D.PHIL. THESIS IN ECONOMICS

Essays in Limitations to Technology Adoption

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ABSTRACT

While new agricultural technologies may lead to substantial yield improvements, the take-up rates in developing countries have frequently been low. There are many possible reasons why a farmer might refrain from adopting a new technology, and literature has pointed to several possible reasons in different settings. A key area for research is to understand what policies could encourage higher adoption rates. This thesis studies the research question by using a case study of fertiliser adoption in cocoa farming in Ghana.

Chapter I investigates whether returns to fertiliser in cocoa farming are high and whether farmers' adoption decisions can be explained by comparative advantage. Chapter I uses data from Ghana to measure the returns to fertiliser using a correlated random model and static and dynamic panel models of homogeneous returns to fertiliser. The estimated returns in different models are positive, high and strongly significant statistically. The chapter also presents a correlated random effects model of heterogeneous technology, which allows for farmer-specific comparative advantage. The effect of the comparative advantage is found not to be statistically significant.

Chapter II explores the fertiliser investment decisions and risk preferences of Ghanaian cocoa farmers in a framed field experiment. The experimental subjects decided whether to invest in fertiliser, and the fertiliser return depended on a stochastic weather realisation. An inexpensive index insurance scheme with a positive level of basis risk was found to have a minor positive effect on the fertiliser take-up, but this effect was statistically insignificant. An expensive index insurance scheme with no basis risk was found to have a substantial positive effect, and this effect was strongly significant. The experimental findings suggest that farmers are willing to pay for an index insurance if it successfully shields them from income variability.

Chapter III investigates the effect of trust and of an ambiguous environment on fertiliser investments under index insurance. These two behavioural factors were studied by means of a framed field experiment conducted with Ghanaian cocoa farmers. The subjects had an option to invest in a package of fertiliser bundled with index insurance with a positive level of basis risk. The returns depended both on the subjects´ investment choices and a stochastic weather realization. The key ingredient of the study was that for different subjects, the nature of the basis risk was framed differently. Substantially fewer subjects adopted fertiliser when possible losses of fertiliser investment were framed as resulting from the insurer´s failure to meet its contract obligations, compared with an alternative in which the losses were framed as resulting from a mismatch between their own weather realizations and those on which the index insurance was based. A large negative effect on fertiliser investments was also found in treatments with either a small or large ambiguity regarding the exact level of basis risk. Both negative treatment effects were strongly significant. This may suggest that technologies with which farmers are relatively more experienced are more likely to be adopted under index insurance schemes. The overall experimental findings provide evidence that trust and ambiguity may be significant factors other than basis risk, limiting the effectiveness of index insurance in promoting agricultural innovation.

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Thesis Introduction

This thesis is motivated by solving the puzzle of low aggregate adoption rates of yield-improving agricultural technologies in developing countries. Agriculture is a key economic activity in many developing countries, generating employment and income for the vast majority of their populations. Improvements in agriculture are considered a precondition for the alleviation of poverty and for achieving sustainable economic development (Gollin et al., 2002). Nevertheless, the adoption rates of technological innovations in agriculture are still very low in Africa (Gollin, et al., 2016). This thesis provides new evidence on the possible causes of the low take-up of new agricultural technologies. In addition to this contribution, this thesis investigates how innovative index insurance may encourage agricultural innovation.

This thesis addresses the problem of the low take-up of agricultural technologies in developing countries, using the case of fertiliser adoption decisions among cocoa farmers in Ghana. While cocoa is a key crop cultivated in Ghana, the productivity rates are rather low (Teal and Vignieri, 2004). This may be due to many farmers refraining from using fertiliser, one of the key yield-improving technologies in cocoa. Moreover, the initial adoption of fertiliser may not be sustained in subsequent seasons (Teal et al., 2010). A better understanding of factors, which influence fertiliser adoption in cocoa farming in Ghana could provide important general policy-relevant insights. Low rates of agricultural innovation and substantial switching across seasons is documented both in other countries, for example, with the use of fertiliser in Ethiopia (Dercon and Christiaensen, 2011), and in the context of other technologies, for example, the adoption of hybrid maize seeds in Kenya (Suri, 2011).

Chapter I studies whether fertiliser returns are positive, and high in cocoa farming in Ghana. This is an important question, since the adoption of new technologies may not always be profitable for farmers (Duflo et al, 2011). In order to identify returns to fertiliser use, Chapter I uses different specifications of the underlying production functions. Fertiliser returns may not be identified if a production function is misspecified (Eberhardt and Helmers, 2016). The empirical results in Chapter I are based on production functions that assume either heterogeneous returns (a correlated random coefficient model, henceforth a CRC model) or homogeneous returns (static and dynamic panel estimators). Irrespective of the choice of the model, the predicted returns to fertiliser are found to be positive and high.

Chapter I also investigates whether differing comparative advantages across farmers can explain the low take-up rates of fertiliser in the studied sample. A Correlated Random Coefficient model (henceforth CRC model) (Suri, 2011) is used to identify the heterogeneous comparative advantage, and to analyse the fertiliser adoption patterns in Ghana. In this, the thesis follows Suri (2011) who introduced the CRC model in the context of technology adoption. Suri studied the adoption of maize hybrid seeds in Kenya. Chapter I of this thesis provides new evidence on the importance of the comparative advantage in the adoption of new technologies. Suri's approach represented a significant departure from previous studies of technology adoption, almost all of which assume homogeneous returns to new technology (Hoff, Braverman and Stiglitz, 1993; Temple and Woßmann, 2006; Cordoba and Ripoll, 2009). These returns are also assumed to be initially unknown to farmers, and only learned over time. In contrast, the novel approach by Suri (2011) assumes that returns to new technology may be both known and heterogeneous. This heterogeneity in returns is determined in Suri´s CRC model by a farmer-specific comparative advantage. The model predicts that a farmer will not adopt a new technology if his comparative advantage results in low returns. Suri's (2011) empirical results are in line with these theoretical predictions. There is statistical evidence that the adoption of hybrid maize in Kenya may be driven by the heterogeneity in comparative advantage.

The empirical results in Chapter I use the methods developed in Suri (2011), but apply them to a different case. The results also differ from Suri's. The analysis in Chapter I is based on the five-wave panel data set of Ghanaian cocoa farmers conducted biannually between 2002 and 2010 by the Centre for the Studies of African Economies. The data suggest that comparative advantage may not explain the fertiliser adoption choices in Ghana.

Chapters II and III of the thesis explore the potential of production and income risk to discourage technology adoption. It is well understood that a risk-averse farmer may not adopt a yield-improving technology due to increased income uncertainty. Even if the new investment results in a higher income in expectation, the income variability may also be higher. Under a large negative weather shock, the new technology may be ineffective in raising yields. However, the investment cost still has to be incurred by the farmer. This can particularly reduce consumption levels of the poorest households (Dercon and Christiaensen, 2011). However, farmers may be willing to pay an insurance premium, if the effect of a drop in income due to bad weather could be mitigated.

If insurance reduced the income variability, it could encourage the adoption of yield-improving technologies. However, indemnity insurance schemes suffer from the problems of adverse selection and moral hazard. Adverse selection occurs when farmers who are more likely to experience low harvests are also more willing to buy insurance. The moral hazard occurs when insured farmers have weaker incentives to aim for a high harvest on their farms. In contrast to indemnity insurance, a number of development institutions have explored as an alternative the use of index insurance schemes that address the problems of moral hazard and adverse selection. Payouts under index insurance schemes are determined by a particular criterion (i.e. the index), typically determined by observable weather or agronomic conditions at a district level. This solves the moral hazard problem in the sense that the actions of an individual farmer can no longer influence whether he receives the insurance payout. Furthermore, since insurance is indexed at a weather condition known both to insurer and to farmers, index insurance also addresses the problem of adverse selection.

Index insurance schemes can significantly reduce the income risks associated with the technological innovation (Clarke and Dercon, 2016). However, a potential drawback of an index insurance scheme is basis risk. Basis risk occurs when the index is not perfectly correlated with the agricultural weather conditions faced by an individual farmer. For example, a farmer can experience a bad idiosyncratic weather shock when the general agricultural conditions are favourable. The farmer would receive no compensation since the index is not triggered. Under the basis risk scenario, the drop in the farmer's income is in fact larger relative to an uninsured farmer. This is due to the fact that the insured farmer receives no compensation for a failed investment and still pays an insurance premium (Clarke, 2016).

Over the past several years, numerous development agencies have explored the use of index insurance as a way to reduce the risk facing farmers in developing countries (Karlan et al., 2014; Cole et al., 2013) and until quite recently, basis risk has been viewed as a relatively inconsequential problem or an academic curiosity, However, take-up rates for index insurance schemes in developing countries have been disappointingly low (Gine and Yang, 2009). A number of recent field experiments study factors, which may explain this low demand. Farmers may be less willing to purchase an index insurance if premium rates are high or if an alternative investment option has an implicit insurance scheme

(Gine and Yang, 2009). Index insurance purchases may also depend on the level of basis risk or on the level trust in the insurer (Cole et al., 2013). In addition, the demand for insurance may be influenced by the past history of insurance claims received both by an individual farmer (Cai et al., 2009) and by other farmers within his social network (Karlan et al., 2014).

This chapter explores the demand for index insurance – and the related demand for fertiliser – using framed field experiments (FFEs) rather than a randomised controlled trial (RCT), which is the standard type of field experiment. From a research purpose, FFEs and field experiments are complementary (Harrison and List, 2004). One of the advantages of FFEs in analysing basis risk is the possibility of identifying the objective probability of the basis risk. This is extremely difficult in field experiments (Clarke, 2011). Moreover, by introducing a treatment variation in a highly controllable environment, FFEs can study in isolation the treatment effect of a given variable of interest. An additional benefit of a FFE is the possibility to study experimental treatments that could not be implemented in field experiments for ethical reasons. The empirical findings in Chapter II and Chapter III are based on two FFEs conducted with Ghanaian cocoa farmers. The chapters provide new evidence and are complementary to the field experiments, which study the limitations of technology adoption.

The impact of index insurance on farming investment decisions is still understudied in the highly controllable setting of FFEs. The two recent notable exceptions are Carter and Galarza (2011) and Hill and Viceisza (2012). While the former experiment found that index insurance strongly encourages the adoption of new technologies, the effects of mandated insurance found in the latter study are substantially more moderate. The experimental design in Chapter II of this thesis introduces some variation in the type of index insurance available.

The main contribution of Chapter II is the study of fertiliser adoption decisions, when index insurance schemes vary in terms of the level of basis risk and of the associated premium rates.

Chapter III investigates the demand for index insurance in the presence of two behavioural factors, betrayal aversion and ambiguity aversion. Betrayal aversion occurs if a subject´s utility is affected more negatively by losses generated by an action of another subject rather than by identical losses due to a random event (Bohnet et al., 2008). Ambiguity aversion occurs if a subject prefers a known risk relative to an unknown risk. Neither betrayal aversion nor ambiguity aversion is present in expected utility theory.

An increasing number of field experiments study behavioural factors in the context of technology adoption (e.g. time-inconsistent preferences as noted by Duflo, et al., (2009), or ambiguity aversion, in Bryan (2014)). If a farmer is betrayal-averse, his decision to purchase insurance may be influenced by the source of potential losses (i.e. the basis risk or an insurer's violation of the contract). Similarly, if a farmer is ambiguity-averse, his decision to purchase insurance may depend on the uncertainty regarding the level of basis risk (i.e. the probability of the basis risk is unknown, and hence ambiguous).

Studying betrayal aversion and ambiguity aversion is challenging in the field. For betrayal aversion, there are obvious ethical problems associated with interventions that involve exposing a subject to a loss through deceit. Similarly, calibrating a treatment that allows study of ambiguity aversion may be very difficult in a field environment. In contrast, FFEs can successfully introduce these experimental treatments. Chapter III provides new evidence on the importance of betrayal aversion and ambiguity aversion in the context of the take-up of new agricultural technologies.

Contents

1 Chapter I

1.1 Introduction

Increasing agricultural productivity may play a crucial role in economic growth and poverty alleviation (World Bank, 2008; Cervantes-Godoy and Dewbre, 2010). While a number of new yield-improving crops have been developed since the beginning of the Green Revolution in the 1960s (Evenson and Gollin, 2003), yield increases in Sub-Saharan Africa (SSA) have been very moderate (Gollin, Hansen and Wingender, 2016). Why aggregate adoption rates of new technologies are still very low in SSA is still an important unresolved question (Griliches, 1957, Bryan, 2014).

There is a vast literature outlining the possible obstacles to agricultural innovation (Foster and Rosenzweig, 2010; Conley and Udry, 2010). New technologies can be risky (Zilberman,1983) and may need to be learned over time (Foster and Rosenzweig,1995). Imperfections in credit and insurance markets can also pose barriers to agricultural innovation (Hoff et al., 1993; Dercon and Christiaensen, 2011). Moreover, a farmer may not invest in a technology that generates idiosyncratic returns, even if high yields have been demonstrated on experimental farms with optimal farming conditions (Duflo et al., 2009; Teal et al., 2010).

Suri (2011) proposes that farmer-specific differences in comparative advantage may lead to heterogeneous returns to technology and therefore determine whether a farmer adopts a new technology. Suri (2011) uses a sample of Kenyan maize farmers to estimate a correlated random coefficient (CRC) model (Heckman and Vytlacil, 1998), and she finds evidence that the adoption of hybrid maize seeds is driven by heterogeneity in comparative advantage. Yield improvements to agricultural innovation may be technology-specific, context-specific and heterogeneous across farmers (Mundlak et al., 2012). Therefore, identification of returns to a given new technology by means of estimating agricultural production functions is important in assessing potential benefits for farmers from agricultural innovation.

Identification of production functions may be difficult due to potential problem of transmission bias. This happens if econometrician does not observe idiosyncratic productivity shocks that are correlated with production inputs choices in the sample (Griliches and Maitresse, 1997). In order to address the transmission bias two main empirical approaches have been developed in recent years, namely dynamic panel estimators and structural models (Gandhi et al., 2011). However, much of the literature on production function estimation tend to consider a single empirical approach and assume its validity, without investigating whether an alternative approach may be more suitable in the given context (Eberhardt and Helmers, 2010). Taking into consideration distinct models of production functions may provide valuable insights into identification of returns to production functions.

This paper considers nested models of agricultural production functions in order to estimate returns to fertiliser in cocoa farming in Ghana. Our distinct empirical specifications enable us to test the following two hypotheses: (i) Are returns to fertiliser positive? (ii) Can comparative advantage explain the low adoption rates of fertiliser? These research questions are explored by means of a five-wave panel dataset of cocoa farmers in Ghana, conducted between 2002 and 2010 by the Centre for the Study of African Economies (CSAE) in collaboration with the Ghana Cocoa Board.

This paper contributes to the literature on identification of agricultural production functions. The novelty of this paper is the description of how the novel CRC model with a structural assumption of comparative advantage (Suri, 2011) is nested into the general framework of agricultural production functions, and the identification of distinct econometric models: a standard static panel model, an increasingly dominant dynamic panel model (Arellano and Bond, 1991; Blundell and Bond, 1998), and a CRC model. These different estimation methods are important, since imposing particular structural or parametric assumptions a priori may not lead to the identification of production functions (Eberhardt and Helmers, 2016). For instance, while standard production functions assume homogeneous returns to new technology, this specification is not correct under heterogeneity in returns across farmers (Mundlak at al., 2012). If the production function is not identified, fertiliser returns are not identified either.

This paper also provides new empirical evidence on the importance of heterogeneous comparative advantage in the context of technology adoption. We use the CRC model to investigate whether comparative advantage can explain fertiliser adoption patterns in the context of cocoa farming in Ghana. Cocoa is tree crop, and in Ghana it is primarily cultivated for exports. Estimations of the CRC model in Suri (2011) are based on a distinct technology, crop and country. Specifically, Suri studies adoption of hybrid seeds in maize farming in Kenya. Maize is staple, and in Kenya it is primarily grown for subsistence.

We conclude that the impact of fertiliser on yields is positive and high in the context of cocoa farming in Ghana. This result is confirmed using several distinct estimation strategies. However, we recognise that due to data limitations we do not evaluate the impact of fertiliser on farmers' profits.

We do not find conclusive evidence that fertiliser adoption decisions in our sample can be explained by heterogeneity in returns due to comparative advantage. This departure from the result of Suri (2011) may simply reflect differences in the context (i.e. adoption of fertiliser in cocoa farming in Ghana as opposed to adoption of hybrid seeds in maize farming in Kenya). Our study indicates that comparative advantage may not be key in understanding technology adoption in all contexts. We do not exclude a possibility that comparative advantage is relevant also in our sample, but we are not able to find evidence for it in any of the empirical specifications considered in this paper. Our negative result suggests a need for further examination of the constraints to adoption of well-adapted technologies; we should not always assume that patterns of adoption and non-adoption reflect underlying comparative advantages.

The remainder of this paper is structured as follows: Section 1.2 reviews the relevant literature. The key descriptive statistics are discussed in Section 1.3. Section 1.4 presents nested models of agricultural production functions considered in this paper, and states the hypotheses Section 1.5 discusses the potential endogeneity problems that arise in estimating different models. Section 1.6 presents our two research hypotheses and discusses the empirical findings. Section 1.7 provides the conclusion and suggests several areas for future research.

1.2 Literature Review

The literature suggests several explanations for low adoption of new agricultural technologies in developing countries. A farmer's decision whether to invest in a new technology may be influenced by a number of factors, such as farm size, learning costs, market imperfections and profitability (Udry, 2010).

Farm size is one of the fundamental factors that can determine technological adoption (Griliches, 1957; David, 1978). David (1978) argues that there is a minimum size of farm required to cover fixed costs of acquiring new technology. Investment in innovation often involves high initial expenditure related to accessing new inputs. It may also require devoting time to learning about the correct use of the new technology (Banerjee, et al, 2010). Due to the higher area of cultivated land, larger farms can enjoy more yield improvements (Banerjee, et al., 2010). These farms may therefore be more willing to incur the fixed learning costs of the technology innovation.[1]

Some farmers may decide not to experiment with a technology that is only recently introduced in a region. Griliches (1957) suggests an S-shaped pattern of technology diffusion.[2] Foster and Rosenzweig (1995) provide empirical evidence for the S-shaped pattern of technology diffusion in the context of the adoption of high-yield varieties of seeds during the Green Revolution in India. They also note that the learning process may be slow due to a possible free-riding problem.[3] Conley and Udry (2010) find that pineapple farmers in Ghana may be highly influenced by the new information from early adopters regarding the correct application of the new technology.

One of the crucial market failures in developing countries is credit rationing (Hoff, et al, 1993). The lack of credit can hinder potentially profitable new agricultural investments. (Croppenstedt et al., 2003). In a sample of farmers in India, Bhalla (1978) finds that lack of credit is the major limitation to fertiliser adoption for 48% of small farmers. [4] Beaman et al. (2012) provide evidence that farmers who gain access to loans experience substantial improvements in returns from fertiliser investments.

While acknowledging that credit constraints can play a role in limiting technological adoption, Dercon and Christiaensen (2011) emphasise that a lack of insurance markets may be an equally important obstacle.[5] Under extreme weather

[1] However, several studies argue that technology adoption may not necessarily be positively correlated with farm size. As pointed by Olmstead and Rhode (1993) and Greene (1973) , smaller farms may cooperate and jointly purchase a new equipment. Moreover, smaller farms often use more low-cost family labour relative to larger farms. Several empirical studies find inverse relationship between farm size and productivity (e.g. Feder, 1980).

[2] The process of learning about a new technology is often initiated by a small proportion of farmers (Griliches, 1957). A large proportion of farmers subsequently follows the early adopters, and only a small number of farmers adopt the technology relatively late.

[3] Farmers may strategically postpone their adoption until a successful method of cultivating is developed by early adopters.

[4] However, Dufto et al. (2009) note that credit constraints may not explain the complete lack of adoption of yield-improving technology among poorer farmers. The fertiliser cost is small and, due to the fact that fertiliser innovation is scale neutral, it could be implemented at least on part of the farm.

[5] The point emphasised by Dercon and Christiaensen (2011) is that fertiliser application

conditions (e.g. a drought or torrential rain), fertiliser investments can generate negative returns. In absence of insurance markets, risk-averse farmers may decide not to invest in new technology, if it is in expectation more profitable but also more risky (Karlan et al., 2014).[6]

While the adoption of technologies such as fertiliser tends to generate higher average yields, the variability of yields may also be higher (Suri, 2011; Teal et al, 2010). Recent empirical studies emphasise that returns to agricultural technologies may be heterogeneous (Mundlak et al, 2012; Eberhardt and Teal, 2010; Eberhardt and Vollrath, 2016). While a number of farmers may enjoy high profits from investments in new technologies, a significant proportion of adopters may also experience negative returns. In a study of Kenyan maize farmers, Dufto et al. (2008) estimate that returns to fertiliser are positive only 56% of the time. Several studies also note that a large number of farmers may switch in and out of new technologies from season to season, and this may be explained by heterogeneity in returns (Dercon and Christiansen, 2011; Suri, 2011; Teal et al, 2010).

Suri investigates the impact of heterogeneity on the adoption patterns of yield-improving hybrid maize seeds in Kenya between 1997 and 2004 and takes a novel approach in her theoretical modelling (Suri, 2011). Rather than following the majority of learning models, where unknown homogeneous costs and benefits of technology are learned by the individuals, Suri assumes that farmers differ in their returns to technology adoption and these returns are known to individuals. The key empirical analysis of the paper is to investigate whether this resulting heterogeneity in comparative advantage (relative productivity in

may have hardly any effect on raising yields under extreme weather conditions. This can prevent farmer from covering the incurred investment and thus result in the negative overall returns to fertiliser.

[6]However, Krause et al. (1990) note that credit constraints may be relaxed by the provision of subsidies, but this may not necessarily lead to investments in new technology. Credit availability can, in fact, increase the risk of such investment due to additional financial obligations.

hybrid seeds over non-hybrid seeds for a given farmer) influences farmers´ decisions to adopt yield-improving technology. After recovering the unobserved comparative advantage coefficient in the yields function of the correlated random effects model, Suri shows that heterogeneity plays a decisive role in the adoption decision.

Suri (2011) suggests that a farmer's decision not to adopt a new technology may be perfectly rational, driven by a profit-maximisation criterion. Suri emphasises that higher yields do not necessarily imply higher profits. In particular, not all farmers face the same costs for inputs; there is significant variation in farm-specific shadow prices, including opportunity costs of time. The returns from yield-improving technologies may not improve profits for the majority of farmers, once the heterogeneity in comparative advantage is taken into account. Suri concludes that there may be a simple answer to the vexed question of why there is limited adoption of yield-improving agricultural technologies in developing countries. Farmers might make a rational choice not to adopt apparently attractive technology if their individual-specific returns from innovation are negative.

This chapter aims to identify and discuss returns to new agricultural technologies in developing countries, under the differing assumptions of the underlying production functions. Furthermore, this chapter investigates the importance of heterogeneous comparative advantage in the context of technology adoption. The particular case study considered in this paper is a sample of Ghanaian cocoa farmers and the adoption patterns of fertiliser between 2002 and 2010. The paper mimics Suri´s estimation strategy (Suri, 2011) and tests whether the significant impact of heterogeneous comparative advantage on adoption of yield-improving hybrid seeds in Kenya also plays crucial role in case of fertiliser adoption among Ghanaian cocoa farmers. This is followed by the identification of returns to fertiliser by means of several different models of agricultural production functions.

1.3 Data Description

The analysis reported here is based on a dataset from the Ghana Farmers Cocoa Survey (GFCS), which was compiled by the Centre for the Studies of African Economies at Oxford University in conjunction with Ghana's COCOBOD (the parastatal cocoa marketing board). This is a panel dataset consisting of five waves of data from 2002, 2004, 2006, 2008 and 2010. Cocoa farmers were visited after the harvest in November of every second season in these years.

The first survey in 2002 consists of 497 subjects who were identified as cocoa farmers in the 1998/1999 Ghana Living Standards Survey. The sample was collected from 25 villages located across five regions (Ashanti, Brong Ahafo, Western, Central and Eastern) in order to be representative of farmers in the key cocoa-growing regions in Ghana. The majority of the farming households tend to live in one location, enabling relatively easy follow-up interviews. All farmers who could not be re-interviewed in any future round[7] were replaced by individuals randomly chosen from the same village in order to preserve the consistent geographic representation of the dataset.[8]

Table 1 presents the descriptive statistics for the main variables at household level. All variables are measured at farm level.[9] One of the most striking features in Table 1 is very low cocoa yields in all seasons. The average yields range from 250 kg/ha in 2002 to 348 kg/ha in 2008. This is substantially lower than the

[7] For instance, 49 farmers were not re-interviewed in 2004. This was due to death (7 farmers), migration (12 farmers), illness (2 farmers) or other reasons.

[8] Given that our panel dataset is unbalanced and the sample attrition may be non-random, our empirical results may suffer from attrition bias (e.g. if a farmer migrates because of his investment in fertiliser is extremely low, our estimates of fertiliser returns may be biased upward).

[9] Plot-level data are not available for all waves of the panel. Therefore, in our empirical analysis we do not account for a possibility of differential input use across plots of an individual farmer.

Table 1: Main descriptive statistics (by year)

	2002	2004	2006	2008	2010	Total
YIELD (kg/ha)	241	270	289	334	276	281
	(220)	(238)	(242)	(291)	(223)	(244)
INSECTICIDE (dummy)	.86	.93	.74	.82	.79	.83
	(.35)	(.26)	(.44)	(.39)	(.41)	(.38)
COCOA (ha) .	1.5	5.5	6	3.7	3.5	4.2
	(.95)	(6.9)	(7.5)	(6.2)	(5.1)	(6.1)
LABOUR (days)	562	957	627	142	335	542
	(1,190)	(1,365)	(1,391)	(131)	(883)	(1,144)
Any fertilizer used	.085	.45	.4	.34	.46	.35
	(.28)	(.5)	(.49)	(.47)	(.5)	(.48)
Observations	2339					

Main statistic is the mean. Standard deviations in parenthesis.
Variables are at farm-level.

average yields of 765 kg/ha generated in neighbouring Côte d´Ivoire (Faostat, 2010).

Table 2 shows descriptive statistics in every wave for two subsamples, depending on whether an individual applied fertiliser (F) or not (N) in a given wave. In any given year, the yields are higher for the fertiliser adopters. The yield improvements are particularly high in 2002 and 2010 when fertiliser adopters generate over 100 kg/ha higher yields than non-adopters.

Finally, the panel nature of the dataset creates a possibility to investigate patterns of technology adoption beyond merely the aggregate adoption rates. Except for the initial rise in fertiliser adoption, the aggregate adoption oscillated at around 40%, but this masks substantial switching behaviour among farmers. Table 3 considers the history of fertiliser adoption in 2002, 2006 and 2010 and shows that around half of the farmers never adopt fertiliser, while only 1.5% adopt fertiliser in every season under consideration. This implies that, despite stable aggregate adoption of around 40%, approximately 46% of farmers switch between adoption and non-adoption in different seasons.

Table 3 sheds light on an additional difficulty in understanding the adop-

Table 2: Descriptive statistics (by F/N and by year)

	2002	2002	2004	2004	2006	2006	2008	2008	2010	2010	Total	
	N	Y	N	Y	N	Y	N	Y	N	Y		
YIELD (kg/ha)	205	238	291	250	295	279	307	303	396	206	355	282
	(62)	(227)	(260)	(215)	(263)	(218)	(238)	(278)	(308)	(193)	(230)	(245)
INSECTICIDE (dummy)	1	.83	.96	.87	.97	.7	.79	.79	.88	.67	.92	.82
	(0)	(.37)	(.19)	(.34)	(.17)	(.46)	(.41)	(.41)	(.32)	(.47)	(.27)	(.39)
COCOA (ha)	2.2	1.4	1.5	4.2	5.3	5.1	5.4	3.2	4.6	2.9	3.5	3.6
	(2.2)	(.87)	(1)	(5.8)	(5.6)	(5.7)	(5)	(5.2)	(7.8)	(4)	(4.3)	(5)
LABOUR (days)	225	264	284	352	407	312	327	136	156	201	247	260
	(239)	(219)	(236)	(257)	(254)	(248)	(223)	(120)	(151)	(199)	(225)	(229)
Observations	1925											

Main statistic is the mean. Standard deviations in parenthesis.

Variables are at farm-level.

COCOA is cocoa land in full productivity.

LABOUR is total seasonal labour used.

Table 3: Fertiliser aadoption history (2002, 2006 and 2010)

Fertiliser History Profile (2002 2006 2010)	Percentage of the Sample (sample size = 424)
NNN	51.44
FNN	2.26
NFN	17.82
FFN	0.88
NNF	18.19
FNF	0.75
NFF	7.15
FFF	1.51

i.e. NFN means: (no fertiliser in 2002, fertiliser in 2006, no fertiliser in 2010)

tion of agricultural technologies in developing economies. Once a technology is adopted in a given season, a farmer should have incurred the majority of the fixed costs related to training in relation to applying the technology. Hence, his perception of risk associated with the new technology should be revisited after higher yields are generated during harvesting periods. Therefore, it might appear unreasonable not to continue adopting the new technology in the following season. But this depends heavily on the timing of shocks (Suri, 2011; Foster and Rosenzweig, 2010).

In a microeconomic study of Ethiopian cereal farmers, Dercon and Christiaensen (2011) find substantial switching behaviour, and suggest that this might be driven by unexpected and large negative shocks to yields. The investment in fertiliser is sunk and crops will collapse anyway in cases of extreme drought or rainfall. And in the absence of insurance schemes, the negative income shock will directly reduce consumption. In poor agricultural communities this often leads to extreme hunger, and a farmer with particularly low ability to tackle unexpected income changes might be very reluctant to continue investing in fertiliser in subsequent seasons, even if, on average, it guarantees higher yields.

Suri (2011) also finds substantial switching behaviour of 30% in the sample of Kenyan farmers. However, Suri provides a new explanation for this pattern. Rather than directly linking individual changes in adoption decision over time with the level of yields, Suri argues that it is not exogenous shocks to income but exogenous changes in the availability of seeds and fertiliser which drive the switching behaviour. In order to benefit from new technology, a farmer must usually apply it very early in the season, significantly sooner than the harvesting period. If the new technology is very expensive due to limited availability, the farmer might decide not to adopt it, irrespective of the possibility of future exogenous shocks to yields.

1.4 Theoretical Models

This section presents the empirical framework and the nested theoretical models used in this paper to identify returns to fertiliser. We begin by considering a standard static Cobb-Douglas production function of homogeneous returns to fertiliser, and derive the baseline empirical specification. Subsequently, we present two extensions of this baseline model. First, we allow for serial correlation in the error term, and therefore consider a dynamic production function. We then consider a correlated random coefficient (CRC) model, in which comparative advantage in the error term is correlated with fertiliser adoption decision. Finally, we discuss identification of fertiliser returns in each of these empirical specifications.

1.4.1 Baseline model

A standard static Cobb-Douglas production function takes the following form:

$$Y_{it} = (\Pi_{j=1}^{j=k} X_{ijt}^{\alpha_j}) e^{f_{it}\beta} e^{u_{it}} \tag{1}$$

In this specification the production function is allowed to vary across individuals i and across time periods t. Y_{it} denotes i's yields in period t, f_{it} describes whether i adopts fertiliser in period t, X_{ijt} are set of j controls (i.e. land, labour, use of insecticide, cocoa prices). After taking logs, we obtain the baseline empirical specification:

$$y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it} \tag{2}$$

where u_{it} is error term.

In the baseline model we assume $u_{it} = \vartheta_i + \varepsilon_{it}$ (i.e. the composite error u_{it} consists of a time-invariant part ϑ_i and of a time-varying part ε_{it}). In order to enable identification of equation (2), the following mean independence assumption is made.

$$E(u_{it}|f_{i1}, \ldots, f_{iT,} x_{i1}, \ldots, x_{iT}) = 0 \tag{3}$$

Equation (3) is the key identification assumption of the model. It states that the error term u_i has a mean zero conditional on all leads and lags of adoption decisions and all other regressors. If the identification assumption is satisfied, static panel data methods (e.g. POLS, FE, OLS) yield unbiased and consistent estimates of the production function parameters, including our key coefficient β for the returns to fertiliser.[10]

The strict exogeneity of ϑ_i part of the composite error does not appear to be highly restrictive in this empirical context. ϑ_i can be interpreted as time-invariant unobservable farmer-specific characteristics that improve his/her yields irrespective of whether fertiliser is adopted, and hence should not be correlated with the adoption decision (Heckman and Honore (1990)). Moreover, the data includes controls for time-invariant farmer-specific characteristics (i.e. age, level of education, household size) that could in principle influence yields and also be correlated with regressors.[11]

The strict exogeneity of the ε_{it} part of the composite error is more restrictive, due to the fact that there may be unobserved transitory shocks to yields that may also be correlated with the fertiliser adoption decision or with the choice of production inputs. Violation of the mean independence assumption would result in endogeneity, and prevent the identification of desired parameters. One of the key exogenous time-varying shocks is the producer cocoa price. This shock can substantially affect returns, and, therefore, influence the adoption decision or the choice of production inputs.

There may be other unobservable transitory shocks to yields, such as labour availability, pests occurrence and tree disease, or rainfall shocks, that also are correlated with regressors. The identification assumption will be violated if $E[\varepsilon_{it}|f_{it}, x_{it}] \neq 0$. For instance, a reduction of household labour due to a death

[10] Our dependent variable y_{it} are yields. While a regression model in which profits are the dependent variable would be an interesting extension of our empirical analysis, this is not considered in this paper. Precise estimations of profits are not feasible due to the absence of crucial information in our dataset (e.g. monetary cost of fertiliser, distance to a fertiliser distribution centre, cost difference between household labour and hired labour). We therefore follow Suri (2011), and assume that profits are primarily determined by yields.

[11] There may be other farmer's characteristics that are unobservable, such as soil quality, general farming skills, and risk preferences, but as long as they do not vary across time, equation (2) is still identified in a fixed effects model.

or a migration might influence the fertiliser adoption decision or the amount of hired labour. An infection of a tree with Black Pod disease might reduce the number of trees used in order to contain the disease outbreak. The impact of a large rainfall shock on the land use may depend on the farm-specific distance to river (and hence the risk of flooding). While these shocks are not accounted for in the data, all regressions control for a fundamental time-varying exogenous shock of cocoa prices, which is not part of the error term.

1.4.2 Baseline model with serial correlation in the error term

We now extend the production function model in equation (2) by allowing for serial correlation in the error term as follows.

$$y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it} \tag{4}$$

where $u_{it} = \vartheta_i + \epsilon_{it}$ and $\epsilon_{it} = f\ (\epsilon_{i(t-s)}) + \varsigma_{it}$ ($f\ (\cdot)$ is some function of past productivity and ς_{it} is a white noise).

Serial correlation in ϵ_{it} (the transitory part of the composite error termε_{it}) means that the impact of a productivity shock ϵ_{it} is not confined to a single period. Since there exists a past period (t-s) such that $E(\epsilon_{it}\epsilon_{i(t-s)}) \neq 0$, the mean independence assumption in equation (3) is violated. Therefore, estimates of production function coefficients obtained from static panel regression models (e.g POLS, RE and FE estimators) are biased and inconsistent.

In order to enable identification of equation (4) we explicitly model the persistence of data. We proceed by considering a dynamic production function with a particular form of serial correlation, and estimate equation (4) by means of GMM methods. We follow Blundell and Bond (1998) by first assuming the data generating process as follows.

$$y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it}$$

where $u_{it} = \vartheta_i + \epsilon_{it}$ and $\epsilon_{it} = \rho(\epsilon_{it-1}) + \xi_{it}$, $|\rho| < 1, \xi_{it} \sim MA(0)$

This AR (1) specification of the model (i.e. a first-order autoregressive model) enables us to generate internal instruments that can be used in the GMM estimation. While the first lags of regressors are likely to be correlated with the error term, higher lags could provide valid and informative instruments.[12] For instance, an unobservable shock of rainfall could affect covariates, such as the amount of employed labour in the current cocoa season. Nevertheless, it is less likely that a current rainfall shock would affect labour inputs in previous cocoa seasons. Hence, our lagged regressors offer a promising approach to address endogeneity due to serial correlation in the error term as long as they satisfy both the informativeness and validity conditions.

A crucial advantage of the GMM estimators is the possibility of testing all identification assumptions of production functions (Blundell and Bond, 1998; Eberhardt and Helmers, 2010). While our initial AR (1) specification may be incorrect (and hence equation (4) remains unidentified), the GMM method enables us to test the validity of internal instruments. The initial specification of data persistency may be examined and refined.

1.4.3 Baseline Model with comparative advantage: Correlated Random Coefficient (CRC) model

We now extend the production function model in equation (2) by allowing for correlation between fertiliser adoption decision with unobservable time-invariant part of the error. Specifically, we follow Suri (2011) by estimating the follow-

[12] Validity of instruments is determined by the degree of persistence of the data (i.e. more persistent data would require higher lags of regressors in order to generate valid instruments).

ing key empirical specification of the Correlated Random Coefficient (CRC) model.[13]

$$y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it} \tag{5}$$

where $u_{it} = \vartheta_i + \theta_i + \phi\theta_i f_{it} + \epsilon_{it}$ and θ_i is an unobservable farmer-specific time-invariant comparative advantage.[14]

Term θ_i forms part of the error term in the CRC model, and may be correlated with regressors. Specifically, coefficient ϕ measures correlation between θ_i and f_{it} in equation (5). The fact that $E(\phi f_{it}) \neq 0$ implies that the independence assumption in equation (3) is violated, and estimates of production function parameters obtained by static or dynamic panel data models are biased and inconsistent.

A method to address this endogeneity problem is to recover parameter θ_i by performing a linear projection on all endogenous inputs (Chamberlain (1984). We therefore estimate the following equation.[15]

$$\theta_i = \lambda_0 + \lambda_1 f_{i1} + \lambda_2 f_{i2} + \lambda_3 f_{i1} f_{i2} + v_i \tag{6}$$

After recovering θ_i from the linear projection (equation (6)), the identification problem in the empirical specification in equation (5) is resolved. As a consequence, estimates of production functions coefficients in equation (5),

[13]CRC model is a Roy model of selection based on comparative advantage, and is introduced in the context of returns to education (Chamberlain, 1982 and 1984; Heckman and Vytlacil, 1998). Suri (2011) adapts this theoretical framework to the context of adoption of agricultural technologies. A key structural assumption of the model is that individual-specific comparative advantage θ_i, unmeasured by the econometrician, is correlated with the technology adoption decision.

[14]The derivations of the CRC model are presented in Appendix.

[15]Our endogenous inputs are all histories of adoption decisions ($f_{i1,..f_{iT}}$). The linear projection in equation (6) is more general than in Chamberlain (1984), as it also includes the interaction terms between histories in different periods. This extension addresses the potential endogeneity that might occur if the excluded interaction terms $f_{ij}f_{ik}$ were correlated with comparative advantage θ_i.

including our key estimand β of returns to fertiliser, can now be estimated consistently.[16]

Strengths and weaknesses of the CRC model

The distinctive feature of Suri´s CRC model is the assumption of heterogeneous fertiliser returns which are known to farmers. This is an important contribution to the literature on technology adoption in developing countries.[17] Suri (2011) assumes that the comparative advantage θ_i is known to a farmer i, and that it affects farmer i´s fertiliser adoption decision. Since θ_i is unobservable, the CRC model is only identified after the linear projection $\theta_i = \lambda_0 + \lambda_1 f_{i1} + \lambda_2 f_{i2} + \lambda_3 f_{i1} f_{i2} + v_i$. Subsequently, the distribution of θ_i can be recovered.[18]

If the assumption of heterogeneous comparative advantage is correct, the CRC model still identifies the parameters of interest (i.e. the returns to new technology). In contrast, the static panel models, such as POLS (Pooled Ordinarily Least Squares) or FE (Fixed Effects) models, are both biased and inconsistent.[19] Furthermore, the dynamic panel models will also be biased and inconsistent under heterogeneous comparative advantage.[20][21]

[16] Our empirical approach of estimating the CRC model is equivalent to Suri (2011), and also equivalent to the methodology in Michuda et al., (2017) for the case of two waves of panel data.

[17] The majority of the models studying technological innovation assume homogeneous returns that are learned over time. Suri (2011) presents the first generalised Roy model adapted to the context of the adoption of agricultural technologies in developing countries.

[18] Once the distribution of theθ_i is recovered, the term $\phi\theta_i f_{it}$ is no longer in the error term. Therefore, the subsequent estimation of the equation (5) will no longer suffer from this source of the omitted variable bias.

[19] In the equation (5) the term $\phi\theta_i f_{it}$ is in the error term. This term is correlated with the fertiliser adoption decision f_{it}. Therefore, the estimation methods which do not take this into account (e.g. OLS, FE models) will suffer from the omitted variable bias.

[20] In comparison with static panel models, dynamic panel models address a number of endogeneity problems (i.e. persistent data). However, none of these models is consistent in the presence of heterogeneous comparative advantage.

[21] The sign of ϕ from the CRC model now becomes crucial in determining the direction of the bias in the direct estimation method (in the case where $\phi \neq 0$). Assuming that the CRC model is correctly specified, the true fertiliser effect on yields is $\frac{\partial y_{it}}{\partial f_{:i}} = \beta + \phi\theta_i$. Given that the general output effect of fertiliser is positive, the β coefficient from the direct estimation method would be biased upward if $\phi > 0$. In this case, the direct estimation model fails to pick up the positive effect of comparative advantage on output, and simply assigns the entire

The CRC model by Suri (2011) offers a very useful new approach in identifying returns to fertiliser under an important potential source of endogeneity (i.e. heterogeneous returns due to comparative advantage).[22] Nevertheless, similarly to the structural models in general, the CRC model´s assumptions about the unobserved productivity shocks (a particular form of the comparative advantage θ_i in the error term, and a particular form of correlation of θ_i with the fertiliser adoption decision) are not testable.[23] This may be important since the complexity of the derivations of the CRC model requires several assumptions in the data-generating process. As Suri (2011) notes, it is not possible to identify the relative magnitudes of $\theta_i^F$and θ_i^N in equations (13) and (14). To address this problem, Suri (2011) must introduce the decompositions of $\theta_i^F$and θ_i^N (equations (15) and (16).

The decompositions in the equations (equations (15) and (16) are only possible under the assumption of the log-concave distribution of the comparative advantage θ_i. Moreover, log-concavity is also needed to derive a linear form of the coefficient $\phi\theta_i f_{it}$. The log-concavity assumption introduces a valuable flexibility in the interpretation of the comparative-advantage coefficient ϕ in the CRC model.[24] [25]

The log-concavity assumption leads to several useful advantages mentioned

output effect to the general impact of fertiliser adoption. Conversely, if $\phi < 0$, the direct estimation model would give a downward bias of the yield effect of fertiliser, and again this bias would lead to inconsistency.

[22]This is an important advantage of the structural model approach (such as the CRC model) in this particular context. While both structural models and dynamic panel models attempt to address the endogeneity, it is not guaranteed that the two approaches will be equally successful in solving a particular source of the endogeneity.

[23]Eberhardt and Helmers (2000) emphasise that structural models and dynamic panel (DP) models may be equally suitable model choice a priori. The DP models (e.g. the GMM estimators) enable tests of the assumptions made about the unobserved productivity shocks. These assumptions are not testable in the structural models (e.g. the CRC model).

[24]Coefficientϕ also determines the sorting in the Roy economy of the CRC model: $\phi < 0$ implies less inequality in this economy relative to the random assignment of the technology. $\phi > 0$ implies more inequality in this economy relative to the random assignment of the technology (Suri, 2011).

[25]In the original CRC model by Heckman and Honore (1990), the model imposes $\phi < 0$. Suri´s log-concavity assumption introduces a more flexible CRC model, where $\phi < 0$, $\phi > 0$ or $\phi = 0$.

above. Nevertheless, it is challenging to test the validity of this assumption. Because the recovered distribution of θ_i is discrete in Suri´s model (the estimate of θ_i contains only four mass points[26] [27]) , it is impossible to test whether the distribution is of a particular continuous form (i.e. log-concave distribution in this case).

It is also worth noting that if $\lambda_1 = \lambda_2$, the key structural parameter of interest in the CRC model (the parameter ϕ) is not identifiable. An even more important concern is that, due to the complex nature of the projection $\theta_i = \lambda_0 + \lambda_1 f_{i1} + \lambda_2 f_{i2} + \lambda_3 f_{i1} f_{i2} + v_i$, only a subset of the panel dataset is used in the estimations.[28]

A very important limitation of the CRC model is that the estimated coefficients will be inconsistent if the error terms are serially correlated.[29] Clearly it is impossible to ascertain a priori the correct form of the serial correlation in the unobservables. While the CRC model ignores this problem, the dynamic panel model may assume different forms of the serial correlation (i.e. the baseline model with serial correlation in the error term) and, more importantly, test the validity of each of these assumptions.

[26] In order to recover the θ_i, I follow Suri (2011) in running the linear projection $\theta_i = \lambda_0 + \lambda_1 f_{i1} + \lambda_2 f_{i2} + \lambda_3 f_{i1} f_{i2} + v_i$. Suri (2011) notes that the estimation strategy could extend this projection by more periods and more interactions, but such extension would soon become cumbersome. Moreover, θ_i could also be projected on other regressors, such as on the agricultural inputs).

[27] While introducing more regressors into the projection $\theta_i = \lambda_0 + \lambda_1 f_{i1} + \lambda_2 f_{i2} + \lambda_3 f_{i1} f_{i2} + v_i$ would generate more mass points of the distribution of θ_i (and offer a possibility to verify the log-concavity assumption, the resulting complexity of the CRC model becomes intractable before any such distribution tests could be performed.

[28] The θ_i is projected on all the histories of fertiliser adoption and on their interaction terms: using two periods would generate three regressors (projection $\theta_i = \lambda_0 + \lambda_1 f_{i1} + \lambda_2 f_{i2} + \lambda_3 f_{i1} f_{i2} + v_i$), while using three regressors would result in a substantially more complicated projection with eight regressors. Suri (2011) uses two waves of her panel dataset. The dataset used in this paper consists of five waves but, due to the complexity of the CRC model, only two waves are used in each estimation of the CRC model (however, different wave is used as period 1 in the projection: this enables us to exploit more fully the information in the dataset and to perform some robustness checks).

[29] Serial correlation can for instance be assumed to take the following first-order autoregressive form: $v_{it} = \alpha + \beta v_{it-1} + \epsilon_{it}$, where ϵ_{it} is a mean zero error term.

1.4.4 Generalisation of the models

One could further generalise the production function model presented in this paper by allowing simultaneously for comparative advantage, for learning and for serially correlated error terms. Our three models would be special cases nested in such theoretical framework. A static production function with homogeneous returns would be the simplest case (i.e. the baseline model in this paper). This could be extended by allowing for serial correlation in the error term or by allowing for comparative advantage. Both of these extended models are presented and estimated in this paper (i.e. the dynamic model and the CRC model, respectively).

A further extension of our baseline model could allow both for serially correlated productivity shocks and for comparative advantage. This extension of the model is not considered in this paper due to the additional identification problems posed by the model complexity (i.e. one would need to assume a particular form of serial correlation, and then consider a new structural model of comparative advantage).

Another extension of the simple model could allow both for comparative advantage and for learning. This extension of the model would assume not only that returns to agricultural technology are heterogeneous due to time-invariant comparative advantage, but also that the farmer-specific comparative advantage must be learned over time. Chamberlain (1993) shows that models of learning about comparative advantage may not be identified. The identification problem may be particularly serious in short panel data sets. Gibbons et al. (2002) present and estimate a model of comparative advantage with learning, but the identification requires an explicit modelling of the learning process and is based on a long, 17-wave panel data set.

1.5 Research Hypotheses

In order to test the two research hypotheses, we estimate the empirical specification (2):

$$y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it}$$

where x_{ijt} are the controls (i.e. land, labour, a dummy for insecticide use, and cocoa prices), f_{it} is a dummy for fertiliser use. u_{it} is error term.[30]

Hypothesis I

Fertiliser raises yields in cocoa farming.

We test Hypothesis I by means of three nested models: the baseline model, the baseline model with serial correlation in the error term, and the CRC model. In these models, coefficient β estimates the impact of fertiliser on cocoa yields. In terms of the empirical specification (equation (2)), this implies the following statistical hypothesis:

Under the null hypothesis, $H_0 : \beta = 0$.

This is tested against a one-sided alternative hypothesis, $H_A : \beta > 0$.

Hypothesis II

Comparative advantage plays a role in the fertiliser adoption decision in cocoa farming.

We test Hypothesis II by means of the CRC model. To recap, θ_i in the CRC model is an unobservable, farmer-specific comparative advantage, and, as demonstrated in footnote46 , $\phi = \frac{\sigma_F^2 - \sigma_{FN}}{\sigma_{FN} - \sigma_N^2}$. It forms part of the error term ($u_{it} = \vartheta_i + \theta_i + \phi\theta_i f_{it} + \epsilon_{it}$), and may be correlated with the fertiliser adoption

[30] As discussed in Section 1.4, assumptions placed on the error term will vary across nested models estimated in this paper.

decision. In terms of the empirical specification (2), the comparative advantage plays a role in the fertiliser adoption decision (f_{it}), as long as $\phi \neq 0$. This implies the following statistical hypothesis:

Under the null hypothesis $H_0 : \phi = 0$.

This is tested against a two-sided alternative hypothesis $H_A : \phi \neq 0$.[31]

1.6 Empirical Results

This section presents and discusses our empirical results. We first test Hypothesis I by estimating the baseline model and the baseline model with serial correlation in the error term. Subsequently, we estimate the CRC model which enables us to test both Hypothesis I and Hypothesis II.

1.6.1 Results from the baseline model and from the baseline model with serial correlation in the error term: Hypothesis I

The initial results in Table 4 might suggest that time-invariant unobservables play an enormous role in determining output. RE (random-effects) results estimate that the adoption of fertiliser has a substantial impact on dependent variables by raising cocoa output by approximately 20%. Nevertheless, the Hausman test, which takes account of the correlation between regressors and the time-invariant components of the error term, clearly rejects the efficiency benefits of the RE estimator in favour of the consistent FE (fixed-effects) estimator. The results in FD (first-difference) are very similar to FE, and while the coefficient on the fertiliser dummy is positive, it is not significant at 5% both in FE and FD (first-difference).

A possible extension of the analysis is to investigate the whether the de-

[31] Hypothesis II cannot be tested by means of the baseline model or the baseline model with serial correlation in the error term. These models assume that there is no correlation between the fertiliser adoption decision and comparative advantage, and it would imply $\phi = 0$ in the CRC model.

Table 4: Dynamic models 1 (dep. var. : ln(COCOA))

	(1) POLS	(2) FE	(3) RE	(4) FD	(5) 2SLS (2 inst)	(6) 2SLS (1 inst)
ln(LAND)	0.540***	0.222***	0.411***	0.207***	0.425***	0.387***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ln(LABOR)	0.180***	0.055**	0.129***	0.060**	-0.040	-0.090
	(0.000)	(0.003)	(0.000)	(0.001)	(0.546)	(0.243)
FERT	0.337***	0.059	0.203***	0.007	3.473***	4.278***
	(0.000)	(0.146)	(0.000)	(0.864)	(0.000)	(0.000)
INSECTICIDE	0.227***	0.085	0.155**	0.014	-0.420*	-0.559*
	(0.000)	(0.108)	(0.001)	(0.799)	(0.043)	(0.020)
2004	-0.590***	-0.047	-0.358***	0.000	0.067	0.115
	(0.000)	(0.372)	(0.000)	(.)	(0.709)	(0.582)
2006	-0.315***	0.146*	-0.130*	0.223**	0.277	0.368
	(0.000)	(0.011)	(0.016)	(0.002)	(0.107)	(0.075)
2008	-0.044	0.294***	0.099	0.111	0.463*	0.584**
	(0.555)	(0.000)	(0.083)	(0.219)	(0.010)	(0.006)
2010	-0.259***	0.264***	-0.045	-0.045		
	(0.000)	(0.000)	(0.433)	(0.548)		
Constant	5.395***	6.207***	5.729***	-0.005	5.322***	5.348***
	(0.000)	(0.000)	(0.000)	(0.923)	(0.000)	(0.000)
Observations	1920	1920	1920	1033	1042	1131

robust se are used; p in parentheses. FD is First-difference estimator.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

pendent variable is persistent. Even moderate data persistency would imply that the FE estimator is inconsistent.[32] In contrast, 2SLS remains a potentially reliable approach in moderate persistence levels. However, a highly persistent cocoa output variable would yield a second lag of interaction terms, which is used in 2SLS, not sufficiently long for instrument validity.

Table 5 presents the results of the regressions when a lagged dependent variable is used as an additional regressor. If the coefficient of this regressor is significantly different from zero, the model becomes dynamic and the strict exogeneity assumption required for FE, RE and POLS is violated. FE and FD estimate the negative coefficient on the lagged value of ln(COCOA). This result, significant at 1%, might appear surprising, since it would imply that high output in the previous season reduces output in the current season. However, in the presence of data persistency, FE and FD models are both likely to be subject to large bias and inconsistency, and different estimation strategies should be considered to identify the key parameters of interest.

Dynamic panel models may address the problem of data persistency due to the availability of internal instruments. While the first lags of regressors are likely to still be correlated with the error term, higher lags should provide valid instruments depending on the degree of persistence of the dependent variable. However, the cost of increasing lags for validity is the negative impact on the informativeness of instruments, which can also prevent identification. GMM estimators enable the use of a set of instruments depending on the assumptions

[32]The consistency of FE critically rests on the assumption of strict exogeneity. Identifying a fertiliser effect on output is particularly challenging due to the large potential endogeneity of this dummy variable for technology adoption. FE correctly addresses the endogeneity problem of time-invariant unobservables ϑ_i that might affect both output and adoption decisions. However, in the presence of time-varying unobservables ϵ_{it} that are correlated with a fertiliser dummy, FE estimations would yield biased and inconsistent estimation results. A possible approach to addressing this potential endogeneity due to omitted variable bias is to use valid and informative instruments (e.g. while a rainfall shock in the current season may affect fertiliser adoption choice in the current season, it is likely to affect the fertiliser use in the past).

Table 5: Dynamic Models 2: ln(COCOA) is dependent variable

	(1) Fixed effects	(2) DiffGMM	(3) DiffGMM	(4) SysGMM	(5) SysGMM
L.ln(COCOA)	-0.119***	-0.056	-0.063	0.736***	0.699***
	(0.001)	(0.868)	(0.845)	(0.000)	(0.000)
ln(LAND)	0.048**	0.439**	0.493**	0.073	0.099
	(0.048)	(0.026)	(0.010)	(0.389)	(0.311)
ln(LABOUR)	0.014	-0.021	-0.036	0.172*	0.162*
	(0.554)	(0.867)	(0.764)	(0.065)	(0.066)
FERT	0.040	0.080	0.049	0.493**	0.540***
	(0.447)	(0.800)	(0.864)	(0.016)	(0.005)
INSECTICIDE	-0.033	-0.276	-0.353	-0.093	-0.054
	(0.631)	(0.437)	(0.326)	(0.731)	(0.839)
2004	-0.367***	-0.391	-0.384	-0.080	-0.090
	(0.000)	(0.127)	(0.145)	(0.444)	(0.377)
2006	-0.146**	-0.259	-0.274		
	(0.027)	(0.213)	(0.187)		
2008	-0.036			0.107	0.105
	(0.567)			(0.437)	(0.448)
2010	0.000	-0.033	-0.032	-0.104	-0.090
	(.)	(0.826)	(0.799)	(0.274)	(0.343)
Observations	1251	570	570	1251	1251
Hansen Test		0.805	0.872	0.883	0.899
Diff-in-Hansen Test				0.795	0.899
m1		0.160	0.0965	0.0000482	0.0000416
m2		0.610	0.556	0.391	0.410
N instruments		26	30	41	45

b coefficients; p in parentheses

standard errors clustered at village level, robust to heteroskedasticity and serial correlation

L.ln(COCOA) is the lagged dependent variable

optimal two-step estimator is computed in all GMM regressions

DiffGMM is Difference GMM with lags 2 and 3 (column (2)), lags 2 or above (column (3))

SysGMM is System GMM with lags 2 and 3 (column (4)), lags 2 or above (column (5))

Hansen Test tests for instrumental validity of GMM (valid under H0)

Diff-in-Hansen Test tests for validity of additional instruments in SysGMM (valid under H0)

m1 and m2 are tests for the absence of first and second-order correlation in the residuals

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

made on our five-wave panel dataset[33]. More restrictive assumptions provide additional moment conditions, and the resulting instruments are valid subject to the correctness of the testable assumptions.

The results of Difference GMM (henceforth DiffGMM) reported in column (2) of Table (5) uses second and third lags of first-differenced regressors for the equations in levels and finds a negative effect of first-differenced lagged ln(COCOA) on the dependent variable. However, this result is not statistically significant and, therefore, DiffGMM estimations do not provide sufficient evidence for persistence of the dependent variable.

It should be noted that the results of DiffGMM are often very susceptible to the choice of the instruments and hence may be subject to manipulations. DiffGMM in column (3) uses more instruments by allowing for moment conditions to be derived from regressors from second lags or higher. The results do not change substantially compared to column (2), but it should be noted that in the case of highly persistent data, the instruments generated by DiffGMM might have very weak properties. Persistent data requires higher lags to ensure validity, but higher lags can significantly reduce the informativeness of the instruments. While highly informative, not strictly valid instruments do not provide consistency, and weakly informative valid instruments also pose empirical challenges due to large finite sample bias negatively related with informativeness of instruments.

SysGMM generates additional moment conditions in levels by placing more restrictive assumptions involving the stationarity of initial conditions. Column (4) of Table 5 shows the SysGMM estimations, which use second and third lags for equations in first differences and second lags for equations in levels.

[33]Given the GCFS dataset comprises five waves, the set of available instruments in the GMM model is larger than in the AH 2SLS. The latter model is consistent in the dynamic model under the condition of informative and valid instruments, but is efficient (under heteroskedasticity) only in case of a three-wave panel dataset (Wooldridge, 2012).

The advantage of GMM is that the validity of the assumptions, which generate additional internal instruments, can be tested. The Diff-Hansen test does not reject the validity of the additional instruments used in SysGMM in column (4) (reported p value is 0.795; hence, the null hypothesis of exogenous additional instruments cannot be rejected, even at high levels of significance). The result of this estimation provides strong evidence that the dynamic model is a correct specification, since the coefficient on the lagged dependent variable is 0.736 and statistically significant at 1%. This suggests the persistence of the data, and the coefficient is also substantially lower than unity (implying that the power of the Diff-Hansen test is strong).

Investigating the violation of the stationary assumption in SysGMM becomes challenging, if the coefficient of the lagged dependent variable is close to unity. Importantly, this does not seem to be the case in this dataset. Moreover, SysGMM with second and higher lags used for instruments reported in the sixth column of Table 5 provides more instruments and also suggests that the dynamic model is the correct specification. The lagged dependent variable has the 0.699 coefficient significant at 1% and SysGMM both in column (4) and (5) do not reject the null hypothesis of no serial second-order correlation in the residuals. This suggests that instruments used in both regressions seem to be valid.

It should be noted that using too many instruments might lead to overfitting of the model, eventually resulting in bias. In fact, the instrument count is typically quadratic in the time dimension of the panel; hence, GMM with the additional assumptions is prone to bias due to too many instruments relative to the sample size (Roodman, 2006). A possible solution is to reduce the set of instruments in the GMM estimation. Therefore, SysGMM with second and third lags is the preferred estimated model as it generates moderately overidentified model, trading off less efficiency for a lower risk of bias due to weak instruments.

Apart from the lagged dependent variable, fertiliser use is the key regressor, which is significant at 5%, and has a coefficient value of 0.493.

Fertiliser use increases cocoa output by 49.3% relative to previous periods but, due to the dynamic model specification, the long-run effect of fertiliser on output is even higher and equal to $\frac{0.493}{1-0.736} \approx 1.867$. This implies that the decision to start using fertiliser continuously will eventually raise the cocoa output by approximately 186%. The estimates of SysGMM in column (5) suggest an even higher effect of fertiliser but, due to the robustness of estimation results, a SysGMM estimator in column (4) is chosen as the most accurate one. The order of magnitude of the fertiliser effect predicted by this estimator is very similar to the results of 2SLS and the external instruments in the non-dynamic model in Table 4.

The SysGMM estimator is limited in testing the informativeness of instruments, but large subsets of internal instruments can be tested for validity. This still will generate consistent results provided that the sample size is sufficiently large. 2SLS can easily test the informativeness of instruments, but the validity of instruments in a just-identified model must be trusted on theoretical grounds. However, all GMM estimators in Table 5 conclude that there is no second-order serial correlation in the residuals, enabling the use of second lags as valid instruments. The external instruments used in 2SLS in Table 4 are also lagged twice; hence, the used interaction terms seem to be valid instruments as well. Therefore, both GMM with internal instruments and 2SLS with external instruments provide strong evidence that the effect of fertiliser on output is enormous, and its adoption can double the cocoa output.

1.6.2 Results from the Correlated Random Coefficient (CRC) model: Hypothesis I and Hypothesis II

In order to test Hypothesis I and Hypothesis II by means of the CRC model, we first need to estimate the following linear projections (Chamberlain, 1984; Suri, 2011).[34]

$$y_{i1} = \kappa + \gamma_1 f_{i1} + \lambda_2 f_{i2} + \gamma_2 f_{i1} f_{i2} + \varepsilon_{i1} \tag{7}$$

In this projection:

1. $y_{i1}$is $Output_{i,2004}$ (the log of cocoa output in 2004),

2. f_{i1} is $FERT_{i,2004}$ (a dummy variable taking value 1 if a subject i adopted fertiliser in 2004),

3. f_{i2} is $FERT_{i,2010}$ (a dummy variable taking value 1 if a subject i adopted fertiliser in 2010),

4. $f_{i1}f_{i2}$ is an interaction term between $FERT_{i,2004}$ and $FERT_{i,2010}$,

5.ε_{i1} is a mean zero error term[35].

$$y_{i2} = \kappa + \lambda_1 f_{i1} + \gamma_3 f_{i2} + \gamma_4 f_{i1} f_{i2} + \varepsilon_{i2} \tag{8}$$

In this projection:

1. y_{i2} is $Output_{i,2010}$ (the log of cocoa output in 2010),

2. f_{i1} is $FERT_{i,2004}$ (a dummy variable taking value 1 if a subject i adopted fertiliser in 2004),

3. f_{i2} is $FERT_{i,2010}$ (a dummy variable taking value 1 if a subject i adopted fertiliser in 2010),

[34] Our hypotheses cannot be tested by directly estimating the equation $y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it}$ in the case that $u_{it} = \vartheta_i + \theta_i + \phi\theta_i f_{it} + \epsilon_{it}$. This is due to the fact that θ_i is unobserved and correlated with f_{it} (term $\phi\theta_i f_{it}$).

[35] As in Suri (2011) the first linear projection consists of regressing the log of output in period 1 (year 2004) on fertiliser adoption decisions in period 1 (year 2004) and in period 2 (year 2010) and on the interaction terms.

4. $f_{i1}f_{i2}$ is an interaction term between $FERT_{i,2002}$ and $FERT_{i,2010}$,

5. ε_{i2} is a mean zero error term[36].

Table 6 estimates the linear projections (7) and (8), when the first period is year 2002 and the last period is year 2010[37]. The results from the linear projections in Table 6 enable me to identify all the structural parameters of the CRC model: $\lambda_1, \lambda_2, \lambda_3, \beta, \phi$.[38]

Table 7 shows the values of all the structural parameters of the CRC model: $\lambda_1, \lambda_2, \lambda_3, \beta, \phi$.[39] As in Suri (2011), the results are based on three different specifications of the linear projections (7) and (8): excluding covariates (column (1)), including covariates (column (2)), and including covariates and interaction terms between the covariates and fertiliser dummies (column (3)).

The estimated values of ϕ and of β are of particular interest. These are the recovered parameters in the key empirical specification $y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it}$ which is used for testing Hypothesis I and Hypothesis II.

The CRC Result: Hypothesis I (H_0 : $\beta = 0$; H_A : $\beta > 0$)

The structural parameter β in the CRC model describes the component of the returns to fertiliser,[40] which does not vary across farmers (the term βf_{it} in equation $y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it}$). Based on the results in Table 7, there is some statistical evidence against $H_0 : \beta = 0$. As expected, the sign of the coefficient β is positive in all columns 13. This result is also significant at 1% in column (3) (when covariates and interaction terms are included). This is similar to the result in Suri (2011), who finds a statistically significant positive value of β in

[36] As in Suri (2011) the second linear projection consists of regressing the log of output in period 2 (year 2010) on fertiliser adoption decisions in period 1 (year 2004) and in period 2 (year 2010) and on the interaction terms.

[37] The results based on the year 2002 as the first period are in the Appendix.

[38] As shown in Appendix, the estimates from the linear projections in Table 6 enable me to identify $\phi = \frac{\gamma_4 - \gamma_2}{\lambda_1 - \lambda_2}$.

[39] The first period is year 2004. The results are similar when the first period is year 2002 (Appendix).

[40] The overall returns to fertiliser are equal to $\frac{\partial y_{it}}{\partial f_{it}} = \phi\theta_i + \beta$.

Table 6: CRC Estimations of the linear projections $y_{i1} = \kappa+\gamma_1 f_{i1}+\lambda_2 f_{i2}+\gamma_2 f_{i1} f_{i2}+\varepsilon_{i1}$ and $y_{i2} = \kappa+\lambda_1 f_{i1}+\gamma_3 f_{i2}+\gamma_4 f_{i1} f_{i2}+\varepsilon_{i2}$ (equations (7) and (8)) ; dep. var. : ln(COCOA); years: 2004 and 2010

	(1)		(2)		(3)	
	Output,2004	Output,2010	Output,2004	Output,2010	Output,2004	Output,2010
FERT,2004	0.031	0.759^{**}	0.096	0.034	-0.515	-0.026
	(0.155)	(0.232)	(0.071)	(0.112)	(0.554)	(0.529)
FERT,2010	0.300	0.077	0.175^{*}	-0.273^{*}	-0.864	-0.062
	(0.155)	(0.234)	(0.068)	(0.116)	(0.521)	(0.450)
FERT2004*FERT2010	0.161	-0.307	-0.055	0.177	0.670	1.599
	(0.232)	(0.348)	(0.103)	(0.168)	(0.914)	(0.844)
ln(LAND)			0.825^{***}	0.868^{***}	0.717^{***}	0.852^{***}
			(0.035)	(0.027)	(0.054)	(0.042)
ln(LABOR)			0.118^{***}	0.043	0.187^{***}	0.126^{*}
			(0.023)	(0.034)	(0.038)	(0.057)
Constant	5.053^{***}	5.332^{***}	0.495^{*}	0.356^{*}	1.066^{***}	0.513
	(0.098)	(0.150)	(0.206)	(0.173)	(0.317)	(0.272)
Observations	269	266	245	198	245	198

se in parentheses

all regressions are OLS, robust se are used

(1) excludes covariates in regressions for both periods

(2) includes covariates in regressions for both periods

(3) includes covariates and interaction terms between covariates and dummies for fertilizer history (not reported) in regressions for both periods

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Recovered Values of the Structural Parameters (2004 and 2010)

Parameter	(1)	(2)	(3)
λ_1	0.077	-0.273	-0.062
	(0.743)	(0.019)	(0.891)
λ_2	0.300	0.175*	-0.864
	(0.053)	(0.011)	(0.098)
λ_3	-0.152	0.073	0.074*
	(0.137)	(0.261)	(0.041)
β	1.480	0.858	0.668*
	(0.824)	(0.410)	(0.004)
ϕ	2.092	-0.517	1.158
	(0.682)	(0.169)	(0.887)

b are coefficients, p values in parenthesis
(1) regressions exclude covariates
(2) regressions include covariates
(3) regressions include covariates and interaction terms with adoption dummies
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Estimations of structural parameters are based on results from Table 6.

the sample of maize farmers in Kenya.

The CRC Result: Hypothesis II (H_0 : $\phi = 0$; H_A : $\phi \neq 0$)

The structural parameter ϕ in the CRC model describes the farmer-specific component of the returns to fertiliser in the equation $y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it}$ (the error term is $u_{it} = \vartheta_i + \theta_i + \phi\theta_i f_{it} + \epsilon_{it}$). The overall returns to fertiliser are equal to $\frac{\partial y_{it}}{\partial f_{it}} = \phi\theta_i + \beta$. Therefore, the heterogeneity in returns to fertiliser is determined by the value of ϕ, and by the farmer-specific value of comparative advantage θ_i.

Based on the results in Table 7, the null hypothesis $H_0 : \phi = 0$ cannot be rejected at the 10% significance level. The estimated coefficient on ϕ is is insignificant in all columns (1-3).[41]

This result has several implications. Firstly, failure to reject $H_0 : \phi = 0$ implies that we do not find sufficient statistical evidence in our sample that

[41] When year 2002 is used as the first period, the coefficient ϕ is negative, but always insignificant (Table 9 in Appendix).

heterogeneity in the returns to fertiliser is explained by heterogeneity in comparative advantage θ_i. This result is in contrast to the results in Suri (2011). Suri explains that hybrid returns are relatively lower for the Kenyan farmers with a higher value of θ_i. These farmers are therefore less likely to adopt the hybrid seeds. However, if the heterogeneity in comparative advantage θ_i does not lead to the heterogeneity in returns to technology (this happens if $\phi = 0$, which is the case in our sample), the comparative advantage may not influence the technology adoption decision.[42]

Secondly, failure to reject $H_0 : \phi = 0$ implies that the fertiliser adoption decision f_{it} is not correlated with the individual-specific comparative advantage θ_i. If $\phi = 0$, the farmer-specific comparative advantage does not influence the fertiliser adoption decision (the term $\phi\theta_i f_{it}$ no longer appears in the error term of equation $y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it}$). This implication also contrasts with Suri (2011),

[42] $\frac{\partial y_{it}}{\partial f_{it}} = \phi\theta_i + \beta$. While the CRC model enables me to recover the distribution of θ_i(Appendix), the heterogeneity in the comparative advantage θ_i does not lead to the heterogeneity in returns if $\phi = 0$.

who finds that the farmer-specific comparative advantage in maize farming in Kenya is negatively correlated with the adoption decision of the hybrid seeds. Suri (2011) finds that the Kenyan farmers with a high comparative advantage are less likely to adopt the hybrid seeds. We do not find statistical evidence that the comparative advantage influences the fertiliser adoption decisions in cocoa farming in Ghana.[43] Finally, if $\phi = 0$ in the CRC model, the fertiliser adoption decision (f_{it}) is no longer correlated with the error term (u_{it}) in the equation $y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it}$. This would imply that the Ordinary least squares (OLS) estimations of the fertiliser returns do not suffer from the endogeneity problem.[44]

1.7 Conclusion

This paper considers nested models of production functions to investigate returns to fertiliser in the context of cocoa farming in Ghana. Using a five-wave panel dataset we first estimate a standard static production function (the baseline model). Subsequently we consider two extensions of this model, and discuss the identification issues. The first extension is the baseline model which allows for serial correlation in the error term (Blundell and Bond, 1998). The second extension is a Correlated Random Coefficient (CRC) which allows for comparative advantage in the error term (Suri, 2011). Our estimations of the CRC model also enable us to investigate whether comparative advantage can explain

[43] The coefficient ϕ also describes the sorting of the economy in the CRC model (Chamberlain, 1982). If $\phi < 0$, the farmers´ adoption decisions would generate less yield inequality relative to the random allocation of technology. This scenario is predicted by Suri (2011) in her dataset. More inequality relative to the random allocation of technology would occur if $\phi > 0$. Since $H_0 : \phi = 0$ was not rejected in this dataset, there is insufficient evidence to claim that the adoption patterns of fertiliser in cocoa farming in Ghana would influence the yield inequality in any direction

[44] If $\phi = 0$, the term $\phi\theta_i f_{it}$ disappears from the equation $y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it}$ and fertiliser returns are homogeneous and equal to β. This is an important implication of the result $\phi = 0$. One of the key research question of this paper is to identify the fertiliser returns (Hypothesis I). If $\phi = 0$, the fertiliser returns are identified not only by the CRC model (fertiliser returns are identified in the CRC model irrespective of the value of ϕ), but also by both the baseline model and by the baseline model with serial correlation in the error term.

fertiliser adoption decisions in cocoa farming in Ghana.

Our first finding suggests that returns to fertiliser in cocoa farming in Ghana may be high. We base our results on distinct models of production functions, as we do not claim a priori that a particular specification of production function is correct (Eberhardt and Helmers, 2016). Irrespective of the model considered in this paper, the estimates of fertiliser returns are found to be high and statistically significant.

While fertiliser adoption appears to be very beneficial for cocoa farmers in our data, we interpret our result with caution for several reasons. First, we find that adoption of fertiliser has a strong positive impact on yields, but this may not necessary improve profits of an individual farmer. Farmers may differ substantially in terms of fertiliser costs, distance to distribution centres, or labour costs. This information is not available in our data, and hence we are not able to make precise statements regarding profitability of a fertiliser investment. Second, our regression results estimate yield impact of a decision to adopt fertiliser, and not of the quantity of fertiliser used on farm. Moreover, we could also estimate returns to fertiliser at plot level, but fertiliser use is only measured at farm level in our data. Finally, migration is one of the explanations for attrition in our sample. We conjecture that if migration occurs due to low fertiliser returns our estimates may be biased upward.

Our second finding cannot conclude with certainty that fertiliser adoption decisions in our sample are necessarily driven by heterogeneous comparative advantage. We do not find sufficient statistical evidence for comparative advantage in our estimations of the CRC model. This appears to contrast with Suri (2011), who finds that Kenyan maize farmers with higher comparative advantage in using hybrid seeds are in fact less likely to adopt hybrid seeds. This discrepancy may be due to the fact that the importance of the comparative advantage may be context-specific (e.g. Kenya or Ghana) or crop-specific (e.g. maize or cocoa). While cocoa in Ghana is tree crop predominantly used for exports (Stutley, 2010; Teal et al., 2010), maize in Kenya is staple food primarily used for subsistence (Duflo et at, 2009; Suri, 2011). Moreover, our study investigates comparative advantage in the context of fertiliser adoption, which is a different agricultural technology to hybrid seeds.

Our empirical results shed light on potential limitations of comparative advantage in explaining technology adoption patterns in all agricultural contexts. However, we do not rule out the possibility that it does play role in our data. One could still find evidence for comparative advantage in the extended versions of the CRC model. For instance, one could allow for persistence in data or for learning about returns to new technology. Suri (2011) argues that learning is of little relevance in her data, as farmers in Kenya have been experimenting with hybrid maize seeds since 1960s. However, Teal et al. (2010) find evidence that, despite the availability of fertiliser in Ghana for a few decades, many cocoa farmers may still need to learn how to correctly apply the technology in order to maximise fertiliser returns. While extensions of the CRC model may be justified in certain contexts, identification of more complex models is difficult. Specifically, it would require not only a correct specification of persistency in data or of the structure of learning (Chamberlain, 1993), but also a very long panel dataset (Gibbons, Kate, Lemieux, Parent, 2002).

This paper suggests that the potential yield improvements of fertiliser adoption are high. While there appear to be large potential benefits from technology adoption, the reasons behind the low take-up are still unknown. Specifically, in the context of cocoa farming in Ghana, the lack of adoption of fertiliser may not necessarily be generated by comparative advantage. A better understanding of both the reasons behind the low take-up of new technologies, and the solutions which may successfully address these obstacles may contribute both to rural development and to poverty eradication in developing countries.

1.8 Appendix

Deriving the CRC model

The CRC model by Suri (2011) assumes heterogeneity in comparative advantage. In this model there are two types of cocoa farmers: farmers who adopt fertiliser (henceforth F), and farmers who do not adopt fertiliser (henceforth N). This is a self-selection model, in which a farmer decides whether to adopt fertiliser (henceforth F), or not to adopt fertiliser (henceforth N). A farmer chooses F over N if it leads to higher profits.

As in Suri´s model, the profits are assumed to be influenced fundamentally by a comparison of yields between F and N. However, the adoption decision in any given season is undertaken before observing yields. Subsequently, the yields may be influenced by exogenous shocks to availability of inputs. The adoption decision takes place during the planting season, which occurs a few months prior to harvesting. There is heterogeneity across farmers in terms of productivity, both because of absolute advantage (irrespective of whether they choose F or N) and comparative advantage (differential productivity by choosing F rather than N). Differences in comparative advantage across farmers may result from different observable and unobservable costs and benefits related to adopting F (e.g. the distance to the closest distributor of F or the cost of credit). Hence, differences in comparative advantage may result in differences in profits across farmers.

The profits are primarily determined by yields, and the production functions in sectors F and N are assumed to take the following Cobb-Douglas forms:

$$Y_{it}^{F} = (\Pi_{j=1}^{j=k} X_{ijt}^{\alpha_j^F}) e^{\beta_t^F} e^{u_{it}^F} \tag{9}$$

$$Y_{it}^{N} = (\Pi_{j=1}^{j=k} X_{ijt}^{\alpha_j^N}) e^{\beta_t^N} e^{u_{it}^N} \tag{10}$$

In this specification the production functions are allowed to vary across individuals i and across time periods t. X_{ijt} are set of j controls (i.e. land, labour, use of insecticide, cocoa prices).

After taking logs, we obtain:

$$y_{it}^{F} = x_{it}^{'} \alpha^{F} + \beta_t^F + u_{it}^F \tag{11}$$

$$y_{it}^{N} = x_{it}^{'} \alpha^{N} + \beta_t^N + u_{it}^N \tag{12}$$

where

$$u_{it}^{F} = \theta_i^F + \xi_{it}^F \tag{13}$$

$$u_{it}^{N} = \theta_i^N + \xi_{it}^N \tag{14}$$

The above equations introduce the idea of heterogeneity across farmers in terms of their productivities.[45]

[45] Rather than following learning models, where farmer characteristics are homogeneous but unknown to individual farmers, in the CRC model there is heterogeneity across farmers in terms of costs and benefits associated with different technologies. The farmer-specific values of θ_i^F and θ_i^N are known to the individual farmer i. The farmer does not know ξ_{it}^F nor ξ_{it}^N , yet these error terms should not affect the adoption decision (as in Heckman and Honore, 1990). This is due to the fact that ξ_{it}^F and ξ_{it}^N are time-varying shocks to production, and are assumed to be uncorrelated with covariates.

θ_i^F and θ_i^N are known to the individual farmer i, but cannot be directly estimated from equations (11) and (12). The following decompositions of θ_i^F and of θ_i^N enable me to obtain an expression for an identifiable farmer-specific comparative advantage (as in Lemieux, 1998 and Suri, 2011).

$$\theta_i^F = b_F(\theta_i^F - \theta_i^N) + \tau_i \tag{15}$$

$$\theta_i^N = b_N(\theta_i^F - \theta_i^N) + \tau_i \tag{16}$$

where

$$b_F = (\sigma_F^2 - \sigma_{FN})/(\sigma_F^2 + \sigma_N^2 - 2\sigma_{FN}) \tag{17}$$

$$\begin{aligned} b_N &= (\sigma_{FN} - \sigma_N^2)/(\sigma_F^2 + \sigma_N^2 - 2\sigma_{FN}) && (18) \\ \sigma_{FN} &= cov(\theta_i^F, \theta_i^N) && (19) \\ \sigma_F^2 &= Var(\theta_i^F) && (20) \\ \sigma_N^2 &= Var(\theta_i^N) && (21) \end{aligned}$$

τ_i is the residual in both equations (15) and (16), hence it is orthogonal to the difference $(\theta_i^F - \theta_i^N)$. As τ_i is the effect on output irrespective of whether F or N is chosen, it can be interpreted as a farmer i´s absolute advantage. I now define the expression for comparative advantage θ_i, which closely relates to the difference $(\theta_i^F - \theta_i^N)$ in the following way:

$$\theta_i = b_N(\theta_i^F - \theta_i^N) \tag{22}$$

Following Suri (2011), the structural parameter ϕ is defined as follows:

$$\phi = \frac{b_F}{b_N} - 1 \tag{23}$$

This enables me to rewrite equations (15) and (16) as:[46]

$$\theta_i^F = (\phi + 1)\theta_i + \tau_i \tag{24}$$

$$\theta_i^N = \theta_i + \tau_i \tag{25}$$

Now the above equations are plugged into expressions for residuals (13) and (14), giving:

$$u_{it}^F = (\phi + 1)\theta_i + \tau_i + \xi_{it}^F \tag{26}$$

$$u_{it}^N = \theta_i + \tau_i + \xi_{it}^N \tag{27}$$

Hence, the original log forms for output (11) and (12) can be rewritten as:

$$y_{it}^F = x_{it}^{'}\alpha^F + \beta_t^F + (\phi + 1)\theta_i + \tau_i + \xi_{it}^F \tag{28}$$

$$y_{it}^N = x_{it}^{'}\alpha^N + \beta_t^N + \theta_i + \tau_i + \xi_{it}^N \tag{29}$$

Finally, by knowing that adoption decision f_{it} is a binary decision equalling 1 in case of F and 0 in case of N, I can simplify the derivations above by using the following generalised equation for output:

$$y_{it} = f_{it} y_{it}^F + (1 - f_{it}) y_{it}^H \tag{30}$$

[46] Note that, by plugging the values for b_F and $b_N$in equations (17) and (18) into equation (23), we can obtain $\phi = \frac{\sigma_F^2 - \sigma_{FN}}{\sigma_{FN} - \sigma_N^2}$.

Plugging the expressions for y_{it}^F and y_{it}^N from equations (28) and (29) respectively into equation (30) gives the following empirical specification:

$$y_{it} = x_{it}^{'}\alpha + \beta f_{it} + u_{it} \tag{31}$$

where

$$u_{it} = \vartheta_i + \theta_i + \phi\theta_i f_{it} + \epsilon_{it} \tag{32}$$

and

$$\epsilon_{it} = f_{it}\xi_{it}^F + (1 - f_{it})\xi_{it}^F, \quad \alpha^F - \alpha^N = \alpha, \quad \beta_t^F - \beta_t^N = \beta\forall t, \tag{33}$$

Equation (31) is the Correlated Random Coefficient (CRC) model, since the comparative advantage θ_i is unobserved, and correlated with regressors. The coefficient $\phi\theta_i$ depends on unobservable θ_i; hence the simple Ordinary Least Squares (OLS) regression results in endogeneity due to omitted variable bias. Excluding θ_i from the regression puts comparative advantage into the error term. Because θ_i influences dependent variable y_{it}, and is now correlated with the regressor f_{it}, estimators in this incomplete regression would be subject to bias and inconsistency.

Table 8: CRC Estimations of the linear projections $y_{i1} = \kappa + \gamma_1 f_{i1} + \lambda_2 f_{i2} + \gamma_2 f_{i1} f_{i2} + \varepsilon_{i1}$ and $y_{i2} = \kappa + \lambda_1 f_{i1} + \gamma_3 f_{i2} + \gamma_4 f_{i1} f_{i2} + \varepsilon_{i2}$ (equations 97) and (8)) ; dep. var. : ln(COCOA); years: 2002 and 2010

	(1)		(2)		(3)	
	Output,2002	Output,2010	Output,2002	Output,2010	Output,2002	Output,2010
FERT, 2002	1.122**	0.823***	0.535*	0.054	1.164	0.229
	(0.352)	(0.198)	(0.222)	(0.096)	(1.476)	(0.423)
FERT, 2010	0.227	-0.019	0.239**	-0.167	0.623	-2.431
	(0.130)	(0.545)	(0.086)	(0.255)	(0.569)	(4.683)
FERT2002*FERT2010	-0.811	-0.192	-0.594*	0.515	-3.388	1.751
	(0.427)	(0.656)	(0.270)	(0.306)	(2.007)	(4.826)
ln(LAND)			0.567***	0.854***	0.595***	0.826***
			(0.046)	(0.027)	(0.055)	(0.037)
ln(LABOR)			0.186***	0.016	0.212***	0.060
			(0.037)	(0.035)	(0.045)	(0.046)
Constant	4.985***	5.284***	1.596***	0.489**	1.498***	0.630*
	(0.080)	(0.125)	(0.268)	(0.181)	(0.315)	(0.246)
Observations	232	235	218	178	218	178

se in parentheses

all regressions are OLS, robust se are used

(1) excludes covariates in regressions for both periods

(2) includes covariates in regressions for both periods

(3) includes covariates and interaction terms between covariates and dummies for fertilizer history (not reported) in regressions for both periods

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Recovered Values of the Structural Parameters (2002 and 2010)

Parameter	(1)	(2)	(3)
λ_1	-0.019	-0.167	-2.431
	(0.972)	(0.514)	(0.603)
λ_2	0.227	0.239**	0.623
	(0.082)	(0.006)	(0.273)
λ_3	0.158	-0.034	0.343
	(0.124)	(0.386)	(0.154)
β	0.252	0.983	0.367*
	(0.845)	(0.325)	(0.034)
ϕ	-2.508	-2.734	-1.683
	(0.974)	(0.358)	(0.320)

b are coefficients; p values are in parentheses
(1) regressions exclude covariates
(2) regressions include covariates
(3) regressions include covariates and interaction terms with adoption dummies
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Estimations of the structural parameters are based on results from Table 6.

Table 10: Recovered Values of the Structural Parameters (2002, 2006 and 2010; randocoef command)

Parameter	
λ_1	-0.078
	(0.382)
λ_2	0.078
	(0.151)
λ_3	0.0322**
	(0.137)
λ_4	0.961
	(0.607)
λ_5	0.204
	(0.446)
λ_6	0.154
	(0.243)
λ_7	-0.983
	(0.701)
β	0.236*
	(0.126)
ϕ	-0.391
	(0.393)

b are coefficients; p values are in parentheses
regressions include covariates and interaction terms with adoption dummies
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
estimations based on 'randcoef' command (Cabanillas, O. B., Davis, C. A., Michler, J. D., Michuda, A., & Tjernström, E. 2017. Fitting and interpreting correlated random coefficient (CRC) models using Stata.).

2 Chapter II

2.1 Introduction

Improvements in agricultural productivity can significantly contribute to economic development (Gollin et al., 2002). Nevertheless, the adoption rates of new technologies are still low in developing countries (Suri, 2011; Conley and Udry, 2010). Farmers may be discouraged from investing in yield-improving technologies, if this leads to greater yield variability (Zilberman, 1983)

In recent years, the development community has grown increasingly interested in the use of insurance arrangements, motivated by precisely the issue of encouraging technology adoption. Such insurance arrangements fall into two broad categories: traditional indemnity insurance and (especially in more recent years) weather index insurance schemes. The payouts in traditional indemnity insurance schemes are determined by outcomes on an individual farm. In contrast, the payouts in index insurance schemes are determined by a correlation of an index with specified weather conditions at a district level. The benefit of the latter type of insurance is the elimination of moral hazard and adverse selection problems (Clarke, 2016).

Index insurance schemes may be unattractive due to basis risk, which occurs when the index on which the insurance payouts are determined is imperfectly correlated with the shocks experienced by the farmer. A growing number of randomised controlled trials (RCTs) have investigated whether index insurance schemes can encourage higher adoption rates of yield-improving technologies (Cole et al., 2013; Gine and Yang, 2009; Karlan et al., 2014; Mobarak and Rosenzeig, 2012). This empirical evidence suggests that basis risk may be an important non-price factor limiting limits the impact of index insurance schemes on agricultural modernisation.

RCTs (Randomised Controlled Trials) are important methodologies for rigorously evaluating the effectiveness of particular policies in development economics (Duflo et al., 2011). Through randomisation, RCTs enable researchers to evaluate a particular policy of interest and disentangle, to a certain extent, its causes and effects. Nevertheless, framed field experiments (FFEs) can provide valuable, complementary insights into the evidence from RCTs.

The FFEs offer the benefit of controlling for potential confounding factors, which could influence the key variable of interest (Harrison and List, 2004). In the context of technology adoption in agriculture, a FFE can focus on the treatment effect of one particular variable of interest (e.g. index insurance), and exclude potential confounding factors, such as learning, trust in institutions, access to credit or loan default. This is substantially more challenging to achieve by means of RCTs.

Another important advantage of FFEs is the possibility to precisely calibrate the level of basis risk (Clarke and Kalani, 2011). While the precise estimation of basis risk is very challenging, if not impossible, in RCTs, (Bryan, 2014), FFEs enable the exact calibration of basis risk. Furthermore, FFEs can precisely introduce experimental treatments with varying levels of basis risk. Equivalent treatments in RCTs are likely to suffer from an enormous lack of precision, and, therefore, may not provide a valid empirical evidence.

Despite the above-mentioned benefits of FFEs, few papers have examined the use of index insurance to promote technology adoption. Two notable exceptions are Galarza and Carter (2011) and Hill and Viceisza (2012) . The former FFE investigates the uptake of credit under an index insurance with a positive level of basis risk. The latter FFE focuses on the behavioural impacts on fertiliser investment decisions of an insurance contract with no basis risk. A possible reason why the findings in the two studies are different may be the fact that the type of index insurance is also very different across these experiments.

The objective of this paper is to examine the direct impacts of different levels of basis risk and premium rates on technology adoption under index insurance. Based on a FFE, conducted with cocoa farmers in Ghana, we test the following two hypotheses: (1) an expensive index insurance with no basis risk increases the uptake of fertiliser; (2) a cheap index insurance with a positive level of basis risk decreases the uptake of fertiliser.

This paper contributes to the existing, growing literature on technology adoption under index insurance. The FFE in this paper enables us to study the effectiveness of distinctive insurance schemes, with varying levels of basis risk or premium rates. The empirical results are important and complementary to RCTs, since the experimental treatments studied in this paper may not be feasibly studied with any precision using RCTs.

The findings of this paper suggest that the effectiveness of index insurance in encouraging technological adoption may largely depend on the specifics of a particular insurance scheme. The impact of expensive index insurance with no basis risk on fertiliser uptake is found to be large and strongly significant statistically. Interestingly, the second hypothesis of the negative impact of inexpensive insurance with positive basis risk is rejected. The treatment effect is, in fact, positive, but it is not statistically significant.

The remainder of this chapter is structured as follows. Section 2.2 summarises the related literature on the impact of index insurance on agricultural technology adoption in developing countries. Section 2.3 describes the experimental design, experimental sample, and formulates the hypotheses. Section 2.4 presents and discusses the empirical findings. Section 2.5 concludes.

2.2 Literature Review

The majority of populations from the poorest countries earn their incomes from farming activities. Promotion of yield-improving agricultural technologies could substantially raise incomes of farmers and thus contribute to the eradication of poverty in rural areas. Nevertheless, the modernisation of agriculture is far from widespread in developing countries. This problem is particularly apparent in Sub-Saharan Africa, where adoption rates of new technologies for a given crop very rarely exceed half of the total number of farmers. The academic debate on the reasons underlying the lack of agricultural adoption as well as the most effective solutions for encouraging this modernisation are still unresolved (Foster and Rosenzweig, 2010).

Cocoa farming in Ghana is characterised both by low levels of productivity and low adoption rates of fertiliser (Teal et al., 2010). Since Ghana is one of the world´s key cocoa producers, the potential for substantial economic growth in its rural areas appears to be high. Nevertheless, investment in fertiliser, the key technological innovation in cocoa farming, involves considerable risks (Stutley, 2010). The agricultural harvest depends highly on weather conditions. Under adverse weather conditions, fertiliser may be completely ineffective in raising yields. The drop in income due to a failed fertiliser investment may be particularly severe in areas of near-subsistence standards of living. Farmers may refrain from innovating, in order to avoid the risk of extremely low level of income (Dercon and Christiaensen, 2011). Insurance schemes that address such risk could potentially encourage higher uptake of new technologies (Gine and Yang, 2009).

Successful insurance schemes must be valuable to farmers as well as commercially viable for the insurers. Traditional indemnity insurance schemes are often unattractive for an insurer due to the problems of adverse selection and moral hazard. A farmer with less attractive farming conditions is more likely to sign an insurance contract, since he already expects low yields on his farm. Moreover, once insured, a farmer may have less of an incentive to exercise effort on his farm because a low harvest would be compensated for by an insurance claim. If contract enforcement is also weak, the farmers defaulting on their loans could not be punished by lenders´ seizure of their collateral (Gine and Yang, 2009).

In light of the drawbacks of indemnity insurance, index insurance appears very attractive, as neither adverse selection nor moral hazard is present in the latter product. Rather than the level of individual losses determining insurance claims, index insurance pays claims depending on an external index that is independent of the individual experiences of the farmer. In an agricultural context, an index might be related to weather or yields at a district level. For instance, if the rainfall level in a given district fell below a specified threshold, all farmers with an insurance contract could make a claim from the insurer. However, if rainfall levels were above the threshold, no farmer would be able to settle a claim with the insurer. Therefore, the index insurance does not suffer from adverse selection and moral hazard problems in this scenario.

Another advantage of index insurance is the low administration and transaction costs, since payment of claims is clearly defined by the index. Nevertheless, index insurance schemes suffer from basis risk. The triggering of an index is not directly related to individual losses; hence, an insured farmer facing a localised farming problem in a district with favourable weather conditions might experience high losses without being compensated by the insurer (correspondingly, the farmer might receive a payout without having incurred a loss – a happier situation for the farmer).

There is both theoretical and empirical evidence confirming the idea that basis risk can significantly lower the demand for index insurance. Clarke (2016) shows with a theoretical model that demand for index insurance with a given level of basis risk has an inverted-U shape among risk-averse farmers. The demand for index insurance would be highest among individuals with moderate rates of risk aversion. The risk-loving and the least risk-averse farmers do not find index insurance attractive simply because of an unwillingness to pay premiums for insurance. On the other hand, very risk-averse farmers prefer not to take index insurance due to basis risk which makes the worst outcome even worse.[47]

In two separate RCTs on rainfall index insurance (Cole et al., 2012) and drought index insurance (Karlan et al., 2014), demand for index insurance is examined, as insurance is sold either at subsidised, actuarially fair, or market-level premium rates to different group of farmers. As expected, both papers find evidence of downward sloping demand for insurance as premium rates increase, yet the authors emphasise the importance of other non-price factors affecting the demand for insurance. Apart from credit constraints and the possible lack of understanding of an insurance concept among less financially literate subjects, lack of trust in the insurer delivering insurance claims may substantially lower demand.

Karlan et al. (2014) find that farmers taking insurance in the first season are more likely to continue in subsequent seasons, if either they or farmers from their social network receive claims from the insurer. Again, this is related to

[47]For instance, a farmer who pays a premium and has a bad harvest due to a localised problem not affecting the rest of the district would suffer even more. On top of a low harvest, the additional spending on insurance premiums lowers farmers´ income even further.

the idea of basis risk.[48] Trust in an insurer would be severely undermined if a farmer experienced low yields and expected claims from an insurer that were never delivered to him. It seems that both pricing of insurance and the perceived level of basis risk are key factors affecting insurance take-up, and research in this area is greatly needed in order for insurance to contribute substantially to higher rates technology adoption.

As shown above, numerous field experiments have studied the impact of index insurance schemes on technology adoption. Unlike the studies in the field, it is very simple in the lab to control for factors like trust, learning and the ambiguity of insurance concept, and focus on examining the pure effect on insurance take-up of, for instance, varying premium rates or basis risk levels.[49] However, decision making on technology adoption under insurance schemes is understudied in the lab environment with two notable exceptions. Galarza and Carter (2011) conducted a framed field experiment with Peruvian cotton farmers in order to study the impact of insurance on take-up of loans for new technology. The experimental design controls for implicit insurance, since subjects failing to repay the loans were not allowed to borrow again and lost half of the value of their collateral. In later rounds of the experiment, an insured loan for new technology was introduced and the authors found a significant positive impact on the uptake of loans.

The other lab experiment undertaken by Hill and Viceisza (2012) with Ethiopian farmers investigated the behavioural impact of insurance on investment in fertiliser. Insurance was mandated for both adopters and non-adopters,

[48]Basis risk is also found to be an important drawback of index insurance in Mobarak and Rosenzweig (2012). The focus of their study is the complementarity between existing informal risk sharing networks and formal rainfall index insurance among Indian farmers.

[49]Harrison et al., (2010) stress that lab experiments can provide a complementary picture to field experiments and further enhance our understanding of real-world phenomena. Subject to satisfying both internal and external validity, lab experiments can control for a number of conflicting aspects potentially affecting the variable of interest and hence study in isolation the impact of one particular factor.

and only a moderate impact on fertiliser purchases was found. It is worth noting that the two experiments offer substantially different index insurance schemes. While the Peruvian subjects could choose to buy a market-priced insurance with basis risk, the Ethiopian subjects selected for treatment were forced to acquire either actuarially fair or free insurance with no basis risk.

The aim of this paper is to investigate in a framed field experiment whether index insurance bundled with credit can raise the adoption of fertiliser among Ghanaian cocoa farmers. This paper contributes to the existing literature on technology adoption, by studying investments in new technology under distinctive index insurance schemes with varying levels of basis risk and premiums. The paper explores whether the impact on fertiliser purchases would be higher under inexpensive insurance with no basis risk, or under expensive insurance with basis risk. By varying the basis risk, this paper provides a valuable insight into the ongoing and unconcluded debate on the potential impact of basis risk on the adoption rates of new agricultural technologies.

2.3 The Experiment

This paper investigates farmers´ fertiliser investment decisions under different index insurance schemes. The experiments were designed in order to resemble closely the farming reality faced by cocoa farmers. The experimental sample consists of cocoa farmers. Hence, the experiment falls into the category of framed field experiments (Harrison and List, 2004).

The experiment was undertaken in November 2012 in 10 villages in the cocoa producing Ashanti region in Ghana. The villages were randomly selected from the list of villages used in the CSAE survey (collected biannually since 2002), and are therefore fairly representative. The dataset consists of 351 farmers, invited at random to participate in the experiment by means of accessing the lists of cocoa farmers of all licensed buying companies operating in a given village. All subjects who were invited participated in the experiment. Sessions were conducted in the local Twi language by trained experimenters who always kept the same roles in all experimental sessions.

All of the subjects played three different farming games, and the counter-balanced design was implemented in order to control for the order effect. Overall, six session types were conducted. Each session type was carried out at least three times and, as a result, there were 20 sessions in total. We conducted two sessions per day, with an average number of approximately 18 subjects per sessions. Depending on the accessibility and the available facilities in a given villages, the sessions were run at local churches or offices of purchasing clerks. A typical session lasted approximately 75 minutes, with an additional 10 minutes per participant for questionnaire related to key personal and agricultural characteristics. The average experimental winnings (paid in GHC, the local currency New Ghanaian Cedis), were approximately GHC 7.5, and included a show-up fee of GHC 2. These winnings were equivalent to approximately two daily wage for hired labour on farm in the local area at the time when the experiment was conducted. [50]

2.3.1 Sample Description

Table 11: Descriptive Statistics

	Not used F	Used F	Total
Yield (t/ha)	187	287	239
	(163)	(249)	(218)
If household head	.76	.78	.77
	(.43)	(.42)	(.42)
If male	.6	.68	.64
	(.49)	(.47)	(.48)
If married	.72	.79	.75
	(.45)	(.41)	(.43)
Age	56	57	56
	(12)	(14)	(13)
Number of children	2.9	2.8	2.8
	(2.6)	(2.4)	(2.5)
Farm size (ha)	3.1	3.7	3.4
	(3.2)	(3.1)	(3.1)
Education level	2.1	2.4	2.3
	(1.4)	(1.4)	(1.4)

Main statistic is the mean. Standard deviation is in parenthesis.
Sample size is equal to 351.
265 farmers did not use fertiliser in the last season (column (1)).
87 farmers used fertiliser in the last season (column (2)).

This section comments on key descriptive statistics obtained in short post-experiment questionnaires from 351 Ghanaian cocoa farmers. The first two columns in Table 11 are subsamples divided according to the real fertilizer investment decisions made in the last cocoa season. First column shows mean

[50] The experimental winnings of each subject depended both on the investment choices made and a stochastic weather condition. Each subject received the payment based on the choice made in only one of the games, T0, T1 or T2. Since the actual game would only be known at the end of the experiment, all three decisions were incentivised.

of the key variable among 76% non-adopters of fertilisers. Only 24% adopted fertiliser in 2012 and their key descriptive statistics are displayed in the middle column. On average, adopters tended to be slightly older and were more likely to be household heads. A slightly higher proportion of adopters than non-adopters were male (68% of adopters versus 60% of non-adopters). This indicates that males may be more willing to experiment with fertiliser technology on their farms. Adopters in 2012 had higher levels of education and a substantially higher land area of 3.7 Ha, relative to 3.1 Ha of non-adopters. This could either indicate that adopters are richer and more willing to take risky investment. They could also have better access to credit, as their land may be more likely used as collateral.

Yields of the fertiliser adopters (column 2 in Table 11), at 0.29 tonne per hectare, were were almost double that of the non-adopters (column 1 in Table 11), at 0.19 tonne per hectare. This is consistent with the idea that fertiliser may improve production on Ghanaian cocoa farms, although it could also reflect selection into fertiliser use by farmers with the best land or highest levels of skill. However, low aggregate adoption rates remain a puzzle. The adoption of other technologies, such as fungicides and insecticides, is almost universal, both among adopters and non-adopters of fertiliser. However, unlike fertiliser investment, which must be fully paid for by farmers, fungicides and insecticides are in part provided free of charge by the Ghana Cocoa Board.

2.3.2 The Experimental Design

In our experiment, participants chose among two investment options, namely an investment in traditional technology (denoted OLD), or in fertiliser (denoted Takeup). Experimental payoffs depended both on the investment choice, and on the random realization of weather. We consider three treatments, denoted T0, T1 and T2, which differ in terms of the type of index insurance that is bundled to the fertiliser investment. Table 12 summaries the experimental games, and shows the ranking of investment options according to Expected Utility.

The weather outcomes were identical in all treatments. The good weather occurred with a probability of 3/4. The bad weather occurred with a probability of 1/4. Two different types of bad weather were specified: a common bad weather and a localised bad weather, [51] each occurring with the equal probability of 1/8 (hence, the overall probability of bad weather was equal to 1/4). Investment in OLD generated experimental winnings of 6 in good weather, [52] and 4 in both types of bad weather.

Under the control treatment T0, an investment option alternative to OLD was the investment in the fertiliser with no insurance (Takeup_{T0}). Under good weather conditions, Takeup_{T0} generated the experimental winnings of 7. Under both types of bad weather Takeup_{T0} generated the experimental winnings of 1.[53]

Under the treatment T1, an investment option alternative to OLD was the investment in the fertiliser with a low-premium index insurance with a 1/8 basis risk (Takeup_{T1}).[54] Under good weather Takeup_{T1} generated experimental

[51]Subjects were asked to think of an example of common bad weather as something affecting all neighbouring farms equally (e.g. excessive sunshine). Localised bad weather was explained as a factor affecting only a given farmer (e.g. the occurrence of pests or an idiosyncratic disease on a farm).

[52]At the end of the experiment all the experimental winnings were converted to the local currency GHC at a 1:1 ratio (e.g., the experimental winnings of 6 = GHC 6).

[53]Takeup_{T0} was an investment with a higher mean but also a higher variance relative to OLD.

[54]The insurance premium under T1 was equal to 1 unit (33% below the actuarially fair

Table 12: Payoff comparisons across investment options

Payoff comparison: 'Old' and 'Takeup$_{T0}$'

Technology	$Pr(good\ weather) = \frac{3}{4}$	$Pr(common\ bad) = \frac{1}{8}$	$Pr(localised\ bad) = \frac{1}{8}$	**Mean**	**CRRA range**
'OLD'	6	4	4	5.5	$\sigma \geqslant 0$
'Takeup$_{T0}$'	7	1	1	5.5	$\sigma < 0$

Payoff comparison: 'Old' and 'Takeup$_{T1}$'

Technology	$Pr(good\ weather) = \frac{3}{4}$	$Pr(common\ bad) = \frac{1}{8}$	$Pr(localised\ bad) = \frac{1}{8}$	**Mean**	**CRRA range**
'OLD'	6	4	4	5.5	$\sigma \geqslant -0.7$
'Takeup$_{T1}$'	6.5	6.5	0.5	5.75	$\sigma < -0.7$

Payoff comparison: 'Old' and 'Takeup$_{T2}$'

Technology	$Pr(good\ weather) = \frac{3}{4}$	$Pr(common\ bad) = \frac{1}{8}$	$Pr(localised\ bad) = \frac{1}{8}$	**Mean**	**CRRA range**
'OLD'	6	4	4	5.5	$\sigma \leqslant 5.4$
'Takeup$_{T2}$'	5	5	5	5	$\sigma > 5.4$

note: CRRA range is the range of coefficients for which a given investment option is preferred for a decision maker with CRRA preferences winnings of 6. Under bad weather, experimental winnings depended on the type of weather. Specifically, under common bad weather, the index of the insurance would be triggered, and the farmer would receive an insurance payout. This would generate the experimental winnings of 6.[55] Under the localised bad weather, the index of the insurance would not be triggered (basis risk scenario). Hence, the farmer choosing FERT_{T1}would not receive an insurance payout. This would generate the experimental winnings of 1.[56]

Under the second experimental treatment T1, an investment option alternative to OLD was the investment in the fertiliser with a high-premium index insurance with no basis risk (FERT_{T2}).[57] Under good weather, Takeup_{T2} generated experimental winnings of 5. Under bad weather, the farmer choosing Takeup_{T2} still receives 5 irrespective of localised or common bad weather.. This was due to the absence of basis risk in index insurance under treatment T2. [58]

2.3.3 Implementation of the Experiment

At the beginning of the experiment, the farmers were asked to think about the experimental payoff as the seasonal cash profit from harvested cocoa beans. Their equivalent harvested income would depend both on the type of investment made and on the stochastic realisation of weather conditions. In all the fertiliser

rates).

[55]These winnings are higher than the OLD winnings of 4 (despite the failed fertiliser investment, the farmer under $\mathrm{FERT}_{T1}$is protected by the insurance under common bad weather conditions).

[56]These winnings are lower than the OLD winnings of 4 under localised bad weather (on top of the failed fertiliser investment, the farmer under $\mathrm{FERT}_{T1}$is not protected by the insurance under localised bad weather conditions.)

[57]The insurance premium under T2 was equal to 2 units (167% below the actuarially fair rates).

[58] The choice of Takeup_{T2} requires a payment of high premium in good weather, but index insurance with zero basis risk completely eliminates the variability of experimental winnings in bad weather.

investment games, they could decide to apply traditional technology with low income variability under uncertain weather realisations (denoted OLD). Alternatively, they could invest in either Assase Wura or Coco Feed fertiliser on their farm. [59]

Figure 1: Explaining Weather Cards with Real Cocoa Pods

The weather outcomes described to experimental subjects did not focus a particular feature of the weather (e.g. good rainfall levels or favourable sunlight conditions).[60] After piloting different ideas, we found that generally good

[59] These two fertilisers are almost the only fertilisers available in the visited villages, and it is common to buy the fertiliser on credit from a purchasing clerk representing one of the several licensed buying companies in Ghana. Therefore, farmers could better relate their choices to real investment decisions on their cocoa farms.

[60] In general, farming is a prolonged process commonly taking several months during each season and involving a number of distinct farming stages. Favourable weather in one stage may differ substantially from another stage.

weather were best understood by the farmers as the weather conditions resulting in high levels of harvested cocoa beans.[61]

In the experiment, good weather occurred with probability of 3/4, and implied high level of harvested cocoa beans. In order to complete the characterisation of the probability distribution, we assigned a probability of 1/4 to the bad weather outcome, which implied a low quantity of harvested cocoa beans. The distribution of weather was illustrated by the real cocoa pods. Farmers were presented with six large healthy, orange pods and two large unhealthy, black pods.[62] Substantial time was spent on demonstration draws that were made by randomly chosen subjects, also resulting in more transparency and trustworthiness among the experimental subjects. To foster a better understanding of the probability distribution, all eight pods could also be seen at all times of the experiment. Furthermore, as seen in Figure 1, in each game, all subjects had a set of visualisation cards illustrating all weather types and all associated levels of harvest.

The vast majority of farmers never had any experience with any form of insurance. Therefore, introducing this concept would both create confusion and result in the lengthy duration of an experimental session. Instead, the term ´protection´ was used.Given that our experimental sample tended to have low levels of formal education, we decided to present a 1/8 probability of basis risk

[61] Cocoa farming encompasses the months from pod ripening to harvesting and drying the cocoa beans. For instance, rain is necessary for cocoa pods to mature but excessive rain can cause black pod disease, while lack of rain is good during the drying of the fermented cocoa beans.

[62] Real cocoa pods were matched by the equivalent number of six small orange marbles and two small black marbles that were used for the actual draw of the weather conditions in the experiment.

in reduced form (see Figure 2).

In order to explain the concept of basis risk, we distinguished between two types of bad weather: a) common bad weather (represented by a striped pod),[63] and b) localised bad weather (represented by a fully black pod).[64]. Drawing a striped pod would imply common bad weather triggering of the index. In this case, the insured farmer would be protected and. In contrast, drawing a fully black pod would imply localised bad weather and no triggering of the index. This was the basis risk scenario, in which case the insured farmer would not be protected.

Figure 2: Explaining Basis Risk

Great care and efforts were made to ensure both the understanding of the experiment and the privacy of the decision-making among the subjects. At the beginning of each session, subjects were randomly allocated their seat in the location of the experiment. After describing the structure of the experiment, subjects were asked questions to test their understanding of the instructions. Each subject was given envelopes with stickers and decision cards to record their preferred choices.[65]

[63]Subjects were asked to think of an example of common bad weather as a factor affecting all neighbouring farms equally (such as excessive sunshine).

[64]The localised problem was explained as a factor affecting only a given farmer (such as the occurrence of pests or an idiosyncratic disease on a farm, which often results in the destruction of cocoa pods and tree infection. (Stutley, 2010)).

[65] Due to the low average level of education of the subjects, stickers (instead of pen and

One of the features of the experiment was that the wealth of the subjects was not accumulated across rounds. Farmers made different technology decisions depending on the available fertiliser package in a given treatment, but their payoff was not revealed until the end of the experiment. This feature of the experimental design was important, as the aim of our study was to investigate the impact of index insurance on technology take-up decisions.[66]

The experiment also addressed the problem of the law of small numbers. Despite the clear description of the objective probabilities of the weather outcomes, farmers could form substantially different subjective expectations after a few rounds of weather realisation.[67]

2.3.4 The Research Hypotheses

This section presents the empirical tests of the research hypotheses. To recap, the empirical strategy aims to test the following two hypotheses:

paper) were used to record subjects' decisions.

[66] For instance, farmers could have waited strategically until they accumulated substantial wealth over early rounds before deciding to adopt the new technology. Our experimental design prevented the subjects from 'hedging' their risk against accumulated wealth.

[67] For instance, farmers who had experienced a few rounds of good weather might overestimate the likelihood of bad weather in the upcoming round. Recent studies have attempted to model and precisely control for the occurrence of the law of small numbers (Rabin, 2002; Rabin and Vayanos, 2010).

Hypothesis I

The takeup of fertiliser under the low-premium index insurance with a 1/8 basis risk ($Takeup_{T1}$) is lower than the takeup of fertiliser with no insurance ($Takeup_{T0}$).

In terms of the choices in treatments T0 and T1, this would imply:

$$Takeup_{T1} < Takeup_{T0} \tag{34}$$

Hypothesis II

The adoption rates of fertiliser mandated with the high-premium index insurance with no basis risk ($Takeup_{T2}$) are higher than the adoption rates of fertiliser with no insurance ($Takeup_{T0}$).

In terms of the choices in treatments T0 and T2, this would imply:

$$Takeup_{T2} > Takeup_{T0} \tag{35}$$

2.3.5 The Empirical Specification

In order to investigate these two hypotheses, I estimate the basic specification to be:

$$Takeup_i = \beta_0 + \beta_1 TreatT1_i + \beta_2 TreatT2_i + \beta_3 LowRA_i + X_i^{\mathrm{k}} \beta_4 + \varepsilon_i \tag{36}$$

In this Linear Probability Model (LPM):

1. $Takeup_i$ is a dummy variable taking value 1 if a subject i adopted the fertiliser,

2. $TreatT1_i$ is a dummy variable taking value 1 if a subject i made the

adoption decision[68] in treatment T1,

3. $TreatT2_i$ is a dummy variable taking value 1 if a subject i made the adoption decision[69] in treatment T2,

4. $LowRA_i$ is a dummy variable taking value 1 if subject i´s CRRA coefficient $\sigma_i \leq 0.18$.[70]

5. X_i^k is a vector of control variables,

6. ϵ_i is a mean zero error term.

The specification (36) enables me to test Hypothesis I while controlling for Hypothesis II and vice versa. Both hypotheses can be tested by means of Student´s t-test (Student, 1927).

2.4 Empirical Results

2.4.1 Preliminary Results (Non-parametrics)

Table 13 reports the subjects' decisions on whether to adopt fertiliser (% of total subjects) in T0, T1, and T2. The second column of Table 13 displays the real investment decisions of farmers in the last cocoa season. It should be stressed that this is not experimental data; hence, it should be compared with caution with the rest of the experimental data. However, due to the design of the experiment, where the greatest effort was made to both resemble closely real investment decisions of farmers and address issues of external validity, occasional comparisons of experimental variables to the dummy variable ´Fert2012´ are also made.

Looking at the descriptive summary of the results in Table 13, one can see

[68] In all experimental treatments the subjects made a binary decision whether to invest in the old technology or in the fertiliser.

[69] In all experimental treatments the subjects made a binary decision whether to invest in the old technology or in the fertiliser.

[70] The CRRA coefficients were elicited by the standard Binswanger procedure: the CRRA coefficients σ_i, which are interpreted as follows: $\sigma_i < 0 \implies i$ is risk-loving, $\sigma_i = 0 \implies i$ is risk-neutral, $\sigma_i > 0 \implies i$ is risk-averse.

Table 13: Proportion of Choices across Treatments

Adopt	If used fertiliser in 2012	$Takeup_{TO}$	$Takeup_{T1}$	$Takeup_{T2}$
No	76%	47%	45%	38%
Yes	24%	53%	55%	62%

that when insurance protection of T1 is available, subjects tend to invest more in fertiliser relative to T0 with no insurance option. A total of 53% adopted fertiliser under T0, and 55% did when cheap basis risk insurance was bundled with insurance. This difference appears substantially higher when fertiliser was bundled with expensive insurance with no basis risk. 62% chose fertiliser under T2. Out of 351 experimental subjects, this is equivalent to 218 farmers adopting under T2, 32 farmers more than under T0. Nevertheless, these early examinations of data are further investigated with a statistical methodology in subsequent sections. Owing to the fact that fertiliser use in 2012 (Table 13 is not a variable derived in a controlled laboratory experiment, it cannot be used in the formal statistical analysis. Nevertheless, the point remains that even under a fertiliser investment option with no insurance, as in T0, the adoption rates in the experiment are substantially higher than in the last real cocoa season . The adoption rates in the experiment more than double the figure of 24% of fertiliser users in the last cocoa season in 2012. This may imply that apart from insurance types offered in the experiment, there may also be other substantial obstacles to high aggregate adoption of yield-improving technologies.[71]

The statistical analysis starts with a Wilcoxon signed-rank test displayed in Table 14 in order to determine non-parametrically whether any of the insurance treatments have a positive impact on the adoption of fertiliser. When T0 is compared to T1, the null hypothesis of equal fertiliser investment choices in T0 and T1 is not rejected even at a 10% significance level. It appears that cheap

[71]It could also reflect the smaller financial stakes in the experiment as opposed to the actual cocoa farms.

Table 14: Signed-rank Tests for T1 and T2

Signed-rank	Null hypothesis	statistic	p-value
$Takeup_{TO}$ vs $Takeup_{T1}$	$Takeup_{TO} = Takeup_{T1}$	z=-0.6	0.549
$Takeup_{TO}$ vs $Takeup_{T2}$	$Takeup_{TO} = Takeup_{T2}$	z=-2.969	0.003

insurance with a basis risk is not attractive to farmers. Hence, farmers seem to adopt the same rates of fertiliser whether it is with or without insurance. It seems that expensive insurance with no basis risk is effective in raising the demand for fertiliser. These preliminary results are further examined in the LPM model.

The treatment effects of T1 and T2 are identified by estimating the LPM model in equation 36. Firstly, all key variables are described in Table 11. We are interested in whether the treatment variations affect farmers' decisions whether to adopt fertiliser. 'TreatT1' and 'TreatT2' are binary variables representing treatments 1 and 2 respectively (1= participated in the treatment, 0 otherwise). Similarly, 'LowRA*TreatT1´ and ´LowRA*TreatT2´ are interaction terms of the ´LowRA´ dummy variable, with indicator dummies for whether game T1 was played or whether game T2 was played, respectively.

Table 11 only describes the ´LowRA´ variable. This is a dummy taking a value of 1 if the subject chose the riskiest lottery A in the risk elicitation game in the gain domain. This dummy would take the value of 0 if individual chose either lottery B, C, D, E or F in the gain domain. Other dummy combinations were also tested, yet dummies for other ranges of risk aversion turned out neither to be statistic significant at the 10% level nor to affect the key results of the experiment.

2.4.2 Key Results (the LPM and Probit Models)

Table 15 presents the main results of the following empirical specification:

$$Takeup_i = \beta_0 + \beta_1 TreatT1_i + \beta_2 TreatT2_i + \beta_3 LowRA_i + X_i^{\mathrm{k}} \beta_4 + \varepsilon_i$$

To recap, the dependent variable is $Takeup$, which is a binary variable, taking the value of 1 if fertiliser is adopted and 0 otherwise. $TreatT1$ and $TreatT2$ are binary variables representing treatments 1 and 2 respectively (1= participated in the treatment, 0 otherwise). Fertiliser under the treatment T1 was offered with the low-premium index insurance with a 1/8 basis risk. Fertiliser under treatment T2 was offered with the high-premium index insurance with no basis risk. $LowRA$ is a dummy variable taking a value of 1 if the subject's CRRA coefficient is $\sigma_i \leq 0.18$.

The specifications in columns (1) and (2) are linear-probability models (LPM), and the specifications in columns (3) and (4) are Probit models.[72] While the LPM regression analysis does not guarantee that the probabilities are within the unit interval [0, 1], the estimated coefficients are consistent in presence of heteroscedasticity (Wooldridge, 2002). Since the simplifying assumption of homoscedasticity is often violated in data, this property is an important advantage of the LPM estimations. In contrast, while the Probit models ensure the probabilities are always bounded by the unit interval [0, 1], the estimated coefficients are inconsistent in the presence of heteroscedasticity (Greene, 2012). Unlike the LPM model, the Probit is not linear in the parameters and thus cannot be estimated by the OLS. The estimated parameters (e.g. via methods such as maximum likelihood estimations) in the Probit model will be inconsistent if the

[72]Columns (2) and (4) include two additional interaction terms of treatment and risk dummies.

assumption of homoscedasticity is violated and the problem of the heteroscedasticity is not actively addressed (Greene, 2012). Due to the fact that the inference based on either the LPM or the Probit model has certain advantages but also some limitations, the results in Table 15 are compared across the two models.

Results: Hypothesis I ($Takeup_{T1} < Takeup_{T0}$)

The data does not support Hypothesis I (fertiliser takeup with insurance T1 is lower than fertiliser takeup with no insurance). In all columns in Table 15, the coefficient on $TreatT1$ is not statistically significant at 10%. The sign of this coefficient is unexpected. The insurance under the T1 treatment was subject to basis risk. Despite the possible scenario of particularly low outcome (the experimental winnings of 0 under the basis risk), the fertiliser takeup rates under the T1 treatment are higher than in the baseline T0 (the coefficient on the $TreatT1$ is positive). However, this effect is not statistically significant, implying that the insurance under the T1 treatment is not effective in raising the fertiliser adoption rates.

Results: Hypothesis II ($Takeup_{T2} > Takeup_{T0}$)

The data supports Hypothesis II (fertiliser takeup with insurance T2 is higher than fertiliser takeup with no insurance). All columns in Table 15 show that the coefficient on $TreatT2$ is positive and statistically significant. Depending on the specification, the coefficient is significant at 1% (column 3), 5% (columns 1 and 4) or at 10% (p-value is 0.052 in column 2).

The estimated value of the coefficient on $TreatT2$ in columns 1-4 ranges from 0.087 to 0.096. The results are very similar in the LPM and Probit models. The high-premium index insurance with no basis risk is effective in raising fertiliser adoption. The fertiliser take-up under T2 is approximately 9 percentage points

Table 15: The Impact of Insurance Schemes T1 and T2 on Fertiliser Takeup

	(1) OLS	(2) Probit	(3) OLS	(4) Probit	(5) OLS	(6) Probit
TreatT1	0.017	0.017	0.026	0.025	0.042	0.041
	(0.650)	(0.649)	(0.409)	(0.392)	(0.380)	(0.354)
TreatT2	0.089*	0.089*	0.105*	0.104**	0.113*	0.110**
	(0.018)	(0.017)	(0.011)	(0.001)	(0.027)	(0.009)
LowRA			0.102*	0.102*	0.127*	0.125*
			(0.034)	(0.013)	(0.039)	(0.014)
If male			-0.030	-0.030	-0.030	-0.031
			(0.646)	(0.632)	(0.646)	(0.631)
Age			-0.003	-0.003	-0.003	-0.003
			(0.330)	(0.299)	(0.330)	(0.298)
If married			-0.028	-0.030	-0.028	-0.030
			(0.694)	(0.664)	(0.694)	(0.665)
Education level			-0.007	-0.008	-0.007	-0.008
			(0.500)	(0.466)	(0.500)	(0.466)
If household head			0.072	0.073	0.072	0.073
			(0.242)	(0.202)	(0.243)	(0.201)
Number of children			-0.011	-0.011	-0.011	-0.011
			(0.240)	(0.225)	(0.240)	(0.225)
Farm size (ha)			0.004	0.004	0.004	0.004
			(0.394)	(0.400)	(0.394)	(0.402)
Yield (t/ha)			0.192	0.191	0.192	0.191
			(0.153)	(0.137)	(0.153)	(0.136)
If ever used fertiliser			0.067	0.068	0.067	0.068
			(0.401)	(0.368)	(0.401)	(0.368)
Site			0.020*	0.019**	0.020*	0.019**
			(0.016)	(0.003)	(0.016)	(0.003)
Sessiontype			-0.007	-0.007	-0.007	-0.007
			(0.656)	(0.645)	(0.656)	(0.646)
Understanding			-0.023	-0.022	-0.023	-0.022
			(0.841)	(0.842)	(0.841)	(0.841)
LowRA*TreatT1					-0.052	-0.052
					(0.577)	(0.554)
LowRA*TreatT2					-0.023	-0.018
					(0.751)	(0.806)
Constant	0.531***		0.542*		0.534*	
	(0.000)		(0.024)		(0.024)	

Robust standard errors clustered at site level.
TreatT1 and TreatT2 take values 1 if the decision is made in T1 or T2, respectively, and 0 otherwise.
LowRA equals 1 if A chosen in the Binswanger lottery, and 0 otherwise.
Sessiontype takes values 1-6 depending on the order in which decisions were presented.
Understanding is the fraction of correct answers to eight questions testing understanding.
Columns (3) and (4) include controls.
Columns (5) and (6) include controls, and interactions between 'LowRA' and the treatment dummies.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

higher than the takeup rates of 53% in the baseline control group (T0).[73]

2.5 Conclusion

This paper examines the impact of different index insurance schemes on the takeup of new agricultural technologies. The empirical results of this paper are based on a framed field experiment on fertiliser investment decisions in cocoa farming in Ghana. The experimental design enables us to precisely calibrate the basis risk as well as to introduce the variation in the level of basis risk and premium rates. The empirical findings of this study complement the RCTs on this highly policy-relevant topic (Cole et al., 2013; Karlan et al., 2014), where measuring the basis risk is challenging, if not impossible (Clarke, 2016; Hill and Viceisza, 2011).

The results from the experimental study suggest that the effectiveness of index insurance in promoting technological innovation may depend on the specific features of index insurance schemes.While insurance can encourage investment in risky investments, the occurrence of basis risk and the level of premium rates may expose farmers to either additional risks or additional investment costs. The experimental findings of this paper suggest that fertiliser adoption rates increased under a relatively expensive index insurance insurance schemes with no basis risk. However, a relatively cheaper index insurance scheme with a positive level basis was found to be ineffective in influencing the fertiliser adoption rates.

Other framed field experiments provide mixed evidence on the effectiveness of index insurance on technological innovation (Hill and Viceisza, 2012; Carter and Galarza, 2011). The empirical discrepancy across the only two framed field experiments may be driven by the fact that the index insurance schemes are also substantially different. A number of RCTs show that premium rates and basis

[73] In our regressions we use 'LowRA' as the control for risk preferences. Our empirical results are confirmed in an alternative specification with the CRRA control (see Table 20 in Appendix).

risk are crucial factors that influence demand for weather index insurance (Cole et al., 2013; Mobarak and Rosenzweig, 2012; Hill et al., 2016). However, the difficulty in measuring and varying level of basis risk in RCTs complicates precise comparison of the relative importance of basis risk and premium rates. Our experimental design enables us to study demand for index insurance when basis risk and premium rates vary. Our empirical results suggest that basis risk may be particularly limiting factor preventing take-up of index insurance. Therefore, policies that can reduce exposure to aggregate shocks, such as investing in more weather stations (Hill et al., 2016), or selling index insurance to informal risk-sharing groups (Dercon et al., 2014), could significantly increase demand for index insurance products.

Identifying policies that are effective in raising the adoption rates of yield-improving technologies can substantially contribute to sustainable economic development and poverty alleviation. It is therefore important that experiments are conducted with the subjects from developing countries who may benefit from such polices. It is also important to better understand the decision-making of these subjects. A growing number of studies depart from the traditional assumption of rationality among the economics agents from less developed countries (Duflo et al, 2011; Bryan 2014). Our experiment elicited the risk preference in the gain and loss domain, and we found that the decisions of only 50% of the subjects could be explained by the expected utility theory. Investigating behavioural factors, such as betrayal aversion and ambiguity aversion, may improve our understanding of the factors influencing farmers´ decisions whether to adopt new agricultural technologies.

2.6 Appendix

Protocols of the farming games

The protocol of the farming game was as follows:

1. Enumerators explained the farming weather and its probability distribution.

2. Subjects were told that choosing Old technology would give GHC 6 in good weather and GHC 4 in bad weather.

3. Subjects had an option to borrow funds for fertiliser investment that would give the payoffs shown in Table 16 (depending on session type, the first farming game could give an option of T0, T1 or T2):

4. Farmers decide whether to adopt fertiliser or not.

5. The new farming game is presented (with one of the two remaining treatments) and steps 1-4 are repeated.

6. The final farming game is presented (with the last remaining treatments) and steps 1-4 are repeated.

7. Each subject determines in private which one of the five decisions made will be played out for him.

If a Gain or Loss game is drawn, then an orange/black token is drawn by each subject.

If one of the three farming games is drawn, then the weather is drawn by each subject.

8. Individual's final experimental winnings are determined both by the choices made and either by good/bad luck (if a Gain or Loss game is played out) or the weather type (if farming game played out).

As we can see in Table 16, choosing old technology always gave GHC 6 in good weather and GHC 4 in bad weather. The payoffs under a fertiliser investment varied depending on the treatment. However, a few aspects were

Table 16: Payoff Details

type of pod	weather	payoffs (TX = T0/T1/T2)	reduced form	$Takeup_{T0}$	$Takeup_{T1}$	$Takeup_{T2}$
orange	good	$6 + r - c - i - m_{TX}$	$7 - m_{TX}$	7	6	5
striped black	bad, common	$4 + 0 - c - i - m_{TX} + P_{TXcommon}$	$1 - m_{TX} + P_{TXcommon}$	1	6	5
fully black	bad, localised	$4 + 0 - c - i - m_{TX} + P_{TXlocalised}$	$1 - m_{TX} + P_{TXcommon}$	1	0	5

T0 : no insurance

T1 : cheap m, 1/8 basis risk

T2 : expensive m, no basis risk

legend:

	description	T0 calibration	T1 calibration	T2 calibration
r	fertiliser benefit	4	4	4
c	fertiliser cost (loan)	2	2	2
i	interest	1	1	1
m_{TX}	premium	0	1	2
$P_{TXcommon}$	insurance payout (bad, common)	0	6	6
$P_{TXlocalised}$	insurance payout (bad, localised)	0	0	6

common across fertiliser investments. Firstly, applying fertiliser in bad weather provided no additional harvesting income (adding GHC 0 in all treatments). Secondly, irrespective of the weather outcome, loan default was never allowed and the borrowed money ´c´ had to be returned together with the interest ´i´. This illustrates the idea by Dercon and Christiaensen (2011) that fertiliser has no harvest benefit and is a failed investment under occasional bad weather realisations.

Under T0, there was no insurance hence $m_{T0} = 0$and $P_{T0} = 0$ always (subjects were not told about any form of protection under T0). Under T1 and T2, farmers could either stick to old technology or, on top of a loan for fertiliser, pay additional an premium. Index insurance aimed to eliminate the income variability completely, as we were interested in the largest effect an insurance scheme could have on the adoption of fertiliser. Hence, an insurance payout would pay GHC 6 under both T1 and T2 if the index was triggered. However, under the T1, the index was not perfect and a localised problem would not be compensated for by the insurer; hence $P_{T1localised} = 0$. The index would always be triggered under common farming problems; hence $P_{T1common} = P_{T2localised} = 6$.

Procedures of the farming games

The procedures of the farming games were as follows:

1. Enumerators used cocoa pods and equivalently coloured marbles to describe the farming weather.

2. Several demonstration weather draws were made by the subjects.

3. The first decision problem started (either T0 or T1 or T2, depending on the session type), and subjects were given the respective illustration cards with weather outcomes and corresponding harvesting outcomes.

4. Mathematical additions and subtractions of fertiliser investment (as shown in Table 16) were demonstrated on large blackboards.

5. Large board with pod types and final payoffs both under old and first type of fertiliser technology was displayed.

6. Several additional weather draws were made by subjects; enumerators reminded them of the experimental winnings under both old and fertiliser technology.

7. Subjects were given an envelope with a sticker and decision cards with a summary of the payoff under both technologies.

8. Subjects made private decisions by placing stickers on a side of the decision cards with the preferred technology.

9. Choices were collected by the enumerators and recorded by the experimenter.

10. The Binswanger Gain game was played.

11. Steps 2-9 were repeated for the second fertiliser game (subjects were reminded that the weather is the same; eight real cocoa pods were displayed but it was stressed that the new package of fertiliser was considered).

12. Steps 2-9 were repeated for the final fertiliser game.

13. Each subject determined which one of the five choices was played out for him by drawing one of the five numbered tokens

14.The final winnings were determined depending on subjects' choices and their own random draw of black/orange tokens (if the Binswanger game was played out) or a random draw of an orange/striped/black marble (if the farming game was played out).

Table 17: Lottery Choices (Loss)

Loss (10 GHC at start)	50% (low)	50% (high)	Mean	Variance	CRRA range
A	- 10	0	- 5	25	$(-\infty)$ - 0.17
B	- 9.5	- 1	- 4.75	18,06	0.18 - 0.45
C	- 9	- 2.5	- 4.25	10,56	0.46 - 0.64
D	- 8.5	- 4	- 3.75	5,06	0.65 - 1.00
E	- 8	- 5.5	- 3.25	1,56	1.01 - 1.41
F	- 7	- 7	- 7	0	1.42 - $(+\infty)$

notes:
N: no fertiliser chosen in a given farming game.
Y: fertiliser chosen in a given farming game (either T0 or T1 or T2).
First letter in each profile is a binary choice when T0 is available.
Second letter in each profile is a binary choice when T1 is available.
Third letter in each profile is a binary choice when T1 is available.

Figure 3: Confidence Intervals (Treatment Effects)

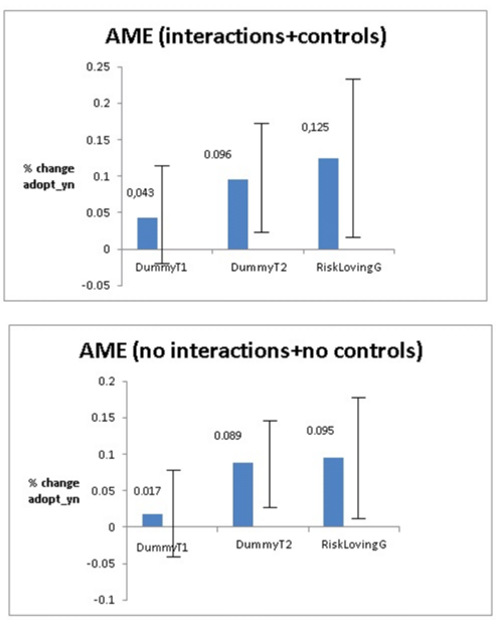


Table 18: Payoff Table

Weather	Weather Probability	Payoffs 'Old´	Payoffs 'Fertiliser´
good	0.75	6	6.5
bad & common	0.125	4	6.5
bad & localised (basis risk)	0.125	4	0.5

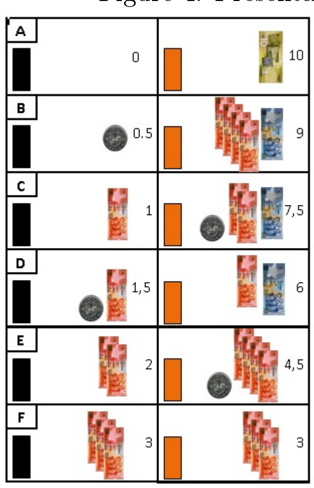
Comparisons across the two technologies:

Technology	Mean	Variance
'Old´	5.5	1.75
'Fertiliser´	5.75	2.67

Elicitation of Risk Preferences

The experiment elicited risk preferences by means of two risk elicitation games. The chosen design was one of the most common variants of the Ordered Lottery Selection originally implemented by Binswanger (1980, 1981).[74] The Binswanger risk elicitation in this experiment was conducted both in the gain and loss domain. This enabled us to investigate whether the decision making of the experimental subjects was consistent with Expected Utility Theory (EUT).

Figure 4: Presentation of Binswanger Game (Gain and Loss)



[74]There are alternative methods of eliciting risk preferences, such as the Holt and Laury procedure (2002). However, since this study was undertaken with subjects from remote rural areas with very low level of education, the Ordered Lottery Selection design is more appropriate as shown by previous studies (Barr and Genicot, 2010; Clarke, 2012).

Figure 4 shows the Binswanger game in the gain domain. Farmers were presented with a set of six lotteries (A, B, C, D, E, F) and were asked to pick the most preferred option. Each lottery consisted of two equally likely outcomes. The outcome of the lottery was determined by a draw of one of the tokens (a low outcome if a black token was drawn and a high outcome if an orange token was drawn). For instance, a subject who chose lottery A would win 0 if a black token was drawn or win 10 if an orange token was drawn (Table 19). The mean and variance were highest in lottery A and gradually decreased in subsequent lotteries, with lottery F offering the lowest mean and no variance across low and high outcomes (winnings of 3 with certainty).

Table 19: Lottery Choices (Gain)

Lottery	50% (low)	50% (high)	Mean	Variance	CRRA range	CRRA Midpoint
A	0	10	5	25	(-∞) - 0.17	0.085
B	1	9.5	4.75	18,06	0.18 - 0.45	0.315
C	2.5	9	4.25	10,56	0.46 - 0.64	0.55
D	4	8.5	3.75	5,06	0.65 - 1.00	0.825
E	5.5	8	3.25	1,56	1.01 - 1.41	1.21
F	7	7	7	0	1.42 - (+∞)	1.71

The benchmark used for analysing our data is the EUT and we consider the utility function of the following form:

$$U(x) = \begin{cases} \frac{x^{1-\sigma}}{1-\sigma} \end{cases} for \quad \sigma \neq 1; \quad U(x) = ln(x) \quad for \quad \sigma = 1 \qquad (37)$$

where x is the lottery prize and σ is the risk aversion parameter. With the specification of the constant relative risk aversion (CRRA),[75] individuals with $\sigma < 0$ are risk-loving, individuals with $\sigma = 0$ are risk-neutral, and individuals with $\sigma > 0$ are risk-averse. Under EUT, a decision-maker evaluates each

[75]We choose the CRRA form of utility (Arrow, 1965 and Pratt, 1964) for tractability and simplicity purposes An additional advantage of the CRRA functional form is the possibility to compare the elicited risk preferences with results from related experiments conducted in developing countries (e.g. Harrison et al., (2010); Teal et al., (2010); Clarke and Kalani (2011)).

outcome $k\varepsilon(1,...,N)$ using objective probabilities $p(k)$, and, hence, the expected utility is the probability weighted utility of each outcome in each lottery $i\varepsilon(1,...6)$:

$$EU_i = \sum_{1}^{N} p(k)U(k)$$

Note that this expression uses objective probabilities. The last column of Table 19 shows the range of the coefficients of risk aversion σ for which a given lottery choice is optimal. Since lottery A offered the highest mean and the highest variance, this lottery should have been chosen by the least risk-averse individuals (σ ranging from - $\infty$to 0.17). Subsequent lotteries were characterised by decreasing mean and variance, and should have been chosen by the individuals with increasing value of σ . Lottery F offered the lowest mean and zero variance, and should thus have been chosen by the most risk-averse subjects (theσ ranging from 1.42 to $+\infty$).[76]

In order to obtain the average value of the CRRA in our sample, we convert each of the six ranges of CRRA coefficients (corresponding to an individual's choice among six available lotteries) in Table 19 into a single number.[77] Specifically, we take the midpoint of the range of the CRRA coefficient corresponding to a choice of particular lottery (e.g. a choice of lottery C implies aσ range from

[76]Risk preferences were also elicited in the loss domain. The Binswanger games in the gain and loss domain were equivalent in terms of expected utility. Therefore, the ranges of the σ in the loss domain are identical to the σ in the gain domain described in Table 19. This implies that an expected utility maximiser should make identical choices in the gain and loss domain. Conversely, the choice of different lotteries in the two risk elicitation games would violate the predictions of EUT. Our experimental design enabled us to identify the proportion of experimental subjects whose decisions were not consistent with the standard model of rationality. A growing number of recent experiments relax the assumption of the EUT of decision-makers in the context of economic development (e.g. Duflo et al., 2011; Bryan, 2014).

[77]This is a standard procedure in analysing the elicited risk preferences (see for example: Tanaka et al., 2006; Liu, 2008).

0.46 to 0.64 in Table 19 and, hence, a midpoint equal to 0.55 in Table 19).[78] [79]

Our estimated value of the average CRRA is equal to 0.68. This is very similar to other recent studies which have elicited risk preferences in developing countries. Harrison et al. (2010) find an average CRRA of 0.891 in Ethiopia while Liu (2008) estimates an average value of CRRA of 0.48 in rural China. Our estimate is particularly close to the mean CRRA of 0.71 in India (Binswanger, 1980), where the risk elicitation procedure was identical to ours. Our average CRRA is also close to the value of 0.48 estimated by Caria (2010). This similarity is particularly important as both studies are based on a sample of cocoa farmers in Ghana.[80]

[78] Since the risk elicitation in the loss domain was identical to the gain domain in terms of expected utility, the midpoints of ranges in σ_L are also identical to the corresponding ranges inσ_G.

[79] The midpoints cannot be calculated for lotteries A and F, which include $-\infty$or$+\infty$. Similarly to Caria (2010), we impose a limiting value for the CRRA range corresponding to a choice of either lottery A (we choose 0 for $-\infty$) or lottery F (we choose 2 for $+\infty$).

[80] Risk preferences in our study were elicited by Ordered Lottery Selection (Binswanger, 1980), while Caria (2010) uses a different procedure (Holt and Laury, 2002). Moreover, the CRRA values for a subset of individuals might not be recovered in the Holt and Laury procedure (2002) due to multiple switching across lotteries (Caria (2010) recovers the CRRA value for approximately 74% of subjects). These factors may explain the slightly different estimated value of the mean CRRA in the two samples.

Table 20: The Impact of Insurance Schemes T1 and T2 on Fertiliser Takeup (specification with the CRRA control)

	(1) OLS	(2) Probit	(3) OLS	(4) Probit
TreatT1	0.017	0.017	0.026	0.025
	(0.650)	(0.649)	(0.409)	(0.393)
TreatT2	0.089*	0.089*	0.105*	0.105**
	(0.018)	(0.017)	(0.011)	(0.001)
CRRA	-0.009	-0.009	-0.020	-0.020
	(0.774)	(0.776)	(0.599)	(0.575)
If male			-0.021	-0.021
			(0.733)	(0.731)
Age			-0.002	-0.002
			(0.400)	(0.378)
If married			-0.017	-0.018
			(0.798)	(0.778)
Education level			-0.003	-0.003
			(0.795)	(0.771)
If household head			0.064	0.064
			(0.279)	(0.247)
Number of children			-0.013	-0.013
			(0.222)	(0.195)
Farm size (ha)			0.006	0.006
			(0.199)	(0.180)
Yield (t/ha)			0.259	0.262*
			(0.065)	(0.043)
Site			0.018	0.018*
			(0.051)	(0.022)
Sessiontype			-0.007	-0.006
			(0.693)	(0.692)
Understanding			-0.037	-0.036
			(0.753)	(0.753)
Constant	0.540***		0.584*	
	(0.000)		(0.019)	

Robust standard errors clustered at site level.

TreatT1 and TreatT2 take values 1 if the decision is made in T1 or T2, respectively, and 0 otherwise.

CRRA is a midpoint of range corresponding to the preferred Binswanger lottery.

Sessiontype takes values 1-6 depending on the order in which decisions were presented.

Understanding is the fraction of correct answers to eight questions testing understanding.

Columns (3) and (4) include controls.

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table 21: Main Results (panel estimations)

	(1) POLS	(2) RE	(3) FE
TreatT1	0.026	0.026	0.026
	(0.479)	(0.479)	(0.487)
TreatT2	0.105**	0.105**	0.105**
	(0.001)	(0.001)	(0.004)
LowRA	0.105	0.105	0.108
	(0.069)	(0.069)	(0.074)
If male	-0.031	-0.031	-0.007
	(0.623)	(0.623)	(0.918)
Age	-0.003	-0.003	-0.003
	(0.234)	(0.234)	(0.274)
If married	-0.023	-0.023	-0.024
	(0.754)	(0.754)	(0.755)
Education level	-0.005	-0.005	-0.009
	(0.738)	(0.738)	(0.567)
If household head	0.069	0.069	0.069
	(0.277)	(0.277)	(0.315)
Number of children	-0.012	-0.012	-0.010
	(0.151)	(0.151)	(0.218)
Farm size (ha)	0.005	0.005	0.007
	(0.363)	(0.363)	(0.275)
Yield (t/ha)	0.241	0.241	0.278
	(0.051)	(0.051)	(0.052)
Site	0.020*	0.020*	0.020*
	(0.016)	(0.016)	(0.028)
Sessiontype	-0.007	-0.007	-0.007
	(0.553)	(0.553)	(0.547)
Understanding	-0.019	-0.019	-0.053
	(0.858)	(0.858)	(0.647)
Constant	0.534**	0.534**	0.544*
	(0.007)	(0.007)	(0.020)
rho	0	0	0.0405

Robust standard errors clustered at site level.
TreatT1 and TreatT2 take values 1 if the decision is made in T1 or T2, respectively, and 0 otherwise.
LowRA equals 1 if A chosen in the Binswanger lottery, and 0 otherwise.
Sessiontype takes values 1-6 depending on the order in which decisions were presented.
Understanding is the fraction of correct answers to eight questions testing understanding.
rho is Intraclass Correlation Coefficient.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table 22: Randomisation test

	(1)
	OLS
LowRA	0.028
	(0.925)
If male	0.670
	(0.083)
Age	0.015
	(0.104)
maritial	-0.002
	(0.977)
Education level	-0.032
	(0.728)
hhhead	-0.052
	(0.500)
AdultsHh	-0.063
	(0.111)
Farm size (ha)	0.031
	(0.473)
Yield (t/ha)	-0.057
	(0.889)
Constant	2.792*
	(0.016)

Robust standard errors clustered at site level.

Sessiontype is the dependent variable.

Sessiontype takes values 1-6 depending on the order in which decisions were presented.

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table 23: Questions measuring subjects' understanding of the experiment

Q1	Number of games played today?
Q2	Which game paid for?
Q3	How many orange/black pods are there ?
Q4	Which one is more likely?
Q5	5+3=?
Q6	7-4=?
Q7	7*3=?
Q8	$\frac{1}{4} * 8 = ?$

Experimental Script

Good [morning/afternoon] everyone and thank you for agreeing to participate in our study. We'd like to see today how you make a particular type decisions. You don't need to know how to read. There are no 'right' or 'wrong' answers. The decisions you make will help us understand what agricultural techniques the Ghanaian cocoa farmers prefer. The information will be used by researchers in the United Kingdom. It will be used only for research purposes - for us to understand better how people in this area make decisions. Important rules Before we describe the decision problems we wish to inform you of a number of rules and practical details. • Your participation is voluntary, and you are free to leave at any point if you wish to do so. In that case we will only pay you the show up fee of GHC 2. • At some points during the session we will ask you to be silent, and not to talk to each other. PLEASE DO NOT TALK TO EACH OTHER when we ask you to make decisions. Those who do not respect this silence requirement will be asked to leave. However, until I tell you otherwise, please feel free to ask questions, or ask me to repeat myself. What will happen at the end of the session Just for showing up, you will receive GHC 2. On top of that you may earn up to GHC 10. On average you people will earn around 5 GHC on top of the guaranteed 2 GHC, but how much you earn will depend on the decisions you make and whether you are lucky or unlucky. Once the experiment is finished you will then be called individually, we will give you an envelope with your earnings and you will be asked to sign a receipt. We will not tell anyone else what your earnings were. The envelope will contain the show up fee of GHC 2 and all your additional earnings.

The money comes from a University fund. It is not our money. The number of choices you will be paid for I'm now going to explain the basic structure for the decision problems today. Over the course of this session you will be presented with five different problems (make sure they do not think of them as games but as DECISION PROBLEMS), all of which will require a choice by you. You all have 5 stickers and 5 numbered envelopes on your table which you will use to mark your choices. Please do not take the envelopes or stickers until you are told to do so. Each envelope has a paper inside in which you will be asked to place a sticker at the place you decide to mark your choice. So you will all place 5 stickers onto papers in 5 envelopes to record your 5 choices. However, we will not play each problem. We will start by describing you the first problem. You will make your first decision by placing a sticker into the paper of envelope 1. After you have made your first decision, the enumerator next to you will collect your envelope. Then we will describe the second problem to you, you will make the decision in envelope 2, enumerator will record it and we will move to third problem. We will continue until all of you have made decisions in all56 problems. At the end of the session each of you will play only one of the problems. The problem you will be paid for will be chosen randomly at the end of the session. We will place numbers 1, 2, 3, 4, and 5 face down on the table and ask you to choose one. [Demonstrate by drawing: say number 3 is picked] If the number 3 is picked we will play out your third choice and you will only be paid the show up fee and your earnings from this play of your choice in 3. [Demonstrate by drawing: say number Y is picked] If the number 5 is picked we will play out your fifth choice and you will only be paid the show up fee and your earnings from this play of your choice in decision 5. Similarly, if other number is picked [mention remaining numbers] we will play out the choice corresponding to that number, and you will only be paid for this play. Because you will not know which of these 5 problems will be paid for real money until all choices have been made you should consider each of them carefully and as if they are all for real money. Group confirmation of understanding Before we start, I have a few questions I'd like to ask all of you. If you know the answer, please say it out loud. [After every question state the correct answer]. Question to group: How many choices will you make today? [Answer: 5] Question to group: How many choices will each of you be paid for? [Answer: 1] Question to group: Do you know which choice you will be paid for? [Answer: No] Question to group: Which is the most important choice? [Answer: They all are] Question to group: What is the minimum each of you will earn for this session? [Answer: GHC 2] Question to group: When will you be paid the remainder of your earnings? [Answer: at the end of the session] [Ask if there are any questions] Description of problems I'm now going to explain the first problem to you. From this point on we ask that you do not communicate with each other or ask questions in public. You will be able to ask us questions in private after I have finished describing each problem. ***Read out individual problem sheets*** [Between every problem • Applaud/congratulate them on what they have done • Make sure they're not worried about their first decision • Emphasise that the next problem is different: 'Now we're going to make a brand new decision DECISION 1

Weather Description

Now you will make a decision related to cocoa farming. I would like you to imagine that your earnings from today will reflect your yearly harvesting season from your cocoa farm. Think of this as total number of cocoa bags that you will sell to your LBC.

Your earnings from today will depend both on your investment choices and on what weather conditions are on your farm in the given season. Please let me explain the weather patterns that you may encounter on your farm: - Weather may be good or bad - You cannot influence if the weather is good or bad - this is decided by nature - Each of you has a weather card illustrating the possible weather outcomes - please have a look at them while I explain them now

- Good weather will be represented by orange cocoa pods (show) Think of good weather as perfect growing conditions and no problems with rain, etc: this weather gives you healthy orange pods with high harvest - Bad weather will be represented by black cocoa pods (show) Think of bad weather as generally unfavourable growing conditions: problems with rain, Blackpod disease or capsids or sun: this weather gives you unhealthy pods with low harvest

- There are 6 orange pods and 2 black pods - I will put all the pods in the bag. Depending on which problem you will be paid for (remind them that they are deciding in 5 different problems today) you will draw a pod from the bag. The pod you will draw will determine the weather on your farm and hence it will affect your today's earnings (if you end up playing this problem) - For simplicity, instead of drawing large cocoa pods we will draw smaller pods from smaller bag. As I explained above there will be 6 orange pods for good weather and 2 black pods for bad weather. - There are more orange pods than black pods. This means that good weather is more likely than bad weather: you are more likely to have high harvest on your farm than bad harvest. (demonstrate a few weather draws) Problem Description Now I would like you to imagine that the raining season is approaching and you are deciding whether to invest in fertilizer that is easily available to you from your local PCs. Think of this as either Asaase Wura or COCOFED fertilizer that you have access to on your cocoa farm or not. Imagine that your earnings from this decision problem are the cash equivalent of all harvested cocoa. Your harvest will depend both on whether you invest in fertilizer, and whether the weather is good or bad.

The weather is exactly the same as I explained before. Please refer to weather cards to remember how likely is good and bad weather (ask if there are any questions). Remember that if you end up playing this problem (remember that you will end up playing just one of the 6 problems – hence you have to treat each decision carefully), then you will draw one cocoa pod from the bag. I will now explain how your winnings will vary depending both on your investment decision and weather outcome.

If you decide NOT to invest in fertilizer then at the end of the harvesting season you get - 6 GHC if orange pod is drawn (good weather) - 4 GHC if black pod is drawn (bad weather)

On the other hand, you may choose to borrow from PC to invest in fertilizer. If you invest in this fertilizer you will also be protected from certain (BUT NOT ALL) types of bad weather. You then need to repay the loan at the end of the season, independent of the weather outcome. When bad weather is drawn then your earnings will depend on whether you are protected or not. There are two scenarios under bad weather for fertilizer adopters (stress this): a) Your weather problem is LOCALISED (i.e. disease/pest at your farm only). Bad conditions were present at your farm ONLY but the general weather in the district was good. In this case you are NOT PROTECTED and you get 0,5 GHC

b) Your weather problem is COMMON (i.e. bad rain/bad sun at all farms). Bad conditions were present NOT ONLY at your farm but also the general weather in the district was bad. In this case you are PROTECTED and you get 6,5 GHC (explain MATHEMATICALLY ON BOARD different payoffs in detail)

Are there any questions at this stage? (answer all questions) Now please open the envelope 1. You should all have two different items in this envelope c) answer sheet d) sticker If you would NOT like to use this fertilizer then place the sticker on the left side of answer sheet. If you would like to use this fertilizer then please place the sticker on the right side of answer sheet with picture of fertilizer bag. (demonstrate).

Remember not to communicate with others and make a private decision without looking at other people. There are no right or wrong answers, we are just interested in how you make your own decisions. Also remember, you will only be paid for one of the 6 decisions you make today. It could be this decision, so consider your choice carefully.

DECISION 2 (Binswanger – Gain Frame) This problem is as follows. [Hand out visual aid.] You must choose one row out of A, B, C, D, E or F [point to sheet]. If you end up playing this problem you will draw a coloured card from the bag. There will be 1 black card and 1 orange card (tell subjects that black colour is– in farming problems it meant that you had bad weather, here it means that you are unlucky. Orange colour is good – in farming it meant you had good weather, here it means that you are lucky). You cannot look into the bag when you are drawing the card. [Enumerator to demonstrate] If the card you pick is black you will earn the amount next to the black token from the option you chose. If the token is orange you will earn the amount next to the orange token from the option you chose. Because you will not know which colour you will draw, it is just as likely that you will pick a black card or orange card. For example, if you choose option B, you will earn 0,5 GHC if the chosen card is black and 9 GHC if the chosen card is orange. If you choose option D, you will earn 1,5 GHC if the chosen token is black and 6 GHC if the chosen card is orange. If you choose option F, you will earn 3 GHC if the chosen card is black and 3 GHC if the chosen card is orange. Now please open the envelope 2. You have there a paper with letters A, B, C, D, E and F. Remember, you must choose one row out of A, B, C, D, E, or F by placing a sticker on the letter that you prefer [demonstrate]. If you end up playing this game you will be paid the amount next to the black card if you are unlucky and pick the black card from the bag, and the amount next to the orange card if you are lucky and pick the orange card from the bag [demonstrate]. If you have any questions please raise your hand and the next available member of the study team will come and answer your question. Once you are ready place the sticker on your preferred option, close the envelope and place it to the right side of your table.

DECISION 3

Similar to Decision 1 but the fertiliser option is now with insurance T1 (to be explained during the training of enumerators)

DECISION 4

Similar to Decision 1 but the fertiliser option is now with insurance T1 (to be explained during the training of enumerators)

DECISION 5

Similar to Decision 2 but now Binswanger elicitation is in loss domain (to be explained during the training of enumerators)

3 Chapter III

3.1 Introduction

Why do innovative insurance schemes often fail to encourage farmers in developing countries to adopt new agricultural technologies? While few development economists disagree that basis risk is a fundamental non-price factor affecting demand for index insurance schemes, academics are increasingly exploring whether behavioural factors, ignored in the standard models of rationality, may also influence technology adoption decisions.

Duflo et al. (2011) provide evidence that some farmers may be present-based and have time-inconsistent preferences. These farmers may be willing to innovate, but they defer incurring the cost until the final moment at which the technology must be employed. Cole et al. (2013) argue that trust in an insurer may also play a very significant role. Several recent RCTs show that at the start of a given season, farmers are more willing to adopt a new technology under index insurance if payment of insurance claims had been experienced in the past either by themselves or by neighbouring farmers (Karlan et al., 2014; Cai et al., 2009). Other potential factors influencing agricultural innovation under index insurance are not only the resulting increased variability of harvest income, but also the uncertainty about the actual magnitude of the risks involved (Bryan, 2014). Farmers´ decisions to invest in new technology may be affected by the fact that exact yield variation may simply be unknown.

Trust and ambiguity may be important behavioural factors that limit the impact of index insurance on promoting technological change in agriculture. Nevertheless, it is challenging to study these aspects purely by RCTs. While recent field experiments show evidence that raising trust may be effective in raising insurance demand (Cole et al., 2013; Karlan et al., 2014) , the scope of studying trust via RCTs is limited due to ethical reasons. Studying ambiguity in the field is even more challenging, since unobserved heterogeneity prevents a precise calibration of ambiguity in the field. While Bryan (2014) shows that subjects with relatively higher rates of aversion to ambiguity are less likely to be encouraged by index insurance to adopt new technology, no field study has explored the impact of an ambiguous environment on insurance demand and technological choices.

The objective of this paper is to explore trust and an ambiguous environment as potential factors influencing agricultural innovation under an index insurance scheme. By means of a framed field experiment with a subject pool of cocoa farmers from Ghana, this paper investigates the importance of still understudied behavioural factors that RCTs cannot address directly.

This paper has four main contributions: 1) Addressing trust in an insurer as a factor influencing technological innovation under index insurance; 2) addressing an ambiguous environment as a factor influencing technological innovation under index insurance; 3) investigating the effect of a reduction in an ambiguous environment on technological innovation under index insurance; 4) providing new quantitative evidence that complements RCTs in the ongoing, highly policy relevant conversations over promoting agricultural modernization in developing countries.

The remaining structure of this paper is as follows: Section 3.2 summarises the existing literature on behavioural and non-behavioural factors, which may influence the take-up of agricultural technologies under index insurance. Section 3.3 describes in detail the experimental design. Section 3.4 presents the empirical strategy. The empirical results are presented in Section 3.5. Section 3.6 concludes and discusses some policy implications of the findings.

3.2 Literature Review

Encouraging the adoption of new agricultural technologies could significantly contribute to poverty eradication in developing countries (Gollin et al., 2002). While a new technology tends to raise average yields, yield improvements are not guaranteed for all adopters in a given region. Index insurance schemes are innovative financial products which may be attractive both to farmers and to insurers. An important advantage of an index insurance scheme relative to a traditional indemnity insurance is the possibility of it addressing the information problems of moral hazard and adverse selection. The payouts under index insurance are determined by an index trigger. This occurs when a particular weather condition, crucial for the crop yields,[81] is above a pre-determined threshold. The threshold is determined at a district level; therefore, the index insurance scheme is less susceptible to the problems of asymmetric information[82] and moral hazard.[83]

While index insurance schemes may be particularly effective in addressing important information problems, an insurance product may be unattractive to farmers due to basis risk (Mobarak and Rosenzweig, 2012; Clarke, 2016). Nevertheless, Cole et al. (2013) also stress the importance of non-price factors other than basis risk, which can fundamentally affect the demand for insurance. There is a large literature which argues that trust may hugely influence the demand for financial products (Doherty and Schlesinger, 1990; Guiso et al., 2008). Trust may be particularly relevant in the context of the insurance, as subjects pay premiums upfront and receive compensation later, but only under

[81]For instance, this may be the number of sunny days during the period of drying of cocoa beans on cocoa farms in Ghana.

[82]Under index insurance payouts are given to all farmers in a given district, if the predetermined weather condition reaches the threshold (index triggered). The payouts are, therefore, not given merely to farmers, who are intrinsically less productive and have incentive to hide this from the insurer.

[83]The payouts under index insurance are determined by the index, which does not depend on the action of a particular farmer.

certain circumstances.

Another factor which can negatively affect the demand for financial products is the presence of ambiguity (Mukerji and Tallon, 2001). This occurs if risks faced by a subject are unknown. Ambiguity may influence an adoption decision, since, unlike traditional and familiar farming practices, the risks associated with new technologies may be unknown. This may be particularly important in the first years of adoption (Akay et al., 2012). Bryan (2014) notes that if a new technology is offered with an index insurance scheme, a farmer may also not know the correlation between his yields and the weather. This may substantially limit the effectiveness of an index insurance scheme in promoting the adoption of new technologies.

Trust and ambiguity may be important behavioural factors which influence technology adoption decisions under index insurance. Behavioural models relax the assumptions of rationality and self-interest.[84] For example, in a trust game,[85] the traditional models predict that none of the individuals is trustworthy, and that individuals never trust each another. However, lab experiments show evidence both for the trusting behaviour of senders and the trustworthiness behaviour of receivers (e.g. Berg et al., 1995; Falk and Kosfeld, 2006).

A subject's utility may be affected differently if, for instance, he loses money due to a random event, rather than due to a deliberate action of another subject. Bohnet et al. (2008) use a lab experiment to test whether subjects are averse to betrayal.[86] The experiment provides evidence that subject may be averse to betrayal. Despite the fact that uncertainty levels were identical across

[84]One of the core simplifying assumptions in the traditional models of decision making is that individuals are perfectly rational and act purely in their own interests.

[85]In a standard trust game (Berg, Dickhaut and McCabe (1995)), a sender, who receives an initial endowment, determines the proportion of the endowment to be sent to a receiver (trust behaviour). Subsequently, the receiver decides how much of this proportion he is willing to send back to the sender (trustworthiness behaviour).

[86]A subject is considered betrayal-averse, if a loss is caused by a random event is preferred to an identical loss caused by another subject.

the experimental treatments,[87] subjects accepted significantly less risk when it originated from other person rather than from nature.

By using experimental data on sow insurance in China, Cai et al. (2009) studied the demand for new technology under a government-sponsored sow insurance scheme.The dataset from this natural experiment shows that substantially higher rates of purchase of government sponsored insurance for sows were also found in these areas. This is interpreted by the authors as being due to a higher level of trust in the local government, which complied with its actuarial obligations. Cole et al. (2013) study insurance demand in a study with experimental variation in trust. Relative to subjects in control groups faced with unknown insurance educator, the insurance educator in the treatment group is endorsed by a trusted and well established local agent. Cole et al. (2013) found that this experimental variation substantially influences demand for index insurance, suggesting that being offered identical insurance by a trusted party can indeed influence farmers´ decisions.

It is worth noting that, apart from the fact that similar evidence for trust importance is found in studies based on RCTs (Cole et al., 2013) and natural experiments (Cai et al., 2009) studies, neither of these studies is directly relevant to the context considered here.[88] A recent RCT in northern Ghana by Karlan et al. (2014) is particularly relevant to our paper, as its key focus is also on the impact of index insurance on the adoption of new technologies. While the experiment did not involve explicit treatments related to trust, experimental subjects were revisited twice in this RCT. Karlan et al. (2014) found that demand for insurance in subsequent seasons was positively related both to farmers'

[87]Subjects assigned to control sessions played an adapted version of a trust game. Subsequently, the average probability of receiving endowment from senders was calculated, and this calibrated probability was used in risk games in experimental treatment sessions.

[88]The study by Cole et al (2013) investigates the demand factors purely for a commercial non-agricultural index insurance scheme. The experiment by Cai et al. (2009) investigates the impact of a subsidised index insurance scheme on farming investment decisions.

own experience of receiving insurance compensation and to insurance compensation received by others within the farmers´ social network. This suggests that having been compensated in the past may substantially raise trust in receiving insurance payouts in the future.[89]

Ambiguity may be another behavioural factor affecting decisions both in abstract games and in investment decisions in real-world environments. Ambiguity occurs when certain outcomes are not only subject to risk but the exact level of the risks involved is not known either. As with trust and betrayal aversion, preferences over ambiguous environment are also not present in traditional decision theory. Ellsberg (1961) proved with two thought experiments that people may be averse to lotteries involving unknown, ambiguous probabilities. The Ellsberg paradox resulting from these experiments showed that subjects may violate axioms of subjective expected utility models (Savage, 1954). Subsequently, new models of decision-making taking account of ambiguity aversion were developed, such as Choquet expected utility (Schmeidler, 1989) and Maxmin expected utility (Gilboa and Schmeidler, 1989).

A number of laboratory experiments have investigated the presence of ambiguity aversion in preferences (e.g. Magdeldorff and Weber (1994), Moore and Eckel (2003)). Our paper studies investment decisions of farmers from developing countries; hence, it is of interest whether similar patterns of preferences are also to be observed among subjects from developing countries. Akay et al. (2012) found evidence both for RA and for AA among Ethiopian farmers, yet the data was obtained only in the gain domain. Apart from identifying AA among the subjects, the authors also claim that agricultural innovation may lead to a highly ambiguous environment.[90] Warnick et al. (2011) found sup-

[89]However, Karlan et al. (2014) also note that trust in an insurer may be particularly hard to establish at the beginning when farmers have no personal experience of whether their compensation claims are going to be respected or not.

[90]While farmers using traditional technologies are familiar with yield distribution, switching to new technologies may not only involve higher variability in yields but also unknown yield

portive evidence that Peruvian farmers are also risk averse and ambiguity averse in the gain domain.[91]

Exploring quantitatively the importance of ambiguity in investment decisions is challenging in the field since the scope of controlling precisely for ambiguity levels is very limited. Bryan (2014) uses the data in Gine and Yang (2009) on Malawi groundnut farmers to study whether AA affects the uptake of new technologies under mandated index insurance. Relative to more elaborate elicitation techniques in studies such as those by Warnick et al. (2011) and Barham et al. (2014), the data used by Bryan (2014) does not enable the distinction between ambiguity-averse, ambiguity-neutral and ambiguity-loving subjects. Nevertheless, the author shows evidence that the impact of mandated index insurance on technological innovation is greater among subjects who are relatively less ambiguity averse. This suggests that ambiguity-averse subjects may indeed value the index insurance less and, hence, remain unaffected in their choice of agricultural technology in the presence of index insurance.[92]

The RCTs may provide valuable insights into the importance of trust and ambiguity in technological innovation under index insurance schemes. Nevertheless, the scope of answering certain research questions by means of this methodology is limited. First, treatments involving contract violation should not be implemented in the real world on ethical grounds, as it would generate real income losses for experimental subjects. Second, the precise calibration of insurer trustworthiness may be very difficult. Moreover, introducing exper-

distributions. The uncertainty about this distribution of harvest outcomes under innovation may discourage ambiguity-averse farmers from experimenting with new technologies in the first place.

[91]This study also links risk and ambiguity preferences with post-experimental survey data on farmers´ decisions whether to plan more than one variety of the main crop. Warnick et al. (2011) found that crop diversification is less likely among ambiguity-averse farmers, and no evidence was found that RA affects diversification decision.

[92]Bryan (2014) also found that the difference in insurance impact on technological innovation between more and less ambiguity-averse subjects is increasing in risk aversion and, interestingly, becomes negligible as farmers gain experience with new technology.

imental variation in ambiguity as part of an RCT study appears even more challenging. It is almost impossible in the field to measure precisely the level of basis risk in newly introduced index insurance schemes. Therefore, any attempt to convincingly control for ambiguous environment in technological innovation under index insurance is likely to fail. To the best of my knowledge, no RCT explicitly studies the impact of an ambiguous environment on the adoption of new technologies.

The above-mentioned obstacles faced by RCTs may be overcome by means of a framed field experiment. The latter methodology can provide a complementary picture to RCTs, as it can introduce experimental treatments in a highly controllable environment. Depending on the treatment in the lab, potential losses deducted from experimental winnings may be framed as either due to a basis risk or due to contract violation. If these two frames occur with identical probabilities, the challenge of precise probability calibration under both treatments is also resolved. Furthermore, as long as all subjects are guaranteed sufficient monetary winnings from attending the experiment, the ethical concerns of conducting treatments on trust are also addressed. Finally, although RCTs are unable to precisely measure and introduce variation in the level of ambiguity in the environment, this treatment may be easily implemented in a laboratory setting.

This paper provides one of the first pieces of field evidence documenting the importance of trust and ambiguity in agricultural innovation under index insurance. An increasing number of RCTs aim to better understand non-price factors determining the demand for index insurance. By introducing the framing effect of trust and an ambiguous environment in a controllable setting, this framed field experiment enables me to ask and potentially answer questions that are either challenging or impossible to be addressed by means of RCTs. By studying the potential demand effects of two understudied behavioural factors, this framed field experiment provides a complementary picture to the important policy debate in development economics.

Table 24: Sub-samples across Treatment Groups

	'Basis´	**'Trust´**	**'Small Ambiguity´**	**'Large Ambiguity´**	Total
subjects	116	117	117	116	466
sessions	6	6	6	6	24

3.3 Sample Description

The experiment was conducted in 12 randomly selected villages from the cocoa-growing Ashanti region in central Ghana. A total of 466 subjects were randomly selected for the experiment from lists of farmers selling cocoa harvest to all Licensed Buying Companies operating in a given village. Due to an excellent advance team from COCOBOD, attrition was non-existent in the experiment. Depending on village accessibility, experimental sessions took place in different locations, such as schools and churches.[93] There were altogether 24 experimental sessions. Four different types of sessions were conducted depending on treatment ('Basis´, 'Trust´, 'Small Ambiguity´ or 'Large Ambiguity´). Table 24 shows the number of subjects in each treatment group as well as the number of different sessions of each treatment group. Session types were allocated randomly across 24 sessions of the experiment. Six sessions of each session type were conducted.[94]

Table 25 shows summary statistics both for fertiliser non-adopters (left column), fertiliser adopters (middle column) and two combined sub-samples (right column). Standard deviations are in parentheses. Adoption rates of fertiliser in the real world are higher among married subjects and females. Adopters are also slightly older and more educated. They also have substantially higher yields, which is reasonable due to the potential of yield increases due to fertiliser adoption.

[93] We conducted only two sessions in each village (one in the morning and one in the afternoon), and the type of each session was assigned randomly.

[94] In Table 32 in Appendix we report results from regressing session types on our set of controls. The coefficients on controls are not statistically significant, suggesting that assignment to a particular session group was randomised fairly successfully.

Table 25: Descriptive Statistics

	No Fert	Used Fert	Total
Yield (t/ha)	.15	.21	.18
	(.13)	(.23)	(.18)
If household head	.78	.75	.77
	(.41)	(.44)	(.42)
If male	1.5	1.7	1.6
	(3.1)	(3.8)	(3.4)
Age	48	53	50
	(14)	(16)	(15)
Number of children	6.1	6.4	6.2
	(3.2)	(3.5)	(3.3)
Education level	3	3	3
	(1.4)	(1.4)	(1.4)

Main statistic is the mean. Standard deviation is in parenthesis.
Sample size is equal to 467.
266 farmers did not use fertiliser in the last season (column (1)).
201 farmers did not use fertiliser in the last season (column (2)).

It is very promising that the sample size included primarily head households that were middle-aged. This appears to be the group of farmers that are most likely responsible in the household for making decisions on whether to invest in fertiliser or not. This is the key question of interest in this paper; hence, this sample appears to be good for the purpose of the study. It is worth stressing that the experimental sample is relatively representative as the key sample descriptive statistics are comparable with the five-wave nationwide panel of cocoa farmers from 2002 to 2010.

3.4 The Experiments

The experiment was conducted with cocoa farmers in Ghana. Cocoa farming is the dominant cash crop in Ghana but cocoa cultivation is rather labour intensive and does not require the use of sophisticated machinery. One of the key investment decisions a farmer may consider is whether to apply fertiliser on their farm. The experimental subjects were cocoa farmers, who were asked to think of their decisions as investment choices on their farm; hence, the experiment falls into the category of framed field experiments (Harrison and List, 2004). The following subsections describe the design and implementation of the experiment.

3.4.1 The structure of the experiment

The between-subject design of the experiment implied that each experimental subject was randomly assigned to either a control group or to one of the treatment groups. Hence, each subject of the 467-subject sample made only one farming investment decision. Subsequently, the risk preferences of each experimental subject were elicited via the Binswanger (1980) procedure. Only after all the decisions were collected and the actual decision was played out was the payout determined randomly for each subject. Since it was not known until the very end which decision would determine the experimental winnings; the experimental subjects had to make careful choices in all parts of the experiment. Moreover, this experimental design did not allow for wealth effects and, hence, the design incentivised subjects to treat each decision independently. Each subject was given a 2 GHC show-up fee and he could win up to an additional 10 GHC. The eventual game earnings depended on the choices made, the decision problem that was played out, and the random elements within the decision problem that was played out.

3.4.2 Fertiliser investment games

Irrespective of the random assignment of farmers to either control group or any of the three treatment groups, each experimental subject was asked to make a binary decision in the single-season farming investment game. He could either choose 'Old´ technology or take a loan for fertiliser investment with mandated index insurance ('Fertiliser´). A farmer´s returns would be determined both by his choice of technology and by the stochastic realisation of weather. Under all treatment, 'Fertiliser´ technology had the same higher expected yields relative to 'Old´ technology. However, it was also a more risky technological choice since the fertiliser investment was entirely ineffective in bad weather. The individual weather realisation was also identical across the control and all treatments. With a probability of 0.75, the weather conditions on farmers´ land were good, implying high yields. With a probability of 0.25, the weather conditions on farmers´ land were bad, implying low yields.

The unique experimental variation was a second-stage lottery under a bad weather scenario. Bad weather could result in either basis risk (control group), a bad type of insurer ('Trust Frame´ treatment) or an uncertain likelihood of basis risk ('Large Ambiguity´ and 'Small Ambiguity´ treatments). The subsequent paragraphs describe the payoff structure and explain the experimental variation in greater detail. This is followed by a statement of the game theoretical predictions as well as the four main hypotheses of this paper.

Control Treatment T0: $Takeup_{T0}$ (Fertiliser with Basis Risk)

Subject choosing 'Fertiliser´ in the control group ($Takeup_{T0}$) would not be protected by insurance under the basis risk scenario. This would happen if bad weather turned out to be localised. The index would not be triggered; therefore, the farmer would not receive any insurance compensation. An alternative second-stage lottery scenario would be bad weather turning out to be common. This would imply that bad weather had affected the surrounding area and the index would be triggered. Under this scenario of bad weather, the farmer would receive full compensation for a failed fertiliser investment. Localised bad weather and common bad weather were equally likely to occur. Since bad weather occurred with a probability of 0.25, the probability of basis risk (localised bad weather) was equal to 0.125

Table 26: Payoff Table

Weather	**Weather Probability**	**Payoffs ‘Old´**	**Payoffs ‘Fertiliser´**
good	0.75	6	6.5
bad & common	0.125	4	6.5
bad & localised (basis risk)	0.125	4	0.5

Comparisons across the two technologies:

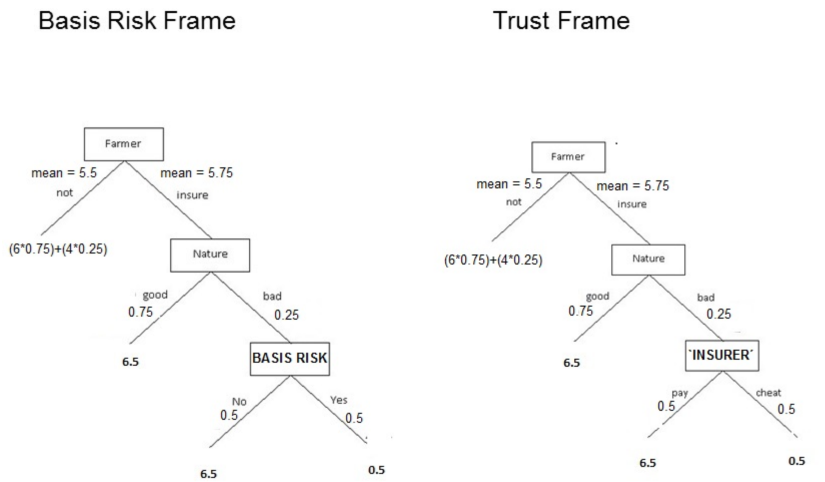
Technology	**Mean**	**Variance**
‘Old´	5.5	1.75
‘Fertiliser´	5.75	2.67

Index insurance was not available for the ´Old´ technology; hence, in this case, it was irrelevant whether bad weather was common or localised. Choosing ‘Old´ technology would generate GHC 6 under good weather and GHC 4 under both types of bad weather. Adopting fertiliser would result in yield improvement under good weather, generating GHC 6.5. Despite being a failed investment in bad weather, fertiliser adopters would still receive GHC 6.5 if the bad weather was common – implying that the index would be triggered. However, if bad weather was localised, fertiliser was a failed investment with no insurance compensation. Since the subject was still obliged to repay loan with interest and insurance premium, this would result in very low outcome of GHC 0.5. Table 26 summarises the payoffs under each weather scenario for both types of technologies (simple mathematical equations showing benefits and costs of fertiliser investment are shown in the implementation section).

Treatment T1:$Takeup_{T1}$ (Fertiliser under the Trust Frame)

The subject choosing ‘Fertiliser´ in the Trust Frame treatment group ($Takeup_{T1}$) would not be protected by insurance under a bad type of insurer. This would happen if the farmer faced bad weather but was informed that the insurer had denied insurance compensation. The experiment involved no real subjects playing the role of insurer; hence, under this treatment, the lack of insurance compensation was framed as facing a bad type of insurer (rather than facing basis risk as in the control group). Under an alternative second-stage lottery scenario, the farmer would face bad weather and a good type of insurer. Under this scenario, the farmer would be informed that the insurer type was good and the farmer would receive full compensation for a failed fertiliser investment. Bad and good types of insurers were equally likely to occur.

Figure 5: Game tree: Control and Trust Frame



Game theoretically, people should be indifferent between the risk generated by nature and the risk generated by human beings, as long as the involved risks are identical. All payoffs under a Trust Frame treatment were identical to payoffs under the control group shown in Table 26. Figure 5 shows that the probabilities of weather outcomes and second-stage lottery were also identical. This implied an identical mean of GHC 5.5 and variance of ‘Old´ technology, as well as an identical mean of GHC 5.75 and a variance of more risky ´Fertiliser´ technology. Therefore the preferences across ‘Old´ and ‘Fertiliser´technology should be identical for expected utility maximisers.

The only difference between the control group and Trust Frame treatment was whether the second-stage lottery was framed as a weather type or an insurer type. Even if the probability and size of the loss is identical, subjects´ utility may be affected differently if the source of the loss is a random event or the unethical action of another human. This treatment tests for the presence of betrayal aversion in framing. Instead of introducing the actions of real insurers, the experimental design enables us to investigate whether farmers are less likely to adopt fertiliser if a possible lack of protection is framed in terms of a bad insurer rather than in terms of localised weather.

Hypothesis I Farmers are less likely to adopt fertiliser with index insurance if the source of risk of denial of insurance claims is framed as a bad insurer type ($Takeup_{T1}$) rather than as basis risk ($Takeup_{T0}$).

$Takeup_{T1} < Takeup_{T0}$

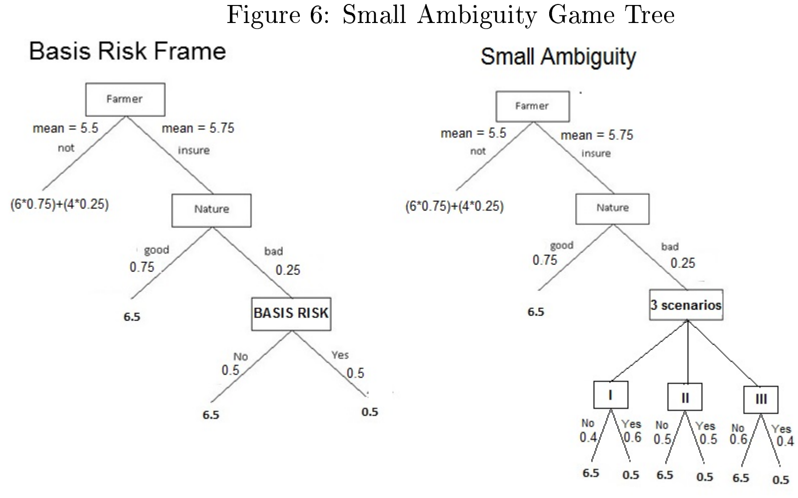
Treatment T2 :$Takeup_{T2}$ (Fertiliser under Small Ambiguity)

Subject choosing 'Fertiliser´ in the Small Ambiguity treatment group ($Takeup_{T2}$) would not be protected by insurance under the unknown probability of a basis risk scenario (localised bad weather). While subjects in the control group ($Takeup_{T0}$) knew that common and localised type of bad weather were equally likely, subjects in the Small Ambiguity treatment group knew that this probability distribution was only one of three possible scenarios. Under the second scenario, bad weather would be localised with a probability of 60% and common with a probability of 40%. Finally, under a third scenario, bad weather would be localised with a probability of 40% and common with a probability of 60%. Therefore, subjects knew an interval by which the probability of basis risk was bounded but the exact probability of basis risk remained unknown. Each of the three above-mentioned scenarios was equally likely.

Game theoretically, people should be indifferent between known and unknown probabilities of outcomes as long as the expected value of the lotteries are identical. Fertiliser adoption under Small Ambiguity treatment ($Takeup_{T2}$) involved three different possible probabilities of basis risk. However, the aggregate probability of basis risk was identical to the probability of basis risk in the control group ($Takeup_{T0}$). Therefore, the mean and variance of fertiliser investment was also identical in the treatment and control group, and expected utility maximisers should be indifferent between these two options.

The only difference between the control group and the Small Ambiguity treatment was whether the probability of basis risk was exactly known or whether it was bounded by a small interval. Even if the overall probability of basis risk was identical across the two groups, subjects´ utility might be affected differently if the likelihood of basis risk is known. This treatment tests for the presence of subjects who are ambiguity-loving or ambiguity-averse. The experimental design enables us to investigate whether farmers are less likely to adopt fertiliser if the probability of basis risk is not entirely known.

Figure 6: Small Ambiguity Game Tree



Hypothesis II Farmers are less likely to adopt fertiliser with index insurance if the probability of basis risk involves a small level of ambiguity ($Takeup_{T2}$) rather than no ambiguity ($Takeup_{T0}$).

$Takeup_{T2} < Takeup_{T0}$

Treatment T3 :$Takeup_{T3}$ (Fertiliser under Large Ambiguity)

Subjects choosing 'Fertiliser´ in a Large Ambiguity treatment group ($Takeup_{T3}$) would not be protected by insurance under the unknown probability of basis risk scenario (localised bad weather). As with the Small Ambiguity treatment, the exact probability of basis risk was not known, as it depended on three equally likely scenarios. One of the scenarios was identical to the control group ($Takeup_{T0}$) where common and localised types of bad weather were equally likely. However, under the second scenario, bad weather would be localised with a probability of 80% and common with a probability of 20%. Finally, under the third scenario, bad weather would be localised with a probability of 20% and common with a probability of 80%.

While the probability of basis risk was known in the control group, the probability of basis risk under a Large Ambiguity treatment was unknown and bounded by a large interval (substantially larger than under Small Ambiguity treatment). However, the overall probabilities of basis risk were identical, implying an identical expected value and variance of fertiliser investment in the control group and the Large Ambiguity group. While expected utility maximisers would be indifferent across these two options, an ambiguity-averse individual would prefer to invest in fertiliser where the probability of basis risk is known.

Hypothesis III Farmers are less likely to adopt fertiliser with index insurance if the probability of basis risk involves a large level of ambiguity ($Takeup_{T3}$)

rather than no ambiguity ($Takeup_{T0}$).

While an unknown probability of basis risk in Large Ambiguity treatment was bounded by a substantially larger interval relative to the Small Ambiguity treatment, the overall probability of basis risk was identical. Introducing even a relatively low level of ambiguity would discourage subjects with the highest level of aversion to ambiguity, yet the overall impact on adoption rates would be substantially higher in an environment with a large level of ambiguity.

$Takeup_{T3} < Takeup_{T0}$[95]

The empirical specification

In order to investigate these three hypotheses, I estimate the basic specification:

$$Takeup_i = Takeup_i = \beta_0+\beta_1 TreatT1_i+\beta_2 TreatT2_i+\beta_3 TreatT3_i+\beta_4 LowRA_i+X_i^{\mathrm{k}}\beta_5+\varepsilon_i \tag{38}$$

In this equation, the variables are defined as follows:

1. $Takeup_i$ is a dummy variable taking value 1 if a subject i adopted the fertiliser,

2. $TreatT1_i$ is a dummy variable taking value 1 if a subject i made the adoption decision in the treatment group T1 ($FERT_{T1}$: Fertiliser under the Trust Frame),

3. $TreatT2_i$ is a dummy variable taking value 1 if a subject i made the adoption decision in the treatment group T2 ($FERT_{T2}$:Fertiliser under the Small Ambiguity),

4. $TreatT3_i$ is a dummy variable taking value 1 if a subject i made the

[95] Given that the level of ambiguity is larger under the treatment T2 than under the treatment T1, one would expect $Takeup_{T3} < Takeup_{T2} < Takeup_{T0}$. Also, since treatment T1 did not involve any level of ambiguity (T1 treatment was based on the Trust Frame), it is not clear a priori how the takeup rates under T1 would compare to the takeup rates under T2 or T3 (according to Hypothesis I, one expects $Takeup_{T1} < Takeup_{T0}$).

adoption decision in the treatment group T3 ($FERT_{T3}$:Fertiliser under the Large Ambiguity),

5. $LowRA_i$ is a dummy variable taking value 1 if subject i´s CRRA coefficient $\sigma_i \leq 0.18$.[96]

6. X_i^k is a vector of control variables,

7. ϵ_i is a mean zero error term.

The specification (38) enables me to test Hypothesis I while controlling for Hypothesis II and for Hypothesis III. and vice versa. All three hypotheses can be tested by means of Student´s t-test (Student, 1927).

3.4.3 Implementation of the experiment

The experiment was conducted with a non-standard subject pool of Ghanaian cocoa farmers with low levels of formal education. The greatest effort was put into ensuring the understanding of the decision problems by the experimental subjects. The following paragraphs discuss how key information was conveyed to the experimental subjects.

Weather description (same across all treatments)

The experiment was designed to resemble the agricultural practices of experimental subjects as closely as possibly. Cocoa seasonal farming consists of several stages, such as planting, tree spraying, pod ripening, pods collection, beans fermenting and beans drying. At these farming stages, different weather conditions were viewed optimally. Sunny weather may be good for beans drying but not necessarily for pod ripening. Pilots of the experiment revealed that optimal seasonal conditions could not be defined purely by one weather condition. However, farmers´ understanding was clear when a high seasonal harvest outcome

[96]The CRRA coefficients were elicited by the standard Binswanger procedure: the CRRA coefficients $\sigma_i$which are interpreted as follows: $\sigma_i < 0 \implies i$ is risk-loving, $\sigma_i = 0 \implies i$ is risk-neutral, $\sigma_i > 0 \implies i$ is risk-averse.

occurred with a high number of harvested beans, and a low seasonal harvest occurred with a low number of harvested beans. Moreover, farmers associate good farming conditions with healthy orange pods producing many cocoa beans and bad farming conditions with unhealthy black pods producing little cocoa beans. Given this information, good weather was described as healthy orange pods producing many cocoa beans at the end of the harvesting season. Bad weather was described as unhealthy black pods producing few cocoa beans at the end of the harvesting season.

Good weather was calibrated at 75% and bad weather was set at 25%. However, due to the fact that the experimental sample was non-standard with subjects primarily having little formal education, use of probabilities was limited in order to maximise the understanding of the experiment. Subjects were told that six out of eight pods would symbolise good weather and two out of eight pods would symbolise bad weather. In order to familiarise the subjects with the probabilities involved, real orange pods (six orange and two black pods) were displayed throughout the experiment. Subject were also given individual visualisation depicting each weather outcome. However, the actual weather draws were made from a bag containing eight small equal-sized marbles,out of which six were orange and two were black. The small bag was used both for several demonstration draws before decision making as well as for the actual weather determination at the end of experiment. In order to ensure subjects´ familiarity with the probabilities involved, farmers could see real cocoa pods and individual weather cards at all stages of the experiment.

Payoff description (the same across all treatments)

Another common feature across all treatment groups were payoffs. Subjects were asked to think that their choice of technology together with the weather realisation would determine their seasonal harvesting income. Subjects could choose traditional, safer ‘Old´ technology. Alternatively, they could invest in new technology of fertiliser investment with mandated index insurance. This would generate a higher mean for the harvest, but also a higher harvest variability. The payoff calibrations incorporated the idea that fertiliser was effective in raising yields in good weather, yet it could be failed investment when the weather was bad (Dercon and Christiaensen, 2011). Due to the fact that no formal insurance products were present in the area of study, insurance was explained by enumerators as a protection mechanism linked to fertiliser investment. This concept was understood well by the experimental subjects.

Table 27 displays all payoffs under ‘Old´ and ‘Fertiliser´ technology. In order to ensure subjects’ understanding of the technological investments, all payoffs were explained with basic mathematical equations, including summations and subtractions. Fertiliser involved costs and benefits that were presented with reference to baseline payoffs under ´Old´ technology.

Fertiliser investment was positive in good weather leading to final payoff of GHC 6.5. Nevertheless, fertiliser was completely ineffective in bad weather. Subjects were asked to think of this scenario as corresponding to an event such as unexpected torrential rain completely washing away the fertiliser. Fertiliser investment could then give either more (6.5 GHC) or less (0.5 GHC) relative to ‘Old´ technology. The final payoff for fertiliser adopters under bad weather was determined by the second-stage lottery, where the treatment variation was introduced. The implementation of different experimental treatments is described below.

3.4.4 Treatment implementation

Following the investment decision, each experimental subject determined his individual weather realisation by drawing a marble. Drawing a black marble implied that weather on a farm was bad. While payoffs under the ‘Old´ tech-

Table 27: Payoff Table Detailed

Weather	**Weather Probability**	**Payoffs 'Old´**	**'Fertiliser´ relative to 'Old´**	**Payoffs 'Fertiliser´**
good	0.75	6	$6 + r - c - i - m$	6.5
bad & common	0.125	4	$4 + 0 - c - i - m + P_{common}$	6.5
bad & localised	0.125	4	$4 + 0 - c - i - m + P_{localised}$	0.5

Legend:

	Description	**Calibrated values**
r	fertiliser benefit	4
c	fertiliser cost (loan)	2
i	interest	1
m	premium	0.5
P_{common}	insurance payout (bad, common)	6
$P_{localised}$	insurance payout (bad, localised)	0

Comparisons of two technologies:

Technology	good (0.75)	bad & common (0.125)	bad & localised (0.125)	**Mean**	**Variance**
'Old´	6	4	4	5.5	1.75
'Fertiliser´	6.5	6.5	0.5	5.75	2.67

nology would already be known (insurance was bundled for fertiliser adopters only), payoffs under 'Fertiliser´ would need to be determined by the second-stage lottery. This additional draw from another type of bag would depend on experimental treatment.

Second-stage draw under control group (Basis Risk)

Under the control group, a fertiliser adopter experiencing bad weather would receive insurance compensation if bad weather was common. Bad weather could be common or localised (basis risk) with equal probabilities. The type of bad weather would be determined by drawing district weather from a bag containing 10 tokens.

Five black tokens represented bad district weather. Drawing a black token implied common bad weather. Under this scenario, failed fertiliser investment was compensated by insurance, implying a final payoff of GHC 6.5. Similarly, five orange tokens represented good district weather. Drawing an orange token implied localised bad weather and basis risk. Under this scenario, failed fertiliser investment was not compensated by insurance, implying a final payoff of GHC 0.5.

Second-stage draw under a Trust Frame treatment

Under a Trust Frame treatment, a group fertiliser adopter experiencing bad weather would receive insurance compensation if the type of insurer was good. The insurer could be either good or bad, with equal probability. The type of insurer would be determined by a draw from a bag containing 10 tokens.

Drawing one of five tokens with positive face expression implied a good insurer. Under this scenario, failed fertiliser investment was compensated by insurance, implying a final payoff of GHC 6.5. Similarly, drawing one of five tokens with a negative face expression implied a bad insurer. Under this scenario, failed fertiliser investment was not compensated for by insurance, implying a final payoff of GHC 0.5.

Additional draw under Small Ambiguity treatment

Under the Small Ambiguity treatment, a fertiliser adopter experiencing bad weather would receive insurance compensation if bad weather was common. The exact probability of common and localised bad weather would be determined by one of the scenarios. Under the first scenario, bad weather was common or localised with equal probabilities (one bag contained five black tokens for common bad weather and five orange tokens for localised bad weather). Under the second scenario, common weather was slightly more likely (the second bag contained six black tokens and four orange tokens). Under the third scenario, localised weather was slightly more likely (the third bag contained four black tokens and six orange tokens).

Payoffs of fertiliser adopters under bad weather would be revealed firstly by subjects´ choices across identical bags determining one of three scenarios. The subsequent draw of tokens was identical as under the control group.

Second-stage draw under Large Ambiguity treatment

Implementation of Large Ambiguity treatment was identical to the Small Ambiguity treatment except that the token composition of second and third bag was different (the second bag contained eight black tokens and two orange tokens; the third bag contained two black tokens and eight orange tokens).

The experiment aimed to better understand the farming investment decisions of Ghanaian cocoa farmers. Therefore, a lot of effort was put into ensuring that experimental subjects made decisions related to farming problems rather than abstract games. The framed protocol of the experiment encouraged subjects to think of their decisions as farming investments and that experimental winnings depended on investment choice and weather realisation. Importantly, subjects were encouraged to view different choices as investment options since costs and benefits were clearly displayed by mathematical equations written on large boards by enumerators. Subjects were incentivised to make careful decisions as average experimental winnings equalled approximately two days' wages at local rates. Weather outcomes were explained in detail to mimic the real weather outcomes faced by farmers in the studied area. A substantial use of visualisation devices was essential in ensuring high experimental understanding, including the use of real orange and black cocoa pods, symbolising either a good or bad harvest.

3.5 Experimental Results and Discussion

This part of the paper analyses the between-subject experimental data using both parametric and non-parametric empirical approaches. Firstly, the adoption decisions of control group are compared with different treatment groups using several parametric and non-parametric methodologies. These early results are then followed by regression analysis estimates where the fertiliser adoption decision is the dependent variable. Subsequently, the data are interpreted with reference to existing evidence and policy discussion on agricultural technology adoption under index insurance.

3.5.1 Preliminary Results

Parametric and Non-Parametric Tests of 'Trust´ Treatment

We first investigate whether fertiliser adoption rates (measured here as the mean of the binary adoption choice variable) differed across subjects assigned to 'Basis´and 'Trust´ groups. Depending on different distributional and variance assumptions, all tests displayed in Table 28 tested the null hypothesis of whether the means in the two samples under considerations were equal.

Table 28 shows that the mean of the 'Basis' group is 0.5897, compared to the mean of 0.4051 in the 'Trust´ group. We first test the null hypothesis of the mean equality by using parametric unpaired two-sample t-tests.[97] The null hypothesis of the mean equality is strongly rejected against the two-sided alternative, even at the 1% significance level.

This result remains unchanged in two non-parametric tests[98]: the null hy-

[97]The test is unpaired due to the fact that the design of the experiment was between-subject. Each experimental subject was assigned to either the control group or to one of the treatment groups; hence, only one fertiliser adoption decision per individual was collected and data points across to sample cannot be assigned to the same individual.

[98]The two tests are substantially less restrictive in assumptions, since non-parametric tests do not impose strong normality assumptions on the distribution of the two underlying samples. While the Mann-Whitney test stills keeps the original assumption of equal variance, the Kolmogorov-Smirnoff test does not impose either normality or variance equality assumptions.

Table 28: Mean Equality Tests (´Basis´and ´Trust´ groups)

	'Basis´ Group	**'Trust´ Group**
mean	**0.5897**	**0.4051**
standard deviation	0.0457	0.0458
observations	116	117

Parametric Tests

	t-test (equal variance)	t-test (not equal variance)
statistic	t = 2.8543	t = 2.8543
p-value	0.0047	0.0047

Non-Parametric Tests

	Mann-Whitney test	Kolmogorov-Smirnoff test
statistic	z = 2.811	K-S = 0.1846
p-value	0.0049	0.038

pothesis of mean equality is strongly rejected both in Mann-Whitney test and in Kolmogorov-Smirnoff test.

Parametric and Non-Parametric Tests of 'Small Ambiguity´ Treatment

The first key feature shown in Table 29 is that approximately 53% subjects adopted fertiliser under an index insurance scheme with a small level of ambiguity in the environment. Relative to the 'Trust ´group, this value is substantially closer to the 59 % proportion of adoption rates among subjects in the 'Basis´ group. This indicates that the ´Small Ambiguity´ treatment is less powerful than the 'Trust' treatment described above. Indeed, the unpaired t-test does not reject the null hypothesis of the mean equality among the 'Basis´and 'Small Ambiguity´groups, as the minimal significance level for rejecting the null hypothesis is 39.74%. Non-parametric versions of both types of t-tests also fail to reject the null hypothesis of mean equality. All tests suggest that there is not enough statistical evidence to claim that the sample means of the 'Basis´and 'Small Ambiguity´ groups are different.

Table 29: Mean Equality Tests (´Basis´and ´Small Ambiguity´ groups)

	'Basis´ Group	**'Small Ambiguity´ Group**
mean	**0.5897**	**0.5345**
standard deviation	0.0457	0.0465
observations	116	117

Parametric Tests

	t-test (equal variance)	t-test (not equal variance)
statistic	t = 0.8478	t = 0.8477
p-value	0.3974	0.3975

Non-Parametric Tests

	Mann-Whitney test	Kolmogorov-Smirnoff test
statistic	z = 0.848	K-S = 0.0553
p-value	0.3963	0.994

Parametric and Non-Parametric Tests of 'Large Ambiguity´ Treatment

Table 30 shows that approximately 42% of subjects adopted fertiliser under an index insurance scheme with a large level of ambiguity in the environment. This is substantially lower than the rate of approximately 59% among subjects from the 'Basis´ group. Both parametric and non-parametric results in Table 30 confirm this. The null hypothesis of the mean equality among the 'Basis´ and 'Large Ambiguity´ samples is rejected in unpaired t-tests with or without equal variance assumptions, even at the 1% significance level. Refraining from a normality assumption and performing a non-parametric version of either t-test with the variance equality assumption maintained (Mann-Whitney test) or the t-test with the variance equality assumption relaxed (Kolmogorov-Smirnoff test) confirm this finding. All tests show strong evidence that the means of 'Basis´ and 'Large Ambiguity´ are most likely not equal. This indicates that there may be a 'Large Ambiguity´ treatment effect and subjects´ decision-making may be affected by the presence of a large ambiguity in the environment.

Table 30: Mean Equality Tests (´Basis´and ´Large Ambiguity´ groups)

	'Basis´ Group	**'Large Ambiguity´ Group**
mean	**0.5897**	**0.4188**
standard deviation	0.0457	0.0458
observations	116	116

Parametric Tests

	t-test (equal variance)	t-test (not equal variance)
statistic	t = 2.6427	t = 2.6427
p-value	0.0088	0.0088

Non-Parametric Tests

	Mann-Whitney test	Kolmogorov-Smirnoff test
statistic	z = 2.609	K-S = 0.1709
p-value	0.0091	0.066

The Main Results

Table 31 presents the main results of the following empirical specification (equation 38):

$$Takeup_i = \beta_0+\beta_1 TreatT1_i+\beta_2 TreatT2_i+\beta_3 TreatT3_i+\beta_4 LowRA_i+X_i^{\mathrm{k}}\beta_5+\varepsilon_i$$

In this equation, the dependent variable is $Takeup_i$ (taking the value of 1 if fertiliser is adopted by a subject i and 0 otherwise). The binary variables $TreatT1_i$, $TreatT2_i$ and $TreatT3_i$ describe whether a subject i participated in either treatment T1, T2 or T3 respectively (1= participated in the given treatment, 0 otherwise).

To recap, treatment T1 studies the effect of trust frame.[99] Treatments T2 and T3 introduce an ambiguous level of basis risk.[100] $LowRA_i$ is a dummy

[99]Under the treatment T1 the subjects were told that the potential loss under insurance scheme would be due to insurer´s contract violation (under the control group this potential loss would be due to the basis risk).

[100]Under the treatments T2 and T3 the exact level of the basis risk was uncertain. The treatments introduced the mean preserving spread of the probability of basis risk. Under the control group the level of basis risk was known and occurred with the probability 0.5. Under the treatment T2 (´Small Ambiguity´) the basis risk could occur with the probability either 0.4, 0.5 or 0.6. Under the treatment T3 ('Large Ambiguity´) the basis risk could occur with

Table 31: Key Results: Dep Var is Takeup

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Probit	OLS	Probit	OLS	Probit
TreatT1	-0.192***	-0.205***	-0.169***	-0.182***	-0.181***	-0.182***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
TreatT2	-0.117***	-0.134***	-0.119***	-0.145***	-0.099*	-0.114*
	(0.001)	(0.001)	(0.000)	(0.000)	(0.019)	(0.011)
TreatT3	-0.258***	-0.263***	-0.251***	-0.259***	-0.261***	-0.254***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
LowRA			0.155***	0.177***	0.151*	0.254
			(0.004)	(0.006)	(0.042)	(0.146)
Yield (t/ha)			0.084	0.095	0.081	0.095
			(0.377)	(0.440)	(0.391)	(0.433)
If household head			0.063	0.059	0.061	0.058
			(0.279)	(0.253)	(0.285)	(0.251)
If male			0.004+	0.007*	0.004	0.007+
			(0.062)	(0.034)	(0.101)	(0.050)
Number of children			0.001	0.001	0.002	0.001
			(0.867)	(0.866)	(0.806)	(0.836)
Education level			-0.013	-0.012	-0.012	-0.012
			(0.287)	(0.302)	(0.332)	(0.308)
Age			-0.002	-0.002	-0.002	-0.002
			(0.136)	(0.137)	(0.119)	(0.114)
Site			0.007*	0.009*	0.007*	0.008*
			(0.028)	(0.030)	(0.045)	(0.048)
Understanding			0.171	0.186	0.166	0.174
			(0.450)	(0.366)	(0.465)	(0.393)
LowRA*TreatT1					0.101	0.018
					(0.435)	(0.932)
LowRA*TreatT2					-0.121	-0.219
					(0.275)	(0.246)
LowRA*TreatT3					0.094	-0.016
					(0.501)	(0.944)

Robust standard errors clustered at site level.

TreatT1-T3 take values 1 if the decision is made in T1-T3, respectively, and 0 otherwise.

LowRA equals 1 if a respondent chooses A in the Binswanger lottery, and 0 otherwise.

Understanding is the fraction of correct answers to eight questions testing understanding.

Columns (3) and (4) include controls.

Columns (5) and (6) include controls, and interactions between 'LowRA' and the treatment dummies.

* p<0.05, ** p<0.01, *** p<0.001

variable taking a value of 1 if the subject i's CRRA coefficient $\sigma_i \leq 0.18$. The empirical results in Table 31 are based on the four specifications: columns (1) and (2) are linear-probability models (LPM) and columns (3) and (4) are Probit models.[101] The standard errors in all the specifications are robust to heteroskedasticity and are clustered at the session level.[102]

Results: Hypothesis I ($Takeup_{T1} < Takeup_{T0}$)

Hypothesis I tests whether the fertiliser take-up under the trust-frame treatment T1 ($Takeup_{T1}$) is lower than the fertiliser take-up under the control group ($Takeup_{T0}$). This is equivalent to testing whether $\beta_1 < 0$ in the (equation 38).

Hypothesis I is strongly rejected in all specifications in Table 31. The estimated value of the coefficient β_1 is in the range of -0.169 and -0.205. Therefore, the adoption rates drop substantially by approximately 17-20% when the source of incomplete insurance coverage is explained as the insurer´s contract violation (the trust-frame treatment T1).[103] This result is significant at 1% in all columns in Table 31.

Results: Hypothesis II ($Takeup_{T2} < Takeup_{T0}$)

Hypothesis II tests whether the fertiliser take-up under the small-ambiguity treatment T2 ($Takeup_{T2}$) is lower than the fertiliser take-up under the control

the probability either 0.2, 0.5 or 0.8.

[101] Under homoscedasticity, the LPM and Probit estimators are both consistent but the Probit estimator is more efficient (Wooldridge, 2002). Nevertheless, only the LPM will be consistent under heteroskedasticity. The Probit estimator will be inconsistent unless the problem of the heteroscedasticity is not actively addressed (Greene, 2012). Since either the LPM or the Probit model is preferred under different assumptions, both models are estimated in Table 31.

[102] The statistical inference can be invalidated if standard errors fail to account for the presence of the heteroskedasticity or the correlation among the observations within the session (Wooldridge, 2002). Heteroskedasticity-robust standard errors clustered at the session level are frequently applied in the experimental data to address this issue (e.g. Barr and Genicot, 2008; Clarke and Kalani, 2011).

[103] The fertiliser adoption rates in the control group (the source of incomplete insurance coverage explained as basis risk) are equal to about 58%.

group ($Takeup_{T0}$). This is equivalent to testing whether $\beta_2 < 0$ in the (equation 38).

The data does not support Hypothesis II. The estimated value of the coefficient β_2 is always negative. When the exact level of basis risk is unknown,[104] the estimated reduction in the fertiliser adoption rates ranges between 9.9% (column 5) and 14.5% (column 4).[105] Depending on the model, the result is significant either at 1% (columns 1-4) or at 5% (columns 5-6).

Results: Hypothesis III ($Takeup_{T3} < Takeup_{T0}$)

Hypothesis III tests whether the fertiliser take-up under the large-ambiguity treatment T3 ($Takeup_{T3}$) is lower than the fertiliser take-up under the basis risk control treatment ($Takeup_{T1}$). This is equivalent to testing whether $\beta_3 < 0$ in the (equation 38).

Hypothesis III is strongly rejected in the data. The estimate of β_3 in Table 31 is always highly negative and significant at 1%. Therefore, when the level of basis risk of the index insurance is highly ambiguous,[106] the drop in the fertiliser adoption is in the range of 25.1% (column 3 in Table 31) and 26.3% (column 2 in Table 31). This is a very high impact, given that the approximate fertiliser adoption rates are 58% when the level of basis is known to equal 0.5.

Additional Results (The Risk-Aversion Dummy $LowRA$ and the Interaction Terms)

The estimated coefficient β_4 of the dummy variable $LowRA$ is always positive in Table 31. The variable $LowRA$ identifies subjects who are relatively more

[104]Under the small-ambiguity treatment T2, the basis risk probability is either 0.4 or 0.5 or 0.6.
[105]The adoption rates are about 58% when the basis-risk probability is known to be equal to 0.5 (the control group).
[106]Under the large-ambiguity treatment T3, the basis risk probability is either 0.2 or 0.5 or 0.8.

willing to choose risky investment options[107]. This result is not surprising. In the experiment, fertiliser investment ($Takeup_i = 1$) is a risky investment (it has a higher mean and higher variance than the safe investment ($Takeup_i = 0$)). The subject i, who has a preference for risky lotteries (the CRRA coefficient $\sigma_i < 0.18$), is expected to invest in fertiliser irrespective of the experimental treatment.

The estimate $\hat{\beta}_4$ in Table 31 is significant at 1% in columns 1 and 3 or at 5% in column 2. However, it is marginally insignificant in column 4 (the p-value equals 0.127). The specification in column 4 estimates a Probit model, which may be inconsistent in the presence of heteroscedasticity (Greene, 2012).[108] Importantly, the estimate $\hat{\beta}_4$ is significant in the LPM models in columns 1 and 2. Since these results use heteroskedasticity-robust standard errors, which imply a valid statistical inference (Greene, 2012), we are inclined to conclude that the effect of $LowRA$ variable is positive and significant. The fertiliser adoption rates increase by between 15.4% and 17.4% if a subject is relatively less risk averse or risk loving ($LowRA_i = 1$ implies $\sigma < 0.18$).[109]

[107]The risk preferences were elicited by the Binswanger elicitation procedure (Binswanger, 1981). Assuming the constant relative risk aversion (CRRA) utility function $U(x) = \frac{x^{1-\sigma}}{1-\sigma}$, the dummy variable $LowRA$ identifies subjects, whose CRRA coefficient $\sigma < 0.18$ (this includes all the risk-loving subjects ($\sigma < 0$), the risk-neutral subjects ($\sigma = 0$) as well as the subjects which are relatively less risk-averse ($0 < \sigma < 0.18$).

[108]The standard errors used in a Probit model are incorrect (and hence the statistical inference is invalidated) if the heteroskedasticity is not actively addressed (Greene, 2012). While modelling correctly the exact form of heteroskedasticity is challenging in a Probit model, the statistical inference in a LPM model are correct under heteroscedasticity-robust standard errors. Under the Probit model it is not possible to obtain heteroscedasticity-robust standard errors due to the model´s assumption of normal distribution (and hence non-linearity in the coefficients).

[109]In our regressions we use 'LowRA' as the control for risk preferences. Our empirical results are confirmed in an alternative specification with the CRRA control (see Table 33 in Appendix).

The specifications in column 5 (the LPM model) and in column 6 (the Probit model) in Table 31 in Table 31 also include the interaction terms between the dummy variable for risk preferences and the treatment dummies: $LowRA.TreatT1$, $LowRA.TreatT2$ and $LowRA.TreatT3$. While all these dummies are statistically significant, none of the interaction terms is significant in Table 31.

3.5.2 Discussion of the Empirical Results

This section discusses the overall empirical findings both in the trust and the ambiguity treatments and compares these with the evidence in the related literature.

The Negative Effect of the Trust Treatment ($Takeup_{T1}$)

Both regression and mean equality results provide strong evidence that a trust frame may reduce the technological innovation under index insurance. According to the empirical results in Table 31, framing the potential lack of insurance payoff as trust (and not as basis risk) reduces the fertiliser take-up by approximately 17 percentage points.

Our interpretation of this finding is that experimental subjects may be averse to betrayal.[110] The one-period feature of the experiment enabled us to rule out reputations, punishment or collusion as explanations of subjects' decisions.[111] Furthermore, our experiment involves equally calibrated risks of losses in the treatment group and in the control group. However, in our treatment, the loss is framed as if caused by a human action. This is distinct from the experimental design in Bohnet and Zeckhauser (2004), where the loss is caused by the actions of the experimental subjects. We find new evidence that betrayal aversion may be present in a trust frame. Our framing implied that decisions made by experimental subjects could only affect their own payoffs, and hence could not be

[110] Betrayal aversion is a behavioural departure from the expected utility theory (EUT). According to the EUT all subjects should be betrayal-neutral (i.e. subjects should be indifferent between losses due to nature and due to human action). The experimental evidence in Bohnet and Zeckhauser (2004) indicates that subjects may be betrayal averse. The experimental subjects are found to dislike substantially more risks of losses due to action by other subjects rather than risks of equally calibrated losses due to random event.

[111] Berg et al., (1995) note that a multiple-period trust game enables subjects to take advantage in later rounds of faking reputation for pro-social behaviour. Furthermore, players would also have a possibility to collude or to punish anti-social actions observed in earlier rounds of the game. These mechanisms are not motivated by trust and could explain investment decisions. This concern is addressed in a one-period version of the trust game addresses this concern.

influenced by other-regarding preferences or efficiency preferences.[112]

An individual may be averse to betrayal if he cares not only about his payoffs but also about how these payoffs were generated (Bohnet et al., 2008). A growing body of literature argues that utility may be affected not only by consequences but also by procedures (Rabin, 1993; Sen, 1997; Dufwenberg and Kirchberger, 2004). In our experiment, the identical consequences were generated either by a random event or by an equally likely framing of an action of an insurer. Our experimental treatment did not involve real insurers, therefore the decisions made by experimental subjects could not be influenced by intentions of others.[113] Nevertheless, our negative treatment effect can be explained by procedural utility theory (Frey et al., 2004; Stutzer and Frey, 2003). Our results could possibly be driven by subjects' aversion to procedures that involve betrayal. However, we also do not rule out a possibility that aversion to procedures distinct from betrayal could conceivably explain the pattern of decisions we observe in our data. For example, if our experimental subjects dislike institutions (such as insurance providers), they may prefer to experience an identical loss that is generated by nature.

Our experimental design aimed to identify the lower bound of the treatment effect; hence, the framing of the treatment was neutral (i.e. 'insurers´). One could expect a stronger treatment effect under a negative loading of the

[112] A number of theoretical models and experimental evidence suggests that decision makers may care not only about their own payoffs, but also be altruistic (Andreoni and Miller, 2002), be averse to inequality (Bolton and Ockenfels, 2000), or be concerned out efficiency of outcomes (Charness and Rabin, 2002). In our experiment, a farmer's decision to enter an insurance contract could not affect payoffs of the insurer. Therefore, this decision should be motivated only by his own payoffs.

[113] Numerous studies find experimental evidence that a subject' utility and his response (either positive or negative reciprocity) to an action of the opponent may depend not only on the opponent's action but also on the intention behind this action (for example McGabe et al., 2003, Stutzer and Frey, (2003), Falk et al. (2006)). Falk and Fischbacher (2003) develop a theoretical framework in which a reciprocal behaviour can be explained both by outcomes (as in outcome-based models by Fehr and Schmidt, (1999), Bolton and Ockenfels, (2000)) and by intentions (as in intention-based models by Rabin (1993), Dufwenberg and Kirchsberger (2004)).

framing (e.g. ‘cheating insurers´ or ´bad insurers´).[114] Burnham et al. (2000) found evidence that negatively loading the trust game (i.e. describing players as ’strangers, as opposed to ’partners’) significantly changes both the trusting and trustworthiness behaviour.

Several related studies also investigate the framing effects in experimental samples of subjects from developing countries. Ross and Mittel (1998) found in a laboratory experiment that the demand for insurance is significantly affected whether an identical investment option is presented as an opportunity or as a threat. In a field experimental setting in South Africa, Bertrand et al. (2010) also found a strong framing impact on credit demand. Nevertheless, Cole et al. (2013) did not find a differential effect where index insurance offered in India is explained to pay in two out of 10 years relative to not paying in eight out of 10 years. The existing experimental evidence on framing effects among the experimental samples from developing countries appears to be context specific.

Our experimental findings indicate that trust may be a very significant factor reducing the take-up of new technologies under index insurance schemes. Our framing treatment revealed that subjects dislike it substantially more if insurance payouts are not received due to an insurer´s decision rather than due to an equally likely basis risk. These results are valuable and complementary to the findings emerging from recent RCTs that explore other reasons for low take-up of insurance. Certain trust treatments of interest would be unethical in the RCTs if subjects could experience losses. The trust treatment in our framed field experiments still ensured positive winnings for all subjects at the end of the experiment.

Our findings add more information to an important policy discussion. While

[114] One could also possibly expect a higher impact on subjects´ behaviour if the trust framing was replaced by an experiment involving real insurers. Bohnet et al. (2008) found evidence for betrayal aversion in such an experimental setting, and argue that the treatment effect could be magnified under different calibrations of probabilities in the studied games.

basis risk is often regarded as a crucial factor reducing the demand for index insurance, trust may be equally important. Investments in insurers' credibility may thus be as essential as investments in more precise indices reducing basis risk. If farmers are particularly reluctant to get insurance due to the lack of trust in insurers, policies aiming at raising the level of trustworthiness of insurance schemes could also successfully improve the demand for index insurance. A recent RCT by Cole et al. (2013) identified one policy successfully addressing the trust issue. While the index insurance studied by Cole et al. (2013) is not linked to technology, the demand for index insurance improves significantly when farmers are informed about an insurance policy by a trusted local organisation. While two other important RCTs (Karlan et al., 2014, and Cai et al., 2009) did not introduce trust interventions as part of their experimental treatment,these studies found evidence that observing insurance payouts in a given area in first season encourages farmers to purchase more index insurance in subsequent seasons.

A potentially successful policy would be to design index insurance contracts paying insurance claims of lower amounts but with sufficient frequencies. However, as noted by Cole et al. (2013), the scope of this approach should be limited. This is because farmers can typically manage small variations in weather through a variety of coping mechanisms, so that insurance payouts are really needed primarily to help manage weather scenarios that are less frequent but more severe. An alternative policy would be to consider selling insurance at the meso level, such as to cooperatives or to an entire village. Group management is more likely to be better educated and familiar with financial products and less biased with respect to (mis)trust in the insurer. Once index insurance is implemented in the early seasons and individual farmers are more experienced with index insurance and possibly receive insurance payouts, index insurance may later be more easily sold at the individual level. If index insurance is linked to technology adoption, it can significantly contribute to the aggregate agricultural modernisation.

The Negative Effect of the two Ambiguity Treatments

The experiment introduced ambiguous environment in the payoff distribution with the small ambiguity treatment T2 (the probability of basis risk equal to either 0.4 or 0.5 or 0.6) and the large ambiguity treatment T3 (the probability of basis risk equal to either 0.2 or 0.5 or 0.8). These treatment effects enables us to test for the presence of the ambiguity-aversion.[115]

The empirical results in Table 31 suggests that both the ´Small Ambiguity´ and the ´Large Ambiguity´ treatments reduce the demand for fertiliser under index insurance.[116] The estimated reductions in the fertiliser adoption rates are in the range of 9.8% and 14.6% under the ´Small Ambiguity´ and in the range of 25.1% and 26.3% under the 'Large Ambiguity´.

Our results suggest that an ambiguous environment may be a strong factor in addition to basis risk for reducing the effectiveness of index insurance in raising the adoption rates of new technologies. Akay et al. (2012) argue that the initial lack of knowledge of the yield distribution may discourage farmers from experimenting with the new technology. The farmers would not know the correlation between bad weather and yield. Index insurance may not be attractive due to the resulting ambiguity in the exact level of basis risk.

Bryan (2014) provides the first empirical evidence showing that ambiguity

[115]Distinction between known probabilities and unknown probabilities was first proposed by Knight (1921) and Keynes (1921). Ellsberg (1961) was first to show that decision makers may be affected by lotteries characterised by unknown (i.e. ambiguous) probabilities (Mas-Colell et al., 1995). Ambiguity-aversion is a behavioural departure from the expected utility theory (EUT). According to the EUT all subjects should be ambiguity-neutral (i.e. subjects should be indifferent between lotteries which have the same mean but different level of uncertainties with respect to the probabilities).

[116]In the control group the subjects new the exact probability of basis risk).

may negatively affect the uptake of new technologies. Based on elicited preferences for ambiguity in a RCT (Randomised Controlled Trial) among the Malawi maize and groundnut farmers (Gine and Yang, 2009), Bryan (2014) identified the subjects who were relatively more ambiguity averse. These farmers were then found to be less likely to adopt new technologies under index insurance.

Studying ambiguity by means of the RCTs is particularly challenging, however, since it is effectively impossible to precisely measure the ambiguity. It would be at least equally challenging to introduce a treatment variation in the level of ambiguity. However, precise calibration and variation in the level of ambiguous environment can easily be implemented in controlled laboratory settings. Our experimental design enabled us to precisely measure the ambiguity by introducing a mean preserving spread of the basis risk probability. Our experimental treatments also introduce variation in this level of ambiguity (the ´Small Ambiguity´ and the ´Large Ambiguity´ treatments). To the best of my knowledge, this is the first paper studying technology adoption choices under index insurance with a precisely calibrated ambiguous environment. The experimental findings provide a complementary picture to this early RCT evidence in Bryan (2014) on a still understudied area of ambiguity impact on technology adoption.

There could potentially be alternative explanations of our empirical results. It could be the case that our negative treatment effect is not due to aversion to ambiguity but due to subjects' lack of understanding of the experiment. In order to address this concern we measured subjects' level of understanding, and used it as one of explanatory variables in our regressions. However, the negative treatment effect we identify could be interpreted not as aversion to ambiguity but as aversion to compound lotteries.[117] The main focus of our

[117]A compound lottery is a two-stage lottery. A subject is averse to a compound lottery if he prefers a probabilistic equivalent single-stage lottery (Machina, 1989).

experiment was to study in detail farmers' fertiliser investment decisions under index insurance. We abstained from eliciting preferences towards ambiguity and compound lotteries, as this would significantly prolong experimental sessions. Moreover, aversion to ambiguity is closely related to aversion to compound lotteries. A subject is confronted with unknown (ambiguous) probabilities if he cannot reduce compound lotteries to a single lottery (Segal, 1987). A number of recent experimental studies (e.g. Halevy, 2007; Chew et al., 2017) show that subjects who are averse to compound lotteries are also averse to ambiguous lotteries. We find evidence that farmers may be less likely to adopt fertiliser under index insurance schemes that include risks that we interpret as either compound or unknown.

A successful policy which may address the problem of ambiguity in index insurance may be to subsidise initially the premium rates (Bryan, 2014). Farmers may be more willing to experiment with the new technology at subsidised premium rates. Over time, as they understand better the yield distribution and its correlation with the weather, the premium subsidies can be lowered and eliminated. Bryan (2014) found that the difference in the adoption rates between relatively more ambiguity-averse farmers and less ambiguity-averse farmers is decreasing with experience with new technology.

The results from our experimental treatments are in line with these findings. The reduction in the fertiliser investments were lower under the ´Small Ambiguity´ treatment relative to the ´Large Ambiguity´treatments. The short-term subsidised premium rates may encourage farmers to start experiment with new technologies. As the farmers gain more experience with the new technology, the farming environment becomes less ambiguous and the subsidies could be removed in the long run.

3.5.3 External Validity

This experiment focused primarily on the context of agricultural investment decisions; hence, it is essential that the experimental data was conducted in the area where future policies might be implemented. The aim of the experiment was to understand the choices of farmers from LDCs, and hence the external validity of this experiment should be viewed on the spectrum of developing countries. Ghanaian cocoa farming is rather small-scale, with little agricultural intensification, low productivity or low aggregate adoption rates of new technologies. Therefore, the experimental findings may be less applicable to those developing countries with agriculture characterised by large farms and capital intensity (parts of Latin America) or high agricultural productivity (parts of East and South East Asia). The results may perhaps be more generalisable to developing countries dominated by agricultural sectors with similar characteristics (for instance other African countries).

One of the key benefits of framed field experiments is the possibility to study the impact of a particular factor on the variable of interest in a controllable laboratory environment. Under all treatments of this experiment, subjects could choose between traditional technology with low yields or fertiliser with mandated index insurance (as in the experiment of Hill and Viceisza, 2012). Moreover, fertiliser investment in the experiment was financed with loans, and loan defaults were not allowed either. This depicts accurately a popular Abrabopa credit programme for fertiliser operating in Ghana, with highly successful repayment rates exceeding 90%. Limited liability was not studied in this paper, although it is also a possible factor limiting the effectiveness of insurance schemes linked to loans (see, for example, the Malawi maize farmers studied in Gine and Yang, 2009). Finally, the experimental subjects made private decisions and only played one round of the fertiliser investment games. The literature shows that farmers learn about the benefits of technology either from their accumulated experience over time (Conley and Udry, 2010) or from informal social networks (Mobarak and Rosenzweig, 2012). Studying peer learning, network or peer effects would substantially prolong and complicate the design. This experiment abstracted from issues such as learning and limited liability in order to focus more directly on aspects of trust and ambiguity that are still understudied in the development literature on technology adoption.

One of the challenges of any experiment studying index insurance is to explain complex basis risk to subjects. In this experiment, basis risk was explained with concepts of localised bad weather (basis risk occurs if only an individual farm is affected) and common bad weather (basis risk does not occur when farms on district level are affected). Similar descriptions of basis risk were also chosen in Clarke and Kalani (2011). This concept was clearly understood by participants, despite low levels of formal education. Cole et al. (2013) stress in their RCT that it is essential that an insurance educator spends enough time to explain how index insurance works. This approach was not taken in our framed field experiment, as it would have significantly prolonged experimental sessions and possibly would not have succeeded in ensuring sufficient understanding within a reasonable time. However, the details of the index were not specified either. In an experimental game, it was sufficient to give participants a rather general exposition of the insurance product and associated basis risk; this would not have been sufficient in an RCT in which subjects were making decisions about their actual livelihoods. For the purposes of FFE, the experimental results are generalisable to many different types of index insurance where index could be based either on yield on different types of weather or on a combination of yield and weather indices.

This paper focuses on the behavioural limits of index insurance in increasing the adoption of new technologies; therefore, a baseline index insurance with a moderate fixed basis risk of 0.125 was chosen. The study investigated whether factors other than basis risk may also play a role in discouraging technological adoption under index insurance. The trust treatment was a framing experiment where non-payment of claims by an insurer was had a neutral framing . Since treatment could be framed negatively or it could involve real insurers, one could argue that our finding is at the lower end of the scale for the negative impact of trust on insurance demand. Two different ambiguity treatments studied the role of index insurance in raising technology adoption if the environment was moderately or highly ambiguous.

Finally, it should be stressed that the role of this experiment was to complement a growing number of field experiments and provide new information about a highly popular policy discussion. While studying trust and ambiguity is very challenging in the context of RCTs, it is significantly easier to study in the highly controllable environment of a laboratory-in-field experiment. This experimental design enables us to investigate the importance of trust and ambiguity in influencing the effectiveness of index insurance in promoting agricultural innovation in developing countries.

3.6 Conclusion

This paper identifies betrayal-aversion and ambiguity-aversion as two behavioural factors which may reduce technology adoption under index insurance. The empirical results are based on a framed field experiment conducted with the cocoa farmers in Ghana. Studying trust and ambiguity by means of randomised controlled trials is challenging, if not impossible. Our experimental treatments introduce trust framing as well as precisely calibrating and varying the level of ambiguity. The findings provide new evidence that contributes to a growing policy discussion on how rural households in developing countries could benefit from promising index insurance schemes. Organisations familiar to farmers providing education on insurance (Cole et al., 2013) or building insurance regulatory frameworks may successfully improve trust. An initial experience with insurance payouts may also encourage farmers to view index insurance more reliably in future (Karlan et al., 2014). An ambiguous environment may also discourage farmers from innovating under index insurance schemes. The short-term subsidies of the insurance premium rates may reduce the level of ambiguity associated with the level of basis risk. Once farmers have more experience with new technology, the ambiguity gradually decreases and subsidies will no longer be necessary.

3.7 Appendix

Protocol and procedures

The experiment was conducted with a non-standard sample of 466 cocoa farmers from the Ashanti region in Ghana. All experimental sessions were conducted in the local Twi language by three enumerators. Enumerators were well trained during the several pilots undertaken in the cocoa villages close to the capital city, Accra. Enumerators also kept the same roles in all experimental sessions. An

Table 32: Randomisation test: Dep Var is Session Type

	(1) OLS	(2) Probit
Yield (t/ha)	0.004	0.002
	(0.380)	(0.700)
If household head	-0.080	-0.015
	(0.528)	(0.946)
If male	0.004	0.284
	(0.666)	(0.123)
Age	-0.000	-0.000
	(0.910)	(0.928)
Number of children	0.008	0.001
	(0.617)	(0.968)
Education level	0.051	0.042
	(0.167)	(0.358)
overall p-values	0.788	0.550

Dependent variable is Session Type.

Session Type takes values 1-3 for treatment sessions T1-T3, respectively, and 0 otherwise.

Overall p-values correspond to tests of overall significance in regression.

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

average experimental session took approximately 75 minutes and was followed by a short 10-minute questionnaire about general household characteristics and farming practices. Experimental subjects were given 2 GHC for showing up and they could win up to an additional 10 GHC depending on their choices and luck. The average experimental winnings were 8 GHC, which equalled approximately a two-day wage in the studied area. Instead of pen-and-paper methodology, subjects´ answers were collected with stickers and envelopes. Each envelope consisted of an answer sheet, where subjects would record their answers by placing a sticker in the indicated space. This method ensured privacy in decision-making as well as in the effective collection of responses by enumerators (we found in pilot sessions that many illiterate subjects struggled with recording their choices with a pen).

Enumerators assisted subjects by first showing on a large visualisation aid

Table 33: Results: Dep Var is Takeup (risk control is CRRA)

	(1) OLS	(2) Probit	(3) OLS	(4) Probit
TreatT1	-0.185***	-0.198***	-0.172***	-0.184***
	(0.000)	(0.000)	(0.000)	(0.000)
TreatT2	-0.115***	-0.133***	-0.121***	-0.144***
	(0.002)	(0.001)	(0.000)	(0.000)
TreatT3	-0.252***	-0.258***	-0.256***	-0.263***
	(0.000)	(0.000)	(0.000)	(0.000)
CRRA	0.104*	0.110*	0.103*	0.107*
	(0.031)	(0.026)	(0.047)	(0.043)
Yield (t/ha)			0.089	0.104
			(0.361)	(0.412)
If household head			0.070	0.067
			(0.227)	(0.190)
If male			0.004+	0.007*
			(0.060)	(0.043)
Number of children			0.001	0.000
			(0.937)	(0.962)
Education level			-0.014	-0.013
			(0.255)	(0.260)
Age			-0.002	-0.002
			(0.139)	(0.143)
Site			0.008*	0.009*
			(0.026)	(0.029)
Understanding			0.134	0.140
			(0.550)	(0.491)

Robust standard errors clustered at site level.
TreatT1-T3 take values 1 if the decision is made in T1-T3, respectively, and 0 otherwise.
CRRA is a midpoint of range corresponding to the preferred Binswanger lottery.
Understanding is the fraction of correct answers to eight questions testing understanding.
Columns (3) and (4) include controls.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table 34: Questions measuring subjects' understanding of the experiment

Q1	Number of games played today?
Q2	Which game paid for?
Q3	How many orange/black pods are there ?
Q4	Which one is more likely?
Q5	5+3=?
Q6	7-4=?
Q7	7*3=?
Q8	$\frac{1}{4} * 8 = ?$

where the sticker should be placed depending on the chosen option. This method was very successful as a proportion of sample size was illiterate and would have struggled with recording their choices with a pen. Moreover, subjects paid more attention to placing the sticker on their answer sheets and passing the envelope to the enumerator.

Ensuring privacy in the decision-making was one of the core objectives of the experiment. In addition to the sticker-and-envelope method of recording answers, subjects were randomly allocated across numbered and spaced out chairs at the beginning of each session. No talking was allowed during the decision-making. However, the enumerators´ role was to explain the games as clearly as possible. Group understanding questions were asked during the early parts of the experiment, and each subject could later ask enumerators private questions by raising their hands. Large visualisation aids included boards and blackboards for payoff descriptions, and subjects were given individual visualisation aids too. In farming investment problems, real cocoa pods were used to depict the farming environment more closely and to explain the probabilities of different weather outcomes more clearly. Another key feature of the experiment was that experimental subjects made all their decisions prior to knowing the actual weather situation that would affect their winnings. This enabled us to control for wealth effects, and the probability distribution related to each decision problem was explained by a number of demonstration draws made by experimental subjects.

Protocol of the Farming Investment Decisions

The protocol of the farming game was as follows:

1. Enumerators explained the farming weather and its probability distribution.

2. Subjects were told that choosing the Old technology would give GHC 6 in good weather and GHC 4 in bad weather.

3. Subjects had an option to borrow funds for fertiliser investment that would give payoffs shown in Table 27.

4. Subjects were told that under fertiliser adoption, the payoffs under bad weather would be determined by a second draw.

5. Different reasons for a second draw were explained, depending on the treatment session ('Basis´, 'Trust´, 'Small Ambiguity´ or 'Large Ambiguity´).

6. Farmers decided whether to adopt fertiliser or not.

7. If the farming investment decision determined individuals´ final experimental winnings, these were computed both by the choices made and weather draw.

Procedures of the Farming Investment Games

The procedures of the farming games were as follows:

1. Enumerators used cocoa pods and equivalently coloured marbles to describe the weather.

2. Several demonstration weather draws were made by subjects.

3. Enumerators used tokens to describe the second-stage draw under fertiliser investment.

4. Payoffs in Table 27 were shown on a large board by mathematical additions and subtractions.

5. Large board with pod types and final payoffs both under 'Old´ and 'Fertiliser´ technology were displayed.

6. Several additional weather draws were made by the subjects.

7. Subjects were given an envelope with stickers and decision cards and a summary of payoffs under both technologies.

8. Subjects made private decisions by placing stickers on a side of the decision card with the preferred technology.

9. Choices were collected by the enumerators and recorded by the experimenter.

10. Each subject determined which one of the experimental games was played out for him by drawing one of the numbered tokens.

11. Final experimental winnings were determined depending on the subject´s decision and his draw of random weather.

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Thesis conclusion

The objective of this doctoral thesis was to explore the limitations to the adoption of yield-improving technologies in developing countries. The case study for this empirical work examines the adoption of fertiliser by cocoa farmers in Ghana. The analytical results are based on panel data estimations (Chapter 1) and experimental methods (Chapter 2 and Chapter 3).

The regression results in Chapter 1 do not provide sufficient evidence for heterogeneity in comparative advantage that would explain the fertiliser adoption decisions of Ghanaian cocoa farmers in the period spanning from 2002 to 2010. The results do support the idea that fertiliser is a technology with high returns, posing a puzzle about why adoption rates are relatively low. One recurring question that the literature addresses is whether the failure of farmers to take up new technologies lies in risk aversion. Particularly for poor farmers, a negative income shock may have devastating effects. Index insurance is a possible solution to the problem of risk facing farmers, and many development agencies have pursued index insurance solutions over the past few years. However, take-up rates for index insurance itself are low, and in Chapters 2 and 3, this thesis explores possible reasons for the low demand for index insurance. The framed field experiments in Chapter 2 and Chapter 3 analysed farmers´ adoption decisions on credit bundled with different index insurance schemes. Interestingly, irrespective of whether fertiliser investment could be undertaken purely with credit or with additional insurance, the adoption rates of experimental subjects in the lab were always above 50%, substantially higher than their adoption choices in real life. This provides some indication that access to credit is still an important factor enabling technological innovation. Nevertheless, echoing ideas in the field experiment by Duflo et al., (2009), it is unlikely that credit constraints are the only reason behind low adoption rates of yield-improving technologies in developing countries.

The experiment's results in Chapter 2 indicate that bundling fertiliser investment with insurance may be effective, but the presence of basis risk appears to be a substantially more discouraging factor relative to the level of premium rates. The experiment's results in Chapter 3 suggest that behavioural factors, such as betrayal aversion and ambiguity aversion, may also be important factors behind the low take-up of index insurance for technological innovation.

This thesis provides a set of new information that contributes to an important policy discussion regarding the effectiveness of index insurance in encouraging technological innovation in developing countries. The empirical results suggest that while basis risk is a important detrimental factor, behavioural factors may also explain the low take-up of index insurance observed in several recent RCTs. Farmers´ trust in insurers may play a key role, and Cole et al. (2013) suggest either selling insurance at the meso level (i.e. at the village or cooperative level rather than at the farmer level). Another alternative is redesigning insurance contracts to ensure smaller and more-frequent payouts, particularly in the early years of insurance products. Recent findings from northern Ghana indicate that 'demand for insurance in subsequent years in strongly increasing with the farmer´s own receipt of insurance payouts, with the receipt of payouts by others in the farmer´s social network and with recent poor rain in the village (Karlan, et al., 2014). Nevertheless, this should be weighted with the fact that the highest insurance payments are needed by farmers precisely in those years with the most adverse weather conditions (Cole et al., 2013; Clarke and Mahul, 2012).

Another behavioural factor potentially limiting the take-up of index insurance with new technologies is ambiguity aversion. Lack of information about the distribution of yields under new technology and under various weather and market conditions implies that farmers face real uncertainty about the extent of basis risk. Aversion to this lack of information would render index insurance less attractive for ambiguity-averse factors. Potential interventions that could address this issue are short-term subsidies aiming at improving farmers´ understanding of yield variations under new technologies or contract stability preventing insurance companies from adjusting contract details across years. It should be stressed that continued research should focus on understanding not only which polices could work in developing context but also which ones could be most effective, assuming that in addition to being ambiguity averse, some farmers are also present biased. Under this behavioural deviation from standard models of rationality, both short-term subsidies and discount vouchers could address the problem. Nevertheless, the latter intervention would be substantially more efficient, such as the case of Kenyan maize farmers where ´fertiliser vouchers offered at harvest for next season increases adoption by 17% and this effect is about equivalent to 50% subsidy to the fertiliser price (Duflo, et al., 2009).

To sum up, this thesis has advanced our understanding by showing that two important behavioural mechanisms, hitherto not documented under field conditions, could be important sources of explanation for low take-up of new technologies. In principal, both mechanisms are amenable to policy interventions. Approaches that ignore the importance of trust and betrayal, on the one hand, and ambiguity aversion, on the other hand, may very possibly fall short of their expected aims. Further research is needed to understand better the ways in which agricultural development interventions can most effectively (and most cost effectively) address these issues.