with the enumerator. When I asked her later what they had been saying, she said, "They were asking: "Where is Seed B? We need Seed B." " Though their comments were made in jest, knowing the seed distributions were abstractions, their sentiment expressed a real recognition of, and desire for, the certainty offered by the yield stabilizing distribution of Seed B. Without considering opportunity cost, these findings support the research and development of yield stabilizing seed traits, such as drought and pest resistant varieties, to help alleviate some of the vulnerability that poor farmers face.

7.2 Limitations and Directions for Future Research

The main limitations of this study were the high proportion of data that was discarded due to inconsistent collection methods and the censoring of observations. The consequences of these complications were a large reduction in sample size and greater econometric restrictions, which limited the choice of model specification.

Regarding experimental design, while short-term losses in a given round were possible, net earnings were always positive. While this is the only ethically responsible course of action, it does not reflect the true nature of agricultural losses and their potential to critically impact the livelihoods of poor farmers, aspects of decision making that would be impossible to capture in an experimental setting.

Moreover, while we took every opportunity to ensure comprehension, it remains a possibility that some farmers did not understand the experiment but were able to participate without signaling as much by copying other members of the group. The survey that followed game play included a self-reporting of understanding and was conducted individually to allow farmers to indicate privately if they did not understand. 79% of participants professed to understand the experiment ‘well’, but the possibility of mimicry remains.

In the experiment, I chose to provide participants with a framed explanation of yield variance for a specific crop. While I feel that this enhanced understanding and prevented substantial uncontrolled framing, participants may have imported context related to the descriptions we provided. Including a treatment where no frame is used would allow for a direct assessment of how framing affects both WTP and understanding.

While Lybbert (2006) had farmers participate individually, he does point out that farming decisions are often a collective process and that farmers will "interact extensively with other farmers, agro-services dealers and family members as they formulate their input purchasing decisions." While the dynamic in this experiment, which was played in groups, may be a process that is more like what Lybbert is referring to, I must consider peer effects on decision-making in the econometric modeling. It would be better to perform the study with some participants individually and some in groups to determine the effect and extent of peer effects.

Further experiments including a payment type treatment of cash or in kind would allow for a more accurate assessment of the possibility of overbidding when participants are paid in kind. An inflation factor calculated from the aggregation of these works could be used as a conversion factor for researchers who find themselves in the position of needing to use only in kind payments. It would also be of interest to include a hypothetical scenario with no payment as a treatment and compare the inflation factor that exists under this condition to that of in kind and cash payment.

Additionally, further research could be done to determine why WTP changes with the payment scheme. It may be that people's risk preferences changes with the payment type and perceived stakes, or it may have something to do with a switch between social and monetary exchanges.

Another natural extension of this study is framing the distributions with other crops, such as maize, millets, or cassava, and other seed traits, such as expected yield or skewness. A comparison of the WTP for a reduction in the variance of the yield of a cash crop, for instance, may differ significantly from that of the WTP for the same for a subsistence crop. A more comprehensive understanding of what farmers value in seeds will allow for more directed research: is skewness (removal of downside risk) more important than yield stabilizing through reduced variance?

Finally, the study could also be performed in other locations and with different populations. Areas of rain fed agriculture may have a higher WTP than areas where irrigation is more widespread. Identifying target populations will be key to the successful distribution and adoption of new technologies.

References

Adamowicz, W. 1991. "Valuation of Environmental Amenities." *Canadian Journal of Agricultural Economics*, 39: 609–618.

Ahuja, V. and A. Sen. 2006. *Willingness to Pay for Veterinary Services: Evidence from Poor Areas in Rural India*. Research Report 06-03. Pro-Poor Livestock Policy Initiative.

Andreoni, J. 1990. "Impure Altruism and Donations to Public Goods: A Theory of Warm Glow Giving." *Economic Journal,* 100 (401): 464-77.

Ashraf, N., J. Berry, and J. Shapiro. 2010. "Can higher prices stimulate product use? Evidence from a field experiment in Zambia." *American Economic Review*, 100 (5): 2383–2413.

Balistreri, E., G. McClelland, G. Poe, and W. Schulze. 2001. "Can Hypothetical Questions Reveal True Values? A Laboratory Comparison of Dichotomous Choice and Open-Ended Contingent Values with Auction Values." *Environmental and Resource Economics,* 18 (3): 275-292.

Banerjee, A. 2004. "The Two Poverties." In *Insurance Against Poverty,* ed. S. Dercon, 59-75. Oxford, Oxford University Press.

Banerjee, A. and E. Duflo. 2008. *The Experimental Approach to Developmental Economics.* NBER Working Paper No. 14467. National Bureau of Economic Research.

Barrett, C. and M. Carter. 2010. "The Power and Pitfalls of Experiments in Development Economics: Some Non-random Reflections." *Applied*

Economic Perspectives and Policy, 32(4): 515-548.

Becker, D., M. DeGroot, and J. Marschak. 1964. "Measuring Utility by a Single-Response Sequential Method." *Behavioral Science,* 9: 226-232.

Beke, T. 2011. "Institutional constraints and adoption of improved rice varieties: Econometric evidence from Ivory Coast." *Review of Agricultural and Environmental Studies - Revue d'Etudes en Agriculture et Environnement*, 92 (2): 117-141.

Bertrand, M., S. Djankov, R. Hanna, and S. Mullainathan. 2007. "Obtaining a Driver's License in India: An Experimental Approach to Studying Corruption." *Quarterly Journal of Economics,* 122(4): 1639–76.

Bhalla, S. 1979. "Farm size, productivity, and technical change in Indian agriculture." In *Agrarian Structure and Productivity in Developing Countries*, ed. R. Berry and W. Cline. Baltimore: Johns Hopkins Univ. Press.

Binswanger, H., J. Dayantha, T. Balaranaia, and D. Sillers. 1980. *The Impacts of Risk Aversion on Agricultural Decisions in Semi-arid India*. Washington, D.C.: World Bank, Development Economics Department.

Blamey, R., W. Bennett, and M. Morrison. 1999. "Yea-Saying in Contingent Valuation Surveys." *Land Economics,* 75 (1): 126-141.

Brookshire, D. and D. Coursey. 1987. "Measuring the value of a public good: An empirical comparison of elicitation procedures." *American Economic Review*, 77 (4): 554-566.

Brown, T. 1984. "The concept of value in resource allocation." *Land Economics,* 60 (3): 231-246.

Carpenter, J., G. Harrison, and J. List. 2004. "Field Experiments in Economics: An Introduction." In *Field Experiments in Economics*, Vol. 10 (Research in Experimental Economics). Emerald Group Publishing Limited.

Carson, R. and T. Groves. 2007. "Incentive and informational properties of preference questions." *Environmental and Resource Economics*, 37 (1): 181-210.

Cohen, J., and P. Dupas. 2010. "Free Distribution or Cost-Sharing? Evidence from a Randomized Malaria Prevention Experiment." *Quarterly Journal of Economics*, 125 (1): 1–45.

Cole, S., X. Gine, J. Tobacman, P. Topalova, R. Townsend, and J. Vickery. 2009. *Barriers to household risk management: Evidence from India*. Working Paper. New York: Federal Reserve Bank.

Cook, J., S. Chatterjee, D. Sur, and D. Whittington. 2011. *Measuring Risk Aversion among the Urban Poor in Kolkata, India*. Working Paper, SSRN. Online.

Corrigan, J., D. Depositario, R. Nayga, X. Wu, and T. Laude. 2009. "Comparing Open-Ended Choice Experiments and Experimental Auctions: An Application to Golden Rice." *American Journal of Agricultural Economics,* 91: 837–853.

Croson, R. 2005. "The Method of Experimental Economics." *International Negotiation,* 10: 131-148.

Croson, R., and U. Gneezy. 2009. "Gender Differences in Preferences." *Journal of Economic Literature*, 47(2): 448–74.

Cummings, R. and L. Taylor. 1999. "Unbiased Value Estimates for Environmental Goods: A Cheap Talk Design for the Contingent Valuation Method." *American Economic Review,* 89 (3): 649-665.

Cunha, J. 2011. *Testing Paternalism: Cash vs. In kind Transfer in Rural Mexico*. Discussion Papers, No 09-021. Stanford Institute for Economic Policy Research. Online.

Currie, J. and F. Gahvari. 2007. *Transfers in cash and in kind: Theory meets the data*. Working Paper No. 13557. National Bureau of Economic Research.

Dalton, T. 2004. "A household hedonic model of rice traits: economic values from farmers in West Africa." *Agricultural Economics,* 31: 149-159.

Dalton, T., M. Yesuf, and L. Muhammad. 2011. *Demand for Drought Tolerance in Africa: Selection of Drought Tolerant Maize Seed using Framed Field Experiments*. Annual Meeting Agricultural and Applied Economics Association, July 24-26, Pittsburgh, Pennsylvania.

Duflo, E. 2006. *Field Experiments in Development Economics*. Prepared for the World Congress of the Econometric Society. Online.

Dupas, P. 2009. *Short-run subsidies and long-run adoption of new health products: evidence from a field experiment*. Unpublished manuscript, University of California: Los Angeles.

Eckel, C. and P. Grossman. 2002. "Sex Differences and Statistical Stereotyping in

Attitudes Toward Financial Risk." *Evolution and Human Behaviour,* 23(4): 281-295.

The Economist. 2003. "Planting a seed." Special Report, 27 March. Online.

Feder, G., R. Just, and D. Zilberman. 1985. "Adoption of Agricultural Innovations in Developing Countries: A Survey." *Economic Development and Cultural Change,* 33: 255-298.

Fletschner, D., C. Anderson, and A. Cullen. 2010. "Are Women as Likely to Take Risks and Compete? Behavioural Findings from Central Vietnam." *Journal of Development Studies,* 46 (8): 1459–1479.

Food and Agriculture Organization. 15 Dec 2011. *Globally Important Agricultural Heritage Systems (GIAHS)*. Online.

Foster, A., and M. Rosenzweig. 1995. "Learning by doing and learning from others: human capital and technical change in agriculture." *Journal of Political Economy,* 103 (6): 1176–209.

Fraker, T., A. Martini, and J. Ohls. 1995. "The Effect of Food Stamp Cashout on Food Expenditures: An Assessment of the Findings from Four Demonstrations." *The Journal of Human Resources*, 30(4): 633-49.

Gerhart, John. 1975. *The Diffusion of Hybrid Maize in West Kenya*. Mexico City: Centro Internacional de Mejoramiento de Maiz y Trigo.

González-Cossío, T., J. Rivera-Dommarco, J. Gutiérrez, S. Rodríguez, D. González, M. Unar, J. Leroy, and S. Bertozzi. 2006. *Evaluation of the nutrition status of children under 5 years old and their mothers, and of food expenditures of marginalized families in Mexico*. (In Spanish).

Instituto Nacional de Salud Publica report.

Greene, W. 2003. *Econometric Analysis*, 5th Ed. New Jersey: Prentice Hall.

Griliches Z. 1957. "Hybrid corn: an exploration in the economics of technological change." *Econometrica*, 25: 501–22.

Haab, T. and K. McConnell. 2002. *Valuing Environmental and Natural Resources: The Econometrics of Non-Market Valuation.* Northampton: Edward Elgar Publishing.

Hanemann, W. 1991. "Willingness to pay and willingness to accept: How much can they differ?" *American Economic Review*, 81 (3): 635-647.

Harrison, G. and J. List. 2004. "Field Experiments." *Journal of Economic Literature*, 42 (4): 1009-1055.

Harrison, G., E. Johnson, M. McInnes, and E. Rutström. 2005. "Risk aversion and incentive effects: Comment". *American Economic Review,* 95: 897–901.

Harriss, Barbara. 1972. "Innovation Adoption in Indian Agriculture: the High-yielding Variety Program." *Modern Asian Studies*, 6 (1): 71-98.

Heckman, J. 1992. "Randomization and Social Policy Evaluation." In *Evaluating Welfare and Training Programs.* Cambridge, MA: Harvard University Press.

Herberich, D., J. List and M. Price. 2011. *How Many Economists does it take to Change a Light Bulb? A Natural Field Experiment on Technology Adoption.* Working Paper. Online.

Heyman, J. and D. Ariely. 2004. "Effort for Payment: A Tale of Two markets." *Psychological Science*, 15 (11) 787-793.

Holt, C., and S. Laury. 2002. "Risk Aversion and Incentive Effects." *American Economic Review*, 92(5): 1644-1655.

Holt, C., and S. Laury. 2005. "Risk Aversion and Incentive Effects: New data without order effects." *American Economic Review*, 95(3): 902-904.

Honoré, B. 1992. "Trimmed LAD and least squares estimation of truncated and censored regression models with fixed effects." *Econometrica,* 60: 533-565.

Horna, J., M. Smale, and M. von Oppen. 2005. *Farmer Willingness to Pay for Seed-Related Information: Rice Varieties in Nigeria and Benin*. Discussion Paper 142. Environment and Production Technology Division, IFPRI.

Hoynes, H. and D. Schanzenbach. 2009. "Consumption Responses to In kind Transfers: Evidence from the Introduction of the Food Stamp Program." *American Economic Journal: Applied Economics*, 1 (4): 109-39.

Jamison, D., and L. Lau. *Farmer Education and Farm Efficiency*. Baltimore: Johns Hopkins University Press, 1982.

Just, R. and R. Pope. 1978. "Stochastic Specification of Production Function and Economic Implications." *Journal of Econometrics,* 7 (1): 67-86.

Kakumanu, K., K. Palanisami, K. Reddy, U. Nagothu, K. Tirupataiah, S. Xenarios, and B. Ashok. 2012. *An insight on farmers' willingness to* *pay for insurance premium in South India: Hindrances and challenges*. Edlis, Institute of Development Studies. Online.

Kahneman, D., J. Knetsch, and R. Thaler. 1990. "Experimental Tests of the Endowment Effect and the Coase Theorem." *Journal of Political Economy*, 98 (6): 1325-48.

Kahneman, D. and A. Tversky. 1979. "Prospect Theory: An Analysis of Decision Under Risk." *Econometrica,* 47: 263-292.

Kahneman, D. and A. Tversky. 1981. The framing of decisions and the psychology of choice. *Science, New Series*, 211 (4481): 453-458.

Knetsch, J. and Sinden, J. 1984. "Willingness to Pay and Compensation Demanded: Experimental Evidence of an Unexpected Disparity in Measures of Value." *The Quarterly Journal of Economics*, 99 (3): 507-21.

Kohli, D. and N. Singh. 1997. *The green revolution in Punjab, India: The economics of technical change*. Technical report, Conference on Agriculture of the Punjab at The Southern Asian Institute, Columbia University.

Labarta, R. 2009. *Are small Sub-Sahara African farmers willing to pay for vegetative propagated orange fleshed sweetpotato planting material? Evidence from Central Mozambique*. Annual Meeting Agricultural and Applied Economics Association, July 26-28, Milwaukee, Wisconsin.

Levitt, S. and J. List. 2009. "Field Experiments in Economics: The past, the present, and the future." *European Economic Review*, 53: 1-18.

List, J. 2003. "Does Market Experience Eliminate Market Anomalies?" *The Quarterly Journal of Economics*, 118 (1): 41-71.

List, J. and C. Gallet. 2001. "What Experimental Protocol Influence Disparities Between Actual and Hypothetical Stated Values?" *Environmental & Resource Economics*, 20 (3): 241-254.

List, J. and J. Shogren. 1998. "The Deadweight Loss of Christmas: Comment." *American Economic Review*, 88 (5): 1350-55.

Lusk, J. and D. Hudson. 2004. "Willingness-to-Pay Estimates and Their Relevance to Agribusiness Decision Making." *Review of Agricultural Economics,* 26 (2): 152-169.

Lybbert, T. 2006. "Indian farmers' valuation of yield distributions: Will poor farmers value 'pro-poor' seeds?" *Food Policy,* 31(5): 415–41.

Lybbert, T. and A. Bell. 2010. "Stochastic benefit streams, learning and technology diffusion: Why drought tolerance is not the new Bt." *AgBioForum,* 13(1), 13–24.

Mathiyazhagan, M. 1998. "Willingness to Pay for Viable Rural Health Insurance Scheme through Community Participation in India." *International Journal of Health Planning and Management*, 13 (1): 47-67.

McGuirk, A. and Y. Mundlak. 1991. *Incentives and constraints in the transformation of Punjab agriculture*. Research Report 87, International Food Policy Research Institute, Washington D.C.

Moffitt, R. 1989. "Estimating the Value of an In kind Transfer: The Case of Food

Stamps." *Econometrica*, 57 (2): 385-409.

Moschini, G. and D. Hennessy. 2001. "Uncertainty, risk aversion, and risk management for agricultural producers." In *Handbook of Agricultural Economics*, Vol. 1, Ed. B. Gardner and G. Rausser. Elsevier Science.

Moser, C., and C. Barrett. 2006. "The complex dynamics of smallholder technology adoption: the case of SRI in Madagascar." *Agricultural Economics*, 35: 373–88.

Moyo, S., G. Norton, J. Alwang, I. Rhinehart, and M. Deom. 2007. "Peanut Research and Poverty Reduction: Impacts of Variety Improvement to Control Peanut Viruses in Uganda." *American Journal of Agricultural Economics,* 89(2): 448-460.

Munshi, K. 2004. "Social learning in a heterogeneous population: technology diffusion in the Indian green revolution." *Journal of Development Economics*, 73 (1): 185–215.

Napasintuwong, O., and G. Traxler. 2009. "Ex-ante impact assessment of GM papaya adoption in Thailand." *AgBioForum, 12*(2), 209-217. Online.

Newbery, D. 1975. "Tenurial Obstacles to Innovation." *Journal of Development Studies,* 11: 263-77.

Nordman, D. 1969. "Labour Inputs of Farmers: A Case Study of the Zaria Province of the North-Central State of Nigeria." *Nigerian Journal of Economic and Social Studies,* 11: 1-13.

O'Mara, G. 1971. *A Decision-theoretic View of Technique Diffusion in a Developing Country*. Ph.D. dissertation, Stanford University.

Orissa Review. Dec 2010. *Koraput District*. Census Special Issue. Government of Orissa. Online.

Ornstein, P. 2009. "Fighting hunger with flood-tolerant rice." CNN, Davis, California. Online.

Ozor, N., C. Garforth, and M. Madukwe. 2011. "Farmers' willingness to pay for agricultural extension service: Evidence from Nigeria." *Journal of International Devlopment*, Wiley Online Library.

Pandey, G. 2009. "India's 'drought-resistant rice'." BBC News, Hazaribag, Jharkhand. Online.

Plott, C. and K. Zeiler. 2005. "The willingness to pay-willingness to accept gap, the "endowment effect," subject misconceptions, and experimental procedures for eliciting valuations." *American Economic Review*, 95 (3): 530-545.

Rawlings, L. and G. Rubio. 2005. "Evaluating the Impact of Conditional Cash Transfer Programs: Lessons from Latin America." *The World Bank Research Observer*, 20 (1): 29-55.

Rosenzweig, M., and H. Binswanger. 1993. "Wealth, weather risk and the profitability of agricultural investment." *The Economic Journal*, 103 (416): 56–78

Ruffle, B. and O. Tykocinski. 2000. "The deadweight loss of Christmas: Comment." *American Economic Review*, 90 (1): 319-324.

Sandmo, A. 1971. "On the Theory of the Competitive Firm Under Price Uncertainty." *American Economic Review*, 61 (1): 65-73.

Satapathy, R. 25 Feb 2012. "A Very Poor Show." The Times of India. Online.

Shah, A., S. Nayak and B. Das. 2008. *Remoteness and Chronic Poverty in a Forest Region of Southern Orissa*. Working Paper No. 121. Chronic Poverty Research Centre.

Shogren, J., S. Shin, D. Hayes, and J. Kliebenstein. 1994. "Resolving differences in willingness to pay and willingness to accept." *American Economic Review*, 84 (1): 255-270.

Singh, I., L. Squire, and J. Strauss. 1986. "A survey of agricultural household models: Recent findings and policy implications." *The World Bank Economic Review*, 1 (1): 149-179.

Sinha, S. and M. Lipton. 1999. *Damaging Fluctuations, Risk, and Poverty: A Review*. Background Paper for the World Development Report 2000/2001, Poverty Research Unit at Sussex, Brighton.

Skinner, J. and D. Staiger. 2005. *Technology Adoption From Hybrid Corn to Beta Blockers*. NBER Working Papers 11251. National Bureau of Economic Research.

Skoufias, E., M. Unar, and T. González-Cossío. 2008. *The Impacts of Cash and In Kind Transfers on Consumption and Labor Supply*. World Bank Policy Research Working Paper, No. 4778.

Solnick, S. and D. Hemenway. 1996. "The Deadweight Loss of Christmas: Comment" *American Economic Review*, 86 (5): 1299-1305.

StataCorp. 2009. *Stata 11 Base Reference Manual*. College Station, TX: Stata Press.

Stoneman, P. 1981. "Intra-Firm Diffusion, Bayesian Learning and Profitability." *The Economic Journal,* 91 (362): 375-388.

Tobin, J. 1958. "Estimation of Relationships for Limited Dependent Variables." *Econometrica,* 26: 24-36.

Tobin, J. 1970. "On limiting the domain of inequality." *Journal of Law and Economics*, 13: 263-277.

Verbeek, M. 2008. *A Guide to Modern Econometrics,* 3rd ed. Chichester, West Sussex: John Wiley and Sons.

Waldfogel, J. 1993. "The deadweight loss of Christmas." *American Economic Review*, 83 (5): 1328-1336.

Wambugu, F. 1999. "Why Africa needs agricultural biotech." *Nature,* 400, July: 15-16.

Whitmore, D. 2002. *What Are Food Stamps Worth?* Working Paper No. 468. Princeton University Industrial Relations Section.

Wooldridge, J. 2002. *Econometric Analysis of Cross Section and Panel Data*. MIT Press.

Wooldridge, J. 2006. *Introductory Econometrics: A Modern Approach.* Mason, Ohio: Thompson South-Western.

World Bank. 2008. *Agriculture and Poverty Reduction*. Policy brief, World Development Report.

Zimmermann, R. and M. Qaim. 2002. *Projecting the Benefits of Golden Rice in the Philippines*. ZEF Discussion Papers on Development Policies No. 51, Center for Development Research, Bonn.

Appendix A: Information and Consent Sheet

Farmer Adoption and Valuation of Seed Varieties and Traits, Orissa, India

Investigators:
Investigators are from the Faculty of Agricultural, Life and Environmental Sciences, University of Alberta Henry An, Assistant Professor, University of Alberta, 011-780-492-3915, henry.an@ualberta.ca
Fiona Hossack, Graduate Student, University of Alberta, fhossack@ualberta.ca

We are here from the Alleviating Poverty and Malnutrition Project that the MS Swaminathan Research Foundation is running in partnership with the University of Alberta. We are here today to ask you to participate in an experiment that is a part of that project. This study is aimed at understanding how people adopt and value new seeds. We are asking because we would like to understand more about how you make decisions on what to purchase and plant in your fields.

As part of this study, we would like to ask you to play a game and then answer some questions. This visit will take about two hours, and you will be asked to participate in a game and to fill out a short survey. The information from the game and the survey will only be used for the purpose of this study by researchers at MSSRF and the University of Alberta. The results of the study will be linked to the larger study that you are participating in with MSSRF and the University of Alberta, The Alleviating Poverty and Malnutrition Project.

Benefits and Risks:
The benefit from participating in this study is that the results will allow us to better understand what you want in a new technology. In addition, you will earn a minimum of 50 rupees by being here today.
We do not foresee any risks that may occur by participating in this study.

If you choose to participate, you may withdraw from the study whenever you like, and you will still receive 50 rupees. Also, if you choose not to participate in this study, you can still continue to participate in the Alleviating Poverty and Malnutrition project that you have been involved in already. You do not have to participate in this study to continue to be a part of the larger project.

If you have concerns about your rights as a study participant, you may contact the University of Alberta Research Ethics Office at (780) 492-2615 (this office will accept collect calls). This office has no affiliation with the study participants.

Confidentiality:
If you agree to participate, your answers will be protected. The general information from the study will be public, but the information will not be linked to any one person or household. The study data will be stored by the researchers

in Alberta, Canada on a secure, password-protected computer. We will keep the data for at least 5 years.

Consent:

Please circle your answers:

Do you understand that you have been asked to be in a research study? YES NO

Have you read and received a copy of the attached Information Sheet? YES NO

Do you understand the benefits and risks involved in taking part in this research study? YES NO

Have you had an opportunity to ask questions and discuss this study? YES NO

Do you understand that you can quit taking part in this study at any time? You do not have to say why. YES NO

Has confidentiality been explained to you? YES NO

Do you understand who will be able to see or hear what you say? YES NO

Do you know what the information you gave will be used for? YES NO

Do you know that the information that you provide will be used for a written thesis, conference presentations, and academic publications? YES NO

Do you give us permission to use your data for the purposes specified? YES NO

Do you give us permission to compare your decisions in today's session with decisions you made in other sessions under this research project? YES NO

Participant's Name: (please print) ____

Participant's Signature: ______________________________________Date: _______________

Researcher's Name: (please print) ____

Researcher's Signature: ____Date: ____________

Appendix B: Game Instructions

Game 1

We're going to ask you now to choose one of the six options in front of you. They all offer a 50 percent probability of a high payment and a 50 percent probability of a low payment, but the amount changes with each option. To represent this, we will place two chips in this bag: the red chip represents the high payment, and the white chip the low payment. Whichever chip is drawn will determine the outcome of the option you choose.For instance, if I choose the sixth option: if I draw a red chip, I get 35 rupees. If I draw a white chip, I get 1 rupee. If I choose the second option and a red chip is draw, I get 18 rupees. If I draw a white chip, I get 12 rupees. We will actually pay you for the outcome of this game.

Game 2

Now we're going to ask you to play a different game. Please listen to the following instructions. When I'm done explaining, we will play a practice round, and you can ask any questions you might have.

1. We will describe a seed to you. You will play five rounds of the game with this seed. The first four rounds are practice round. In the fifth round for each seed, you will be paid for your winnings.
2. You will start the game with 40 rupees that you can choose to spend on the seed or not.
3. Every round, you will decide how much you are willing to pay for the seed before you know what the real price of the seed is. Deciding how much you are willing to pay is like sending a trusted friend into town to buy seed for you without knowing exactly how much the price of the seed is in town. You must decide how much money to send with your friend. If your friend has enough money to buy the seed, he will buy it and return home with the seed and any leftover money to you. If your friend does not have enough money to buy the seed, he will not be able to buy it, and will return home with your money, but no seed. When you have decided, you will write the number down on the sheet in front of you.
4. We will draw a price for the seed from this bag of prices. Prices range from Rs. 10 to 20. If you were willing to pay the price that is drawn or higher, you will plant the seed. If you were not willing to pay the price that is drawn, you will not plant the seed.
5. If you planted the seed, we will draw a chip from the bag to see whether your harvestfor that round is good, average, or bad. This is because at the beginning of a round, you don't know exactly how much yield you will get; depending on the weather and other conditions, you may get more or less. A green chip will represent a bad harvest (Rs 0), a black chip an average harvest (Rs20), and a red chip a good harvest (Rs 40).
6. We will switch seed types and start again; in total there are three seeds that

we will offer.

7. We will ask you to answer a short survey and collect your payment. Everybody will earn at least Rs 40 today, but you have the chance to earn more depending on the decisions you make and your harvests.

We will now play a practice round, so you will learn more about how the game works. Before we do, do you have any questions?

PRACTICE ROUND

We will now play a practice round of the game. You will not be paid for this round.

PRACTICE ROUND

We would like to offer you a chance to buy the practice seed. It offers:

- A 2 in 6 chance of bad harvest and a low payoff, equal to 0 rupees.
- A 2 in 6 chance of average harvest and an average payoff, equal to 20 rupees.
- A 2 in 6 chance of good harvest and a high payoff, equal to 40 rupees.

To reflect this, we will put two bad harvest green chips into the bag (worth Rs 0), along with two average harvest black chips (worth Rs 20), and two good harvest red chips (worth Rs 40).

THE GAME

DESCRIPTION OF SEED A

We will now begin the game. You will be paid for every fifth round.

We would like to offer you a chance to buy seed A in the next five rounds. Seed A offers:

- A 3 in 10 chance of bad harvest and a low payoff, equal to 0 rupees
- A 4 in 10 chance of average harvest and an average payoff, equal to 20 rupees
- A 3 in 10 chance of good harvest and a high payoff, equal to 40 rupees.

To reflect this, we will put three bad harvest green chips into the bag (worth Rs 0), along with four average harvest black chips (worth Rs 20), and three good harvest red chips (worth Rs 40).

DESCRIPTION OF SEED B

We would like to offer you a chance to buy seed B in the next five rounds. Seed B offers:

- A 1 in 10 chance of bad harvest and a low payoff, equal to 0 rupees
- An 8 in 10 chance of average harvest and an average payoff, equal to 20 rupees
- A 1 in 10 chance of good harvest and a high payoff, equal to 40 rupees.

To reflect this, we will put one bad harvest green chip into the bag (worth Rs 0), along with eight average harvest black chips (worth Rs20), and one good harvest red chip (worth Rs40).

DESCRIPTION OF SEED C

We would like to offer you a chance to buy seed C in the next five rounds. Seed C offers:

- A 5 in 10 chance of bad harvest and a low payoff, equal to 0 rupees
- A 0 in 10 chance (no chance) of average harvest and an average payoff, equal to 20 rupees
- A 5 in 10 chance of good harvest and a high payoff, equal to 40 rupees.

To reflect this, we will put five bad harvest green chips into the bag (worth Rs 0), no average harvest black chips (worth Rs20), and five good harvest red chips (worth Rs40).

Appendix C: Game Results Sheet

Participant ID ___________________ Group Cash ABC ACB

Game 1	Option			Black ●		White ○	
Game 2	Seed	Round	WTP	Adopt?	Draw	Price	Net Profit
		1					
		2					
	A	3					
		4					
		5					
		1					
		2					
	_____	3					
		4					
		5					
		1					
		2					
	_____	3					
		4					
		5					

Table C-1: Response sheet used by the enumerators to record participants' responses during game play.

Appendix D: Survey

1. Name

____

2. Panchayat____

3. Revenue Village ____

4. Hamlet____

5. Enumerator code [ ]

6. Date dd/mm/yy [ ]

7. Gender (1=female; 0=male) code [ ]

8. Age years [ ]

9. Education code [ ]

1=	*5=*
2=	*6=*
3=	*7=*
4=	

10. How many children live in your household? number [ ]

11. How many adults live in your household? number [ ]

12. Land holdings (acres):

Total	Own land	Lease out	Lease in

13. Do you grow improved varieties of any crop right now? (1=Y; 0=N)

Code [ ]

a. If no, have you ever in the past grown improved varieties? (1=Y; 0=N)

Code [ ]

14. Are you the primary decision maker for what to plant on your farm? (1=Y; 0=N)

Code [ ]

15. What is the main crop you grow by land area? Code [ ]

1=Rice	*3=Millets*
2=Maize	*4=Other*

16. What is the most important crop you grow for income generation?
Code [ ]

1=Rice	*3=Millets*
2=Maize	*4=Other*

17. Rank the following farm management goals in terms of importance:

GOAL	ORDER
Increase yield	
Stabilize yield from season to season	
Protect against crop losses	
Lower production costs	
Increase harvest quality	
Grow varieties that use fewer inputs (ie. fertilizer)	

18. How well did you understand the experiment? Code [ ]

1=Well	*2=Somewhat*	*3=Not well*

19. Which of the following best describes you? Code [ ]

Relative to other farmers:

1 = I tend to take on substantial levels of risk in my farming.
2 = I tend to avoid risk when possible in my farming.
3 = I neither seek nor avoid risk in my farming.

20. I am at risk of drought. Code [ ]

1=Most of the time	*2=Sometimes*	*3=Rarely*

21. I am at risk of pests destroying my crops. Code [ ]

1=Most of the time	*2=Sometimes*	*3=Rarely*

22. I am at risk of changes in the price paid to me for my crop.
Code [ ]

1=Most of the time	*2=Sometimes*	*3=Rarely*