Individual preferences and farm-level decisions

Experimental evidence from Costa Rica

María Angélica Naranjo Barrantes

Propositions

1. Risk preferences vary across different contexts. (this thesis)

2. Farmers' risk preferences in the context of fertilizer application reflect an endowment effect. (this thesis)

3. Organic production should not be promoted among small-farmers in developing countries without a rigorous analysis of the consequences for their livelihoods.

4. The best way to experience cultural traditions is through food.

5. We need to find ways for people on Earth to experience the astronaut overview effect.

6. In chess, the player who loses is the one who learns the most.

Propositions belonging to the thesis entitled:

"Individual preferences and farm-level decisions: Experimental evidence from Costa Rica."

María Angélica Naranjo Barrantes

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María Angélica Naranjo Barrantes

Thesis committee

Promotor
Prof. Dr Erwin H. Bulte
Professor of Development Economics
Wageningen University & Research

Co-promotor
Dr Janneke Pieters
Assistant Professor of Development Economics Group
Wageningen University & Research

Other members
Dr Stefania Bortolotti, Max Planck Institute for Research on Collective Goods, Germany
Prof. Dr Fredrik Carlsson, University of Gothenburg, Sweden
Prof. Dr Miranda Meuwissen, Wageningen University & Research
Dr Andries P. Richter, Wageningen University & Research

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To my family.

Contents

1

Introduction

1.1 Overview

Agricultural activities are characterized by a high variability of returns. The risk of crop-failure is determined by several factors: weather variability, natural disasters, and changes in market and input prices, to name a few. Smallholder farmers in developing countries are particularly vulnerable to these risks, which may have extreme consequences for their livelihoods (Cervantes-Godoy et al. 2013). A strategy to manage risks in agriculture starts with decisions at the farm— which crops to grow, land allocation, the use of inputs, technology adoption and diversification with other on and off-farm activities (OECD 2009).

This thesis focuses on farmers' decisions at farm-level to manage risks. It studies the individual decision-making in interaction with the institutions and government policies that can influence these decisions. It assesses how individual preferences shaped these decisions, and endeavor to show how, through understanding farmers' choices, research can contribute to policy design in agriculture. It presents experimental and quasi-experimental evidence from coffee farming in Costa Rica. It starts by looking at the alternatives for eliciting risk preferences in the field and how the estimates of risk preferences relate to farm-level decisions. Next, it explores social preferences and farmer cooperation with agricultural cooperatives. Then, it examines the interaction between financial instruments and governmental support as the effect of farmers' liability on the demand for credit with and without insurance. Finally, it presents evidence on the environmental impacts of organic certification as a farm-level strategy looking to increase premiums in-hand with conservation awareness.

Although each chapter is intended as a stand-alone contribution to the literature, they all address individual preferences and decisions at farm-level to manage risks. Regarding individual preferences, the emphasis is on studying risk preferences as farmers' willingness to take risks, and social preferences as farmers' willingness to contribute to a public good, and their relations with real-life behavior. Regarding farm-level decisions, emphasis is placed on investments, farm management practices, and formal market-based strategies, such as credit, insurance, and certification schemes, that can be adopted by farmers to manage risks. Overall, this thesis enhances the understanding of individual preferences and decision making at the farm-level and its implications for policy design.

1.2 Risk preferences

Assessing risk preferences is key to understanding decisions at farm level. Farmers choose between options that are not necessarily certain, whose outcomes have a probability of success or failure. An effective risk management strategy, therefore, depends strongly on behavioral factors, including risk preferences (Sulewski and Kłoczko-Gajewska 2014).

The concept of preferences in economics takes us to the seminal work of Von Neumann and Morgenstern (1947). In their book *Theory of Games and Economic Behavior*, they presented a series of axioms which became the basis for Expected Utility Theory (EUT). Under EUT, risk preferences are defined as risk attitudes derived from people's choices, and individuals' choices associated with the subjective value of a statistical expectation of possible outcomes. Therefore, every individual shows unique preferences when facing decisions under risk. To better understand subjective probabilities, Kahneman and Tversky (1979) developed their Prospect Theory, which states that outcomes of individual decision-making under risk are framed around a reference point. In other words, people tend to overweight losses with respect to a comparable gain (Levy 1996), and one should look not only at individual decisions at gain-ranked events but also relating to loss-ranked events (Wakker 2010).

Given the subjective nature of risk preferences, it is essential that research takes preferences into account when assessing the impact of agricultural and development programs, since the adoption or success of a given policy varies with the target population's risk preferences (Charness and Viceisza 2016).

A common assumption when studying technology adoption in the agricultural context is the stability of risk preferences across decision contexts. In other words, risk preferences resulting from choices in one context are used to understand individual behavior in another. However, it is possible that risk preferences are not stable across contexts, or stable across certain specific contexts but not others (Barseghyan et al. 2018). This assumption has major implications for the implementation of surveys aiming to collect risk preferences.

Furthermore, several actions can be undertaken to reduce exposure to risk in agriculture. On one hand, decisions can be part of a bigger picture, with considerations that take into account many household aspects (i.e., broadly bracketed decisions), such as requiring a loan for agricultural investment. On the other hand, other decisions are taken in isolation from all others (i.e., narrowly bracketed), for example, day to day farm management (Barseghyan et al. 2018).

As a result, both the context in which risk preferences are measured and the type of decision under assessment may yield different outcomes when studying how farmers make decisions under risk. Chapter 2 approaches these issues by evaluating a method for estimating risk attitudes that can be easily implemented in the field. It shows how different estimates of attitudes to risk relate to different real-life farming choices and evaluates the survey-based method back-to-back with an incentivize risk experiment.

1.3 Social Preferences

Individuals reveal social preferences when they care not only about resources allocated to themselves, but also about resources allocated to other relevant people (Fehr and Fischbacher 2002). Vast empirical evidence shows that social preferences shape a substantial fraction of people's choices (Fehr and Fischbacher 2002; Fischbacher et al. 2001; Kocher et al. 2008; Martinsson et al. 2013).

Moreover, people's social preferences have significant economic implications. Both the theory and the empirical evidence suggest that the interaction between different types of social preferences (i.e., reciprocity and selfish types) changes the economic incentives, with consequences for the formation of institutions (Fehr and Fischbacher 2002; Kosfeld et al.2009). Agricultural cooperatives are one example of institutions in developing countries' rural areas. Cooperatives are considered an essential vehicle for development that mobilize local resources for a common goal that provides benefits to local farming communities (Zeuli and Radel 2005). Often, cooperatives play an essential role in providing farmers with information and training on risks, tools, and techniques that they can implement in their farms to reduce risk (OECD 2011).

Chapter 3 continues in line with Chapter 2 on the subject of how to measure preferences and how they relate to real-life choices. It focuses on conditional cooperation, a particular type of social preference to study farmer cooperation with local agricultural cooperatives. Conditional cooperators adapt their behavior to others in the group they belong to. In other words, if other people in the group cooperate, conditional cooperators do so as well. If others defect, conditional cooperators follow through.

The chapter examines cooperation in a setting where farmers are commonly organized in agricultural cooperatives. If the majority of farmers are conditional cooperators, they will bring in their coffee harvest for processing if other farmers in the community do so too, and vice versa. Therefore, conditional cooperation can enhance cooperation, but also weaken the cooperative structure if farmers do not bring in their harvest for processing.

1.4 Financial instruments to manage risks

Insurance uptake in developing countries has been extensively studied, but has remained persistently low without the use of continuous subsidies (Carter et al. 2017). Some constraints that limit the capacity of farmers to demand insurance include basis risk, availability of other informal insurance mechanisms, level of knowledge and trust in institutions, and the insurance design, among others (Carter et al. 2017; Hellmuth et al. 2009; Rosenzweig 2012;).

The lack of formal insurance, in turn, aggravates credit constraints. For example, in the absence of insurance markets, the borrower voluntarily withdraws from taking a loan, due to the risk of losing collateral (i.e., risk rationing) (Boucher et al. 2008; Giné and Yang 2009). Under such scenarios, the combination of credit with crop insurance could improve credit markets and encourage investment in the agricultural sector (Carter et al. 2014).

Farmers borrow money with the hope that they will be able to repay their debts after a successful harvest. However, droughts, floods, or extreme temperatures can ruin crops. To cope with losses from extreme weather events, agricultural banks and governments in developing countries cooperate with poor agricultural borrowers by restructuring loans and through debt relief programs (Carter et al. 2007; The World Bank 2007) . Still, governments in general and especially in developing countries have a limited capacity to help. In other words, government assistance is not always certain, and farmers are uncertain about the amount of debt for which they will be liable in the event of crop failure (Carter et al. 2007; Miranda and Gonzalez-Vega 2011).

Financial instruments such as bundling credit together with insurance are a promising solution for risk rationing. However, they may reduce investment due to farmers' limited liability (the certainty of debt relief in case of default), since limited liability provides implicit insurance (Giné and Yang 2009). Thus, when an insurance premium must be paid, this results in a lowered demand for loans.

Chapter 4 follows the theoretical model of Giné and Yang (2009) and focuses on household decisions on how much to borrow in order to invest in his farm. Credit is offered either with or without mandatory insurance with a premium cost, under three types of liability scenarios using a lab-in-the-field experiment.

1.5 Voluntary eco-certification

Initiatives that certify agricultural commodities produced in a sustainable and environmentally friendly way are growingly popular. For example, the number of organic producers has increased by more than 160,000, or over 7% since 2014, with more than 84% of the producers located in Asia, Africa and Latin America (Willer et al. 2017).

Eco-certification schemes like organic certification have the potential to improve commodity producers' environmental performance (Giovannucci and Ponte 2005; Rice and Ward 1996). In theory, they enable consumers to differentiate among commodities by providing information about their environmental attributes, and creating the financial incentives, with price premiums, for producers to meet certification standards.

There is a growing body of academic literature which examines commodity certification. However, little is known about whether it affects producers' environmental performance. Some studies are evaluating the environmental impacts of certification, but many rely on problematic methods that bias their results. To accurately identify certification impacts, an evaluation must construct a reasonable counterfactual outcome, that is, an estimate of what the environmental outcomes for certified entities would have been had they not been certified.

Chapter 5 presents an evaluation of the environmental effects of organic coffee certification in central Costa Rica. Organic certification is a farm-level decision to eliminate the use of chemical fertilizer and pesticide, among the implementation of other environmentally friendly practices. On the one hand, the decision to certify or not the product as organic can be seen as a reflection of individual preferences for a sustainable environment. On the other hand, it can be seen as a farm risk management strategy that reduces dependence on inputs and differentiates the product to receive premiums on it.

1.6 Objectives

The overarching objective of this thesis is to enhance the understanding of farm-level decision making to manage risks. It assesses how individual preferences shape these decisions and endeavor to show how, through understanding farmers' choices, research can contribute to policy design in agriculture. It studies farm-level decisions from different perspectives: regarding farm investments and inputs, decisions on whether or not to support local agricultural cooperatives, choices to demand credit with or without insurance, and decisions regarding voluntary certification schemes; and explores the individual decision-making in interaction with the institutions and government policies that can influence these decisions.

More specifically, this thesis addresses the following research questions in four separate chapters:

- *Chapter 2:* Does context matter when estimating risk preferences via survey methods in the field? Can a context-free survey estimate predict risk-taking behavior in an incentivized experiment? How do the different estimates of risk preferences relate to real-life farming choices?
- *Chapter 3*: Are farmers consistent with their choices across parts of the experiment? Are social preferences determined in an experiment consistent with real-life behavior?
- *Chapter 4*: What is the effect of farmers' liability on the uptake of credit with and without mandatory insurance?
- *Chapter 5*: Does eco-certification yield environmental benefits?

1.7 Methodology

An overall aim of this thesis is to enhance the understanding of how individual preferences shape decisions at farm-level. Although one cannot directly observe preferences, it is possible to make inferences about the underlying preferences by observing behavior in an experiment (Voors et al. 2016). Hence, we analyzed cross-sectional survey data on household and farm characteristics combined with a series of behavioral experiments that allow us to make inferences about farmer preferences to enhance our understanding of farm-level decision making.

Experimental methods have increased in popularity in economics research (Levitt and List 2009; Viceisza 2016). Harrison and List (2004) describe a taxonomy that includes conventional lab experiments, artefactual field experiments, framed field experiments, and natural field experiments. *Conventional lab experiments* are commonly implemented to students, using an abstract framing and set of rules.

Artefactual field experiments, commonly known as *lab-in-the-field experiments,* are those implemented like conventional lab experiments but in the field, with "real subjects"— for example, farmers in rural areas of Costa Rica. *Frame field experiments* use both real farmers and the field context to frame the experiment; for example, in the context of coffee farming. Finally, in *natural field experiments* the "real subjects" participate without their knowledge (Harrison and List 2004).

1.7.1 Empirical strategies

This thesis used standard econometric methods to analyze the survey and data collected from three lab-in-the-field experiments and one quasi-experiment, all implemented with real subjects: coffee farmers in rural Costa Rica.

Chapter 2 uses survey data asking a set of hypothetical willingness to take risks questions (Dohmen et al. 2011) and elicited individuals' attitudes to risk in an experiment with real payoffs (Sutter et al. 2013; Vieider et al. 2015). First, by assessing correlations between a context-free survey estimate and context-specific survey estimates. Then, by testing if survey data predicts risk-taking behavior in an incentivized experiment. Finally, it shows how the different estimates of risk attitudes relate to real-life farming choices using regression analysis.

In Chapter 3, the standard public goods game is combined with the strategy method to identify social preferences of conditional cooperation (Fischbacher et al. 2001), followed by testing both the internal and external validity of the typology of conditional cooperator among farmers that have to decide whether to cooperate with the coffee cooperative. To test for internal consistency, it explores if those identified as conditional cooperators act as such when contrasting their contributions to their beliefs about the contributions of others in the experiment. To test for external consistency, it looks at the interaction between the type of cooperator and the share of other farmers in the village who bring coffee to the local cooperative using regression analysis with interaction effects.

Chapter 4 follows the theoretical model of Giné and Yang (2009). It describes a lab-in-the-field experiment in the traditional setting, conducted by first gathering subjects in a common area to explain the instructions. Each farmer chooses how much to borrow in order to invest in his farm. Credit is offered either with or without mandatory insurance with a premium cost, under three types of liability scenarios: limited liability, 50%, or 100% probability of full liability (i.e. the farmer is liable for sure). A laboratory approach allows us to isolate the impact of limited liability on the demand for loans with and without mandatory insurance.

Finally, Chapter 5 makes use of a quasi-experiment by means of a matching estimator, which constructs a matched control sample of noncertified farms that are very similar to the certified ones in terms of observable characteristics (Dehejia and Wahba 2002; Ferraro et al. 2007; List et al. 2003; Rosenbaum and Rubin 1983). The impact of organic certification is measured as the average treatment effect on the treated (ATT) — the difference between the percentage of certified farms that use a given management practice and the percentage of matched noncertified farms that use it.

1.7.2 Study area and data collection

The empirical research was set in Costa Rican coffee farming areas (Figure 1.1). Although coffee is no longer the main agricultural export of the country, it remains a leading agricultural commodity. According to the Costa Rican Coffee Institute (ICAFE), there are around 41,300 coffee growers (11,180 less than in 2008), producing roughly over 2 million coffee bean *fanegas*[1] annually (ICAFE 2018). The coffee sector is mostly composed of small-scale growers (90%), who produce less than 100 coffee *fanegas* per year (ICAFE 2018). These small-scale farmers are traditionally organized in agricultural cooperatives. Since the 1980s, most coffee production has been converted to a high-yielding "technified" monocrop in which coffee is grown with minimal shade cover and intensive application of agrochemicals, a system that was pioneered in Costa Rica (Adams and Ghaly 2007; Rice and Ward 1996).

Research for this thesis took place in various places and moments in time as part of different research projects. Chapter 5 examines organic coffee certification in Turrialba, a coffee region located about 64 km east of the capital, San José. Certified farmers in this region belong to the Association of Organic Producers of Turrialba (*Asociación de Productores Orgánicos de Turrialba, APOT*). For reasons discussed in said chapter, the study focuses on coffee certification during 2003, the year that census data was collected in the region. In 2003 there were 36 certified farmers registered and 2567 uncertified farmers taking part in the census.

Data collection for Chapters 2 and 3 took place during 2014 in two other Costa Rican coffee regions: Tarrazú and Brunca. Households were sampled through stratified random sampling based on coffee plot density within three districts from each of the two coffee regions. Only household head farmers took part in the survey and experiments. The experiment was introduced as part of the survey and conducted with each farmer at their house. The final sample comprised 293 coffee farmers.

1 A *fanega* is a standard unit of volume to measure coffee in Central America, of approximately 250 kg.

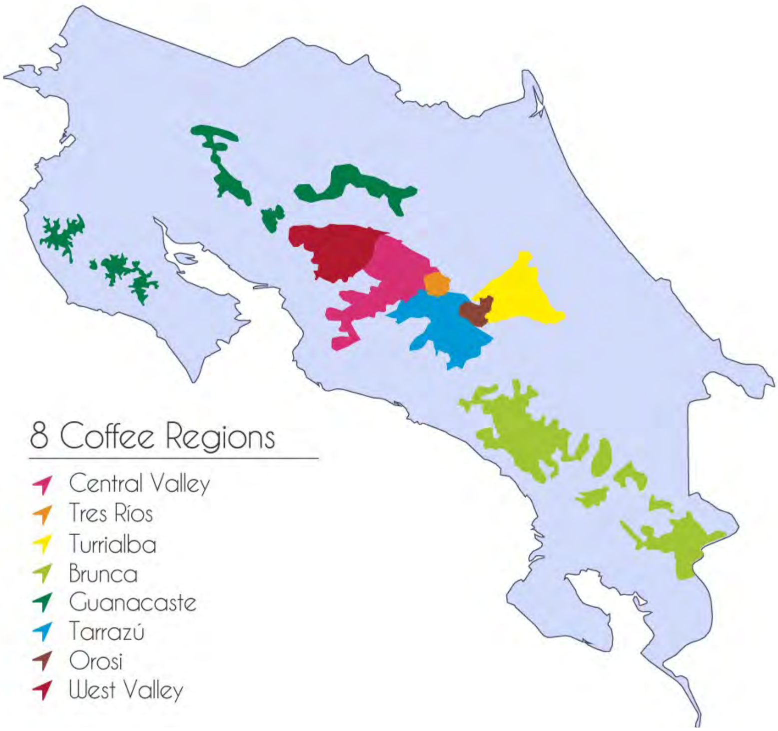


Figure 1.1 | Coffee growing regions in Costa Rica.
Source: Roblesabana Coffee (2018)

Finally, the data for Chapter 4 was collected as a follow up to the 2014 survey, contacting all previously surveyed farmers to conduct thirteen experimental sessions at local primary schools during the second and third week of October 2015. In total, 134 farmers participated in this last experiment (46% of the 2014 survey participants).

1.8 Outline

The chapters are organized as follows. Chapter 2 looks at the alternatives for collecting risk preferences data in the field. Chapter 3 tests conditional cooperation by looking at local participation of farmers in agricultural cooperatives. Chapter 4 examines the effect of farmers' liability on the demand for credit with and without insurance. Chapter 5 looks at the environmental impacts of organic coffee certification. Chapter 6 summarizes the empirical findings, lessons learned, and offers a discussion of policy implications.

2

Alternatives for risk elicitation in the field

Evidence from coffee farmers in Costa Rica

Abstract

In this paper, we evaluate a survey-based method to other methods that elicit farmers' risk attitudes. For both researchers and practitioners, surveys can be easier to implement than field experiments in developing countries. We first assess correlations between a context-free survey estimate of risk-taking and context-specific risk preferences. Then, we test whether survey data predicts risk-taking behavior in an incentivized experiment. Finally, we show how the different estimates of survey risk preferences relate to real-life farming choices in a population of coffee farmers in Costa Rica. Our results indicate that one should be careful when extrapolating risk attitudes across contexts. Context-neutral and context-specific survey questions elicit different risk preferences. While the context-free survey estimate of risk preferences predicts risk-taking behavior in a context-free risk experiment, and context-specific estimates are associated with risk-taking in the same agricultural real-life context , the context-free survey estimate of risk-taking is not associated with actual risk-taking behavior in the agricultural setting. Connecting these methods to farm practices, we find that higher willingness to take risk is associated with the implementation of agricultural practices that require more farm investment. In contrast, farmers who report less willingness to take risks are more likely to have higher expenditures on fertilizer use. Researchers interested in using risk preferences as inputs into the design of policy instruments should make sure that preferences are elicited in the specific context targeted by the potential policy instrument.

This chapter is based on:

Naranjo, M.A., Alpizar, F., Martinsson, P. (2019) "Alternatives for risk elicitation in the field: Evidence from coffee farmers in Costa Rica." (Working paper).

2.1 Introduction

In an agricultural setting there is nothing like certainty. On a daily basis, farmers have to make risky decisions, from the choice of crops and timing of harvest to the application of farming inputs and other strategies to cope with weather variation. Accordingly, it is important that governments and nongovernmental organizations (NGOs) take into account risk attitudes when assessing the impact of agricultural and development programs. Because the adoption or success of a given policy varies with the target population's risk preferences (Charness and Viceisza 2016), policy-makers and researchers must collect information about risk attitudes

A central issue here is how to estimate risk attitudes. The standard in the literature is the use of incentivized experiments, where individual's choices have direct consequences on their earnings (see discussion on induced value theory in Smith (1976)) and many papers have adopted this strategy. However, conducting risk experiments in the field, for example with farmers, is costly, as well as technically and logistically complicated. An alternative is to use stated preference data collected via survey questionnaires, which are less expensive since they are not incentivized with money, and are easier to apply to a more extensive population. However, based on the logic of induced value theory, stated preference can be criticized for not having direct consequences for respondents. Falk and Heckman (2009) summarize this discussion and argue that experiments and surveys are complements, and both have their pros and cons.

Risk experiments typically are conducted in a context-free environment, where individuals make decisions between different lotteries. Experiments can vary regarding the elicitation method or choice task, framed in the loss or gain domain, probabilities, and stakes (Jamison et al. 2012).[2] By contrast, survey methods often use Likert scales to elicit risk attitudes. Survey-based methods are typically more elaborative about the framing of the question, with a general context-free assessment of willingness to take the risk, or a context-specific question regarding risk-taking in, for example, driving or health-related choices, as in Dohmen et al. (2011).

In this paper, we are interested in evaluating a method for estimating risk attitudes that can be easily implemented by policy-makers in developing countries. Hence, our objective is to evaluate a survey-based method to collect risk attitudes in the field with a population of farmers in rural Costa Rica. We do this, first, by assessing correlations between a context-free survey estimate and context-specific survey estimates. Then, we test whether survey data predicts risk-taking behavior in an incentivized experiment, and, finally, we show how the different estimates of risk attitudes relate to real-life farming choices.

2 See Jamison et al. (2012) for a complete review on risk elicitation methods.

This paper makes two major contributions to the literature. First, it contributes to the under-researched issue of the extent to which survey risk preferences are stable across contexts (Barseghyan et al. 2018). We combine laboratory and field settings to gain insight into whether different risk estimates can be directly applied to make real-life predictions. Second, our paper contributes to the literature on the role that lab in the field experiments play in informing policymaking, and the nature of different empirical methods to estimate parameters associated with characteristics such as risk preferences (Viceisza 2016).

Our findings can be summarized as follows. First, regarding the consistency of survey risk preferences across contexts, we do not find one general component across specific contexts. Hence, it is important to elicit risk attitudes for the specific context of interest. Researchers and practitioners should be careful when extrapolating risk attitudes across contexts.

Second, our survey estimate of risk-taking (with no context) predicts behavior in the risk experiment in line with previous studies. Furthermore, we explore how our survey-based estimate of willingness to take risk characterizes the utility function parameters. Higher willingness to take risk is associated with less pessimism, less sensitivity to changes in probabilities that increase the likelihood of a loss, and more loss aversion.

Crucially, we find differences between survey estimates with and without a context in relationship with real-life farming behavior. The context-free survey estimate of risk-taking is not associated with actual risk-taking behavior in the agricultural setting. The risk preference elicited through a survey is associated with real-life farming choices only when the survey question is asked in the specific agricultural context.

In this context, we contribute by understanding farmers' risk-related real-life decisions. Higher willingness to take risk is associated with the implementation of agricultural practices that require more farm investment. In contrast, farmers who report less willingness to take risks are more likely to have higher expenditures in fertilizer use. This last result is consistent with studies showing that applying fertilizer reduces the risk of pests and low yields in coffee farming in Central America (Avelino et al. 2015), but we recognize the effect can be crop and input specific.

Researchers and practitioners interested in using risk preferences in the design of policy instruments should make sure that preferences are elicited in the specific context targeted by the policy instrument. For general financial decisions, the estimation of risk preferences without a context might be sufficient, but if the policy instrument target a specific technology or input (e.g., fertilizer use or implementation of improved seeds varieties) risk preferences should be elicited in that particular context.

The rest of this paper is organized as follows. The next section describes our conceptual framework and hypotheses. The third section describes the study area, the sample selection, and the fieldwork implementation. Section four explains the methods, including the experimental design and modeling approach. Section five presents the results, and the last section concludes the paper.

2.2 Conceptual framework and hypotheses

We divide our analysis into three sets of research questions. First, we want to know whether there is stability across context-free and context-specific survey methods of risk attitudes. Second, we want to test whether the context-free survey estimate predicts risk-taking behavior in an incentivized experiment. Third, we want to know how different estimates of risk attitudes relate to real-life farming choices. In this section, we describe our theoretical considerations and develop our hypotheses, supported by a review of previous studies.

2.2.1 Stability of risk attitudes across context

We want to know whether we can use one general survey-based measure of risk to characterize risk-taking in different contexts and in particular for the agricultural context. If that is the case, adding a simple question to a questionnaire can help practitioners characterize risk attitudes. If that is not the case, more context-specific questions are needed to estimate risk preferences in surveys accurately.

Some studies have found that risk attitudes have a general component (i.e., preferences are stable across specific contexts) (Dohmen et al. 2011; Einav et al. 2012; Vieider et al. 2015). On the other hand, Barseghyan et al. (2011) and Lönnqvist et al. (2014) do not find a link between a general risk question and specific context settings for risky behaviors. Hence, evidence is still mixed, and more importantly, studies have used subjects from developed countries or student samples in a controlled lab setting, either of which can be considered an "unusual population" (Henrich et al. 2010) and difficult to extrapolate to the developing world.[3]

3 Dohmen et al. (2011) used the German Socio-Economic Panel to compare stated risk attitudes in the survey to responses in real-stakes lotteries. Lönnqvist et al. (2014) conducted the experiment at the Laboratory for Experimental Economics in Bonn. Barseghyan et al. (2011) and Einav et al. (2012) data is from the United States and Vieider et al. (2015) applied controlled lab experiments with students in 30 countries who also responded to a series of survey stated preference questions.

In this study, we present an extension from Dohmen et al. (2011) from a non-work context[4] to a real-life working activity such as farming using a population of developing country farmers. We emphasize the context in which the survey is asked. Questions are framed in a general risk question with no context, in a financial context, in an agricultural context, and in to agriculture-specific questions regarding changing or diversifying crops, changing coffee varieties, and applying farm inputs such as fertilizer and pest control (see Figure 2.1).

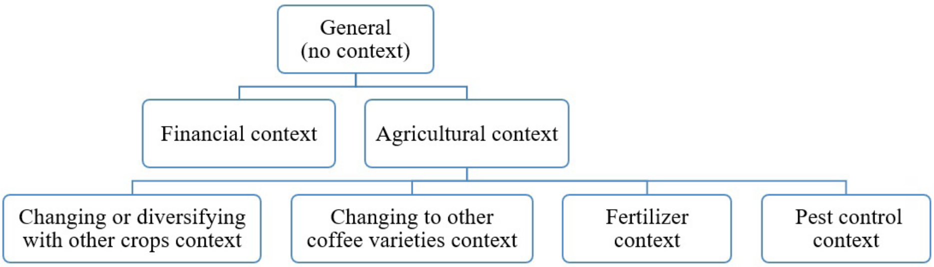


Figure 2.1 | Survey-based estimates of risk attitudes: from general to agricultural specific risk attitudes.

If survey-based estimates for risk-taking in different contexts are correlated with a general survey-based estimate of risk, then we can conclude that risk attitudes have one domain-general component across specific contexts. Otherwise, a principal component analysis will identify if we can group risky choices in components. In other words, we test the following hypothesis:

Hypothesis 1: there is a positive and significant correlation across survey-based risk estimates from general to specific contexts.

2.2.2 Experimental validation of survey estimates

Motivated by different models of risk preferences[5], risk experiments are conducted typically in a context-free environment, where individuals make decisions between different lotteries. Lotteries can be implemented using choice tasks or by eliciting safe alternatives (i.e., certainty equivalents) next to the lottery. Lotteries can differ regarding the frame in the loss or gain domain, under different stakes and probabilities. For example, some experiments offer options over gambles that

4 Dohmen et al. (2011) studied the stability of risk attitudes across six contexts: general, car driving, financial matters, sports and leasure, career and health. These contexts do not relate to specific decisions in the main income activity of a household.

5 For Expected Utility (EU) theory to non-EU models including rank dependent expected utility (RDEU) theory and cumulative prospect theory (CPT), see Barseghyan et al. (2018) for a complete review on the risk preferences models.

increase in expected value (Binswanger 1980; Cardenas and Carpenter 2013). Others elicit certainty equivalents of fixed bets (Henrich and McElreath 2002) or offer a series of paired lotteries to obtain prospect theory parameters (Tanaka et al. 2010), looking at gain-ranked events (gain domain) and loss-ranked events (loss domain) (e.g., Sutter et al. 2013; Vieider et al. 2015).

We want to explore whether reporting a higher willingness to take a risk in response to a general survey-based question is reflected in a greater willingness to take a risk in an incentivized lab-styled experiment. We use the survey-based general willingness to take risks (with no context) because the experiment is context-free as well. Previous studies have found that a general estimate of risk predicts risk-taking in an experiment (Dohmen et al. 2011; Vieider et al. 2015). However, a recent study by Charness and Viceisza (2016) finds different results in a developing country context. They test subjects' comprehension of three methods in rural Senegal: two experimental tasks (the Holt–Laury task and the Gneezy–Potters mechanism) and a non-incentivized willingness-to-risk scale following Dohmen et al., 2011). They find a low level of understanding of the Holt-Laury task and the experimental estimates unlikely to be correlated with the willingness to risk question in general.

Our second objective aims to test whether general survey estimate predicts risk-taking behavior in an incentivized experiment as follows:

Hypothesis 2: Survey-based general risk-taking (with no context) is positively and significantly correlated with the average risk-taking behavior in a context-free experiment.

2.2.3 Risk attitudes and real-life farming choices

Our final research question aims to analyze how different estimates of risk attitudes relate to real-life farming choices in our sample of Costa Rican coffee farmers. We analyze decisions that require significant financial investment: changing to other coffee varieties, changing to other crops, or diversifying coffee farming with other crops. We also analyze fertilizer and pest control applications, which are more day-to-day farm input management practices.

In order to estimate risk preferences in a field context, researchers make assumptions about how the complex field context is translated into a precise estimation that can be used to approximate risk preferences. However, not all risk decisions are the same. Decision making can be broadly bracketed or narrowly bracketed. On the one hand, an individual's decision making could bracket a decision as one grand decision taking into account many aspects of life (broadly bracketed). On the other hand, the decision could be evaluated in isolation from all others (narrowly bracketed) (Barseghyan et al. 2018, p.558).

How risk affects farm investment decisions will depend on the specific agricultural activity and biophysical environment (Berg 2001; Hanus and Schoop 1989). Looking at previous literature, risk aversion delays the adoption of new technologies, because uncertainty regarding a new technology discourages individuals who are more reluctant to take a risk (Feder 1980; Holden and Quiggin 2017; Liu 2013). Investment in agricultural technologies is costly, and farmers have to balance the advantages regarding reduced exposure to uncertainty in agriculture (for example, by replacing their plantation with improved coffee varieties that are resistant to pests and drought) with the increased exposure to financial risk (i.e., acquiring loans). See Feder et al. (1985) and Just and Zilberman (1983) for a full discussion.

Therefore, we expect risk-averse farmers to implement fewer practices that can reduce risk from an agricultural perspective when this entails a higher exposure to financial risk. For instance, Brick and Visser (2015) find risk-averse farmers are more likely to maintain the use of traditional seeds and less likely to use modern farming inputs that require costly financing. Holden and Quiggin (2017) find that more risk-averse households were less likely to adopt improved maize varieties and less likely to dis-adopt traditional local maize.

In contrast, the effect of risk on farm inputs is ambiguous. Farm inputs can increase not only the level of output but also its variability. In other words, fertilizer can increase the average farm yield, but at the same time, the yield can be very low or very high, and this variation across farms yield represents a risk to farmers (Berg 2001; Hanus and Schoop 1989; Vablauwe et al. 2016). As a result, farmers can manage risk through input use, but they can also be discouraged from adopting an input because the input is associated with output variability (Vablauwe et al. 2016). Even so, farmers' risk perceptions do not necessarily correspond with the biophysical effect of fertilizer on yield variability. For example, a typical farmer in the U.S. applies more fertilizer than the utility maximizing level (Berg 2001; Babcock 1992), as producers consider fertilizer to be risk-reducing (Sriramaratnam et al. 1987).

In some contexts risk aversion encourages expenditures on practices that reduce exposure to agricultural risk (Alpízar et al. 2011; Barham et al. 2014), especially if practices do not involve large investments. For example, applying fertilizer and actively controlling for pests reduces the risk of pests and low yields (Avelino et al. 2015) and risk aversion increases pesticide use in China (Liu and Huang 2013). On the other hand, Roosen and Hennessy (2003) and Khor et al. (2015) show theoretically and empirically that an increase in risk aversion reduces fertilizer use intensity, but recognize that it might not be the same for farmers of different wealth levels.

Hence, we want to contribute to the debate by analyzing how different estimates of survey risk attitudes relate to real-life farming choices. According to the theory, we hypothesized that farmers who are more willing to take risks are also more likely to implement investments and changes in their farms, as follows:

Hypothesis 3: Survey-based willingness to take a risk in general and in different contexts is positive and significantly associated with the implementation of risky real-life farming practices.

2.3 Description of the study area, sample, and implementation

Our study took place in the year 2014 in two coffee regions of Costa Rica: Tarrazú and Brunca. Households were sampled through stratified random sampling based on the density of coffee plots within six districts of the two coffee regions (three districts from each region)[6].

Only household head farmers took part in the survey and experiment. The experiment was introduced as part of the survey and conducted with each farmer at his or her house. Most experiments take place in a lab setting where subjects self-select themselves to participate. By applying the experiment as part of the survey, we ensure randomness, representativeness, and anonymity during the experiment as well. First, a survey questionnaire collected detailed household characteristics and farming practices. After completing the survey, the farmer was presented with the incentivized risk experiment, followed by a set of hypothetical willingness to take risks questions as in (Vieider et al. 2015).

In our final sample, we have 293 coffee farmers. Their household socioeconomic characteristics and coffee farm characteristics are presented in Table 2.1. In our sample, coffee farmers have on average only primary education, have life experience in coffee farming, and on average 57% of their income is earned through selling coffee.[7]

6 Costa Rica's national public administration divides the country into provinces, cantons, and districts. Districts were chosen to capture the spread and variation of intensity of the coffee rust epidemic in 2012-13.

7 Only 278 farmers out of 293 answered the question related to the percentage of income from coffee activities.

Table 2.1 | Variables and sample means from survey sample.

Variable	N	Mean	min	max	sd
Household characteristics					
Household head female	293	0.10	0	1	0.30
Age (years)	293	51.77	19	86	13.64
Education (years)	293	5.795	0	15	2.61
Household size	293	3.33	1	10	1.38
Household head labor in another farm	293	0.13	0	1	0.33
Total farm area (ha)	293	5.49	0.04	109	9.96
Number of bedrooms in house	293	3.11	1	7	0.90
Coffee farm characteristics					
Farm experience (years)	293	25.50	1	71	14.49
% of income coming from coffee	278	56.78	0	100	36.20
Total area planted with coffee (ha)	293	3.48	0.09	41.77	4.62
Brings coffee to a cooperative	293	0.78	0	1	0.42
Farm affected by coffee leaf rust	293	0.81	0	1	0.39
Real-life farming risky choices					
Change coffee variety	293	0.13	0	1	0.33
Change or diversify with other crops	293	0.08	0	1	0.27
Number of fertilizer applications (2013)	293	2.58	0	6	0.74
Number of pest control applications (2013)	293	3.40	0	8	1.49

We gathered extensive information on all farmers' management practices.[8] For this paper, we emphasize farming choices that are standard practices for conventional coffee farming and not dependent on specific characteristics of topography. Therefore, we focus on four sets of activities: i. changing crops or diversifying the farm by adding other crops in recent years; ii. changing coffee variety in recent years, iii. the number of fertilizer applications and iv. the number of pest control applications in the year before the survey. From these practices, we can observe that diversification with other crops is very rare among coffee farmers. In addition, only 13% have changed the coffee variety in the last 10 years before the survey. Regarding fertilizer, agronomists recommend a minimum of three applications per year and preventive use of pesticide. Here, the average number of fertilizer applications is below that recommendation, and farmers tend to apply pesticides in a reactive way rather than in a preventive manner.[9]

8 We collect information about the following farming practices: contour planting, use of deviation ditches, natural barriers, shadow management, windbreakers, terraces, live coverage, pruning, application of pesticides and herbicides, fertilizer application, change or diversification with other crops and change of coffee varieties.

9 Pesticide use includes fungicide, insecticide, and nematicide. Fungicide, for example, is applied after long periods of rain to prevent the growth of fungus.

2.4 Methods

This section explains in detail the survey questions on risk preferences implemented in the structured questionnaire. Then, we describe the design and implementation of the risk experiment. Our experimental data also allows us estimate the risk preferences parameters derived from the utility function. Therefore, at the end of this section we include the modeling approach for the estimation of these parameters.

2.4.1 Survey questions

We asked a set of hypothetical willingness to take risks questions based on Dohmen et al. (2011). The general survey-based question is stated as follows: "*On a scale where the value 0 means "not at all willing to take risks" and the value 10 means "very willing to take risks; how do you see yourself, are you a person who is fully prepared to take risks or do you try to avoid taking risks?"* (Dohmen et al., 2011, p.525).

When asking farmers about their willingness to take risks in specific contexts, we focus on agriculture-relevant contexts and decisions. We asked about their willingness to take risks in financial decisions, farming (in general), and then in specific farming context: when changing or diversifying with other crops, when changing to different coffee varieties, and regarding the use of pesticides and fertilizer. All questions follow the same structure as Dohmen et al. (2011)[10]. For example: "*On a scale where the value 0 means "not at all willing to take risks" and the value 10 means "very willing to take risks; how do you see yourself* ***regarding your decisions on changing to other coffee varieties****, are you a person who is fully prepared to take risks or do you try to avoid taking risks?"*

2.4.2 Experimental design and modeling approach

In the experiment, we elicited individuals' risk attitudes with real payoffs, following a design by Vieider et al. (2019) and Sutter et al. (2013). Their design elicits certainty equivalents (CEs), which are easy to explain to subjects with low education.

In this paper, we focus on choices with known probabilities. Three tasks offered the choice between a certain amount that gradually increased in value or a lottery to win 5000 Costa Rican Colones (CRC) or nothing (i.e., gain domain). Then, three tasks gave the farmer an initial endowment of 5000 CRC and the choice between gradually increasing payments to avoid a lottery where the total endowment could potentially be lost (i.e., loss domain). Lastly, we included a task necessary to estimate

10 We also asked questions for the specific contexts of driving a car, sport and leisure, working outside of the farm and health-related decisions. These results are presented in Appendix 2.2 but are not part of the analysis since we focus on work-related real-life farming decisions.

loss aversion (i.e., mixed prospect), which offered the possibility of winning 5000 CRC extra or losing part of the endowment.

Probabilities changed between 50/50, 10/90 and 90/10 of winning or losing the fixed amount (see Table 2.2). Choices are expressed by simple prospects in the format (p: x:y), where p is the probability of outcome x and (1-p) the corresponding probability of outcome y (Vieider et al. 2015).

Table 2.2 | Choice list for risk tasks in the experiment.

Gains	*Losses*	*Mixed*
(0.5: 5000;0)	(0.5: -5000;0)	0~(0.5: 5000;z*)
(0.1: 5000;0)	(0.1: -5000;0)	
(0.9: 5000;0)	(0.9: -5000;0)	

Note: z* varied in a choice list from 5000 to 200 colones.
Source: Vieider et al. (2015)

At the end of the experiment, one choice task was randomly selected for payment by taking a chip out of a bag. In case the lottery was preferred by the farmer, we used another bag containing precisely ten chips numbered from 1-10 to represent probabilities, and this distribution was known and shown to the farmer. We explained this to the farmers graphically using a transparent urn (see Appendix 2.6) and, to avoid predetermined numbers, each farmer individually assigned the lucky number for the winning chips before making the draw.

This method of experimental elicitation allows us to determine the certainty equivalent (CE) for each prospect directly. The certainty equivalent is defined as the midpoint between two levels of sure payoffs where the farmer switches from the lottery to the sure payoff in the gain domain, or from the certain payoff to a lottery in the loss domain (Sutter et al. 2013). In other words, we determine the willingness to accept increasing certain amounts over a lottery to win a prize, in the gain domain; and the certain amount farmers are willing to give up to avoid a lottery where they risk losing their endowment, for the case of the loss domain. Consequently, the higher the certainty equivalent, the more willing the farmer is to take risk in the gain domain. The higher the certainty equivalent, the less willing the farmer is to take risks in the loss domain. For the analysis, we specify the certainty equivalent in the loss domain lotteries with negative values to show the same direction: the higher the value of the certainty equivalent, the more willingness to take risks.

2.4.3 Modeling risk preferences parameters

A common approach in the experimental literature is to characterize risk preferences through a single parameter that reflects the curvature of the utility function using expected utility theory (Holt and Laury 2002). However, several studies have highlighted the predictive power of prospect theory by Tversky and Kahneman (1992) (Camerer 2001; Liu 2013).

In the context of farming, the prospect theory concepts of reference-dependence, loss aversion, and probability weighting can help us understand farmers' real-life choices better. Reference dependence utility is when people evaluate outcomes relative to a reference point (Kahneman and Tversky, 1979). Farming decision making is not necessarily about gain-ranked events, but about loss-ranked events where yields might be lost with a given level of risk. Then, it is intuitive to assume that farmers' choices under risk are to avoid losses rather than to pursue gains. Furthermore, individuals tend to underweight high probability events and overweight low probability events (Babcock 2015; Tversky and Kahneman 1992).

We model individual preferences following L'Haridon and Vieider (2017). They use a cumulative prospect theory model (CPT) in the format (p: x:y), where p is the probability of obtaining the outcome x and y is achieved with the corresponding probability (1-p), $|x| > |y|$. We assume preferences are reference-dependent and, in the experiment, are framed with a reference point equal to zero (Abdellaoui et al. 2011; L'Haridon and Vieider 2017; Tversky and Kahneman 1992).[11] The utility of a prospect (PU) for outcomes that fit in one domain (gain or loss) can be represented as follows:

$$PU = w^j(p)v(x) + \left[1 - w^j(p)\right]v(y) \tag{1}$$

where $w^j(p_i)$ is the probability weighting function that combines probabilities into decision weights[12]. The decision domains are specified by *j*, which takes the values for gains and for losses. For mixed prospects, where $x > 0 > y$, the utility is represented as:

$$PU(x,p) = w^+(p)v(x) + w^-(1-p)v(y) \tag{2}$$

11 Reference-dependence is an important principle in prospect theory. Individuals evaluate outcomes relative to a reference point to evaluate gains and losses.

12 The function is strictly increasing and satisfies $w(0) = 0$ and $w(1) = 1$

We follow the functional forms indicated by L'Haridon and Vieider (2017) and assume a piecewise linear utility function as follows:

$$v(x) = \begin{cases} x & \text{if } \ x > 0 \\ -\lambda(-x) & \text{if } \ x \leq 0 \end{cases} \tag{3}$$

where λ is the *loss aversion* parameter that defines the curvature below zero relative to the curvature above zero, and value $\lambda > 1$s indicate loss aversion, where higher values of λ indicate the farmer is more loss averse (Vieider et al., 2019).

In this framework, risk preferences are captured by probability weighting and loss aversion. Similarly to L'Haridon and Vieider (2017) and Vieider et al. (2019), we adopt a 2-parameter weighting function by Prelec, (1998)[13] :

$$w^j(p) = \exp\left[-\beta^j\left(-ln(p)\right)^{\alpha^j}\right] \tag{4}$$

The parameters of the Prelec function provide a detailed behavioral interpretation. The parameter α governs the slope of the probability weighting function, with values $\alpha < 1$ indicating *probabilistic insensitivity* for gain and losses. Values $\alpha < 1$ also indicate the weighting function has an inverted S-shape, which shows an overweighting of low probabilities of the largest gains or biggest losses and an underweighting of high probabilities (Liu 2013; L'Haridon and Vieider 2017)[14]. The parameter β governs the elevation of the weighting function indicating the weight assigned to the best outcome for gains and the worst outcome for losses. Therefore, the higher values for β indicate increased *probabilistic pessimism* for gains and increased *probabilistic optimism* for losses. Consequently, we refer to the parameter β as *pessimism* for gains and *optimism* for losses (Vieider et al., 2014, p.10).

13 The model and functional forms adopted, estimate an error term directly to the certainty equivalent, that allows for measurements errors for the stochastic elements in the decision process (L'Haridon and Vieider, 2017). All parameters are estimated using the log-likelihood function, programmed in STATA.

14 We could assume the value of α to be equal to one ($\alpha = 1$). This assumption will indicate linearity of the weighting function (the EU case), and then the parameter β can be considered as the standard estimate of risk aversion.

2.5 Results

We start by showing the descriptive statistics of the data collected via the survey and the experiments. Then, following our research questions, we assess the consistency across contexts by assessing correlations between survey-based estimates of general and context-specific risk preferences. We then evaluate how survey risk estimates explain risk taking in the experiment, and finally compare the survey estimates to real-life farming practices.

Figure 2.2 shows the distribution of responses to the general survey-based risk question. Our data shows that a relatively small fraction of respondents choose low values, indicating that most farmers are willing to take risks in general. This result is very different from Dohmen et al. (2011), where only a small fraction of respondents choose high values and subjects are on average risk-averse.

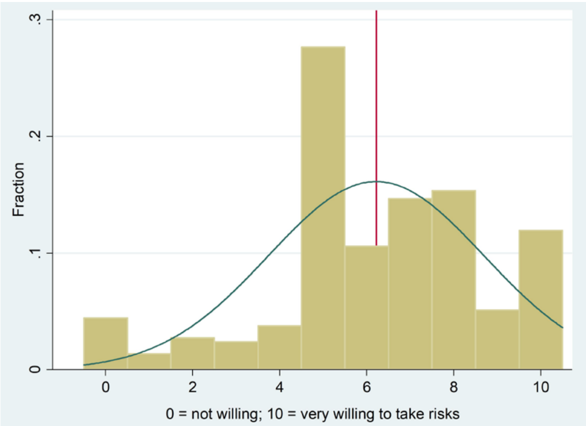
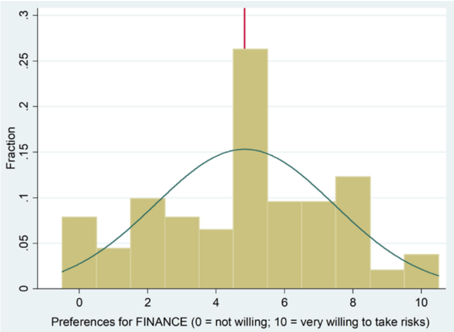
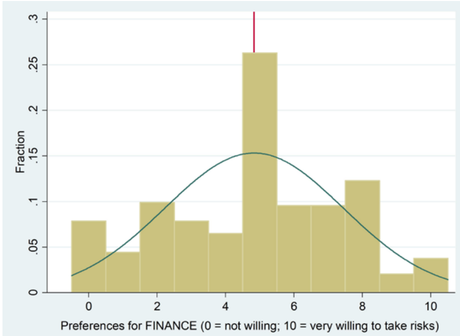


Figure 2.2 | Farmer's response to general survey-based risk preference question.
Note: Red line indicates mean response to general survey-based risk question and blue line shows the adjusted frequency distribution.

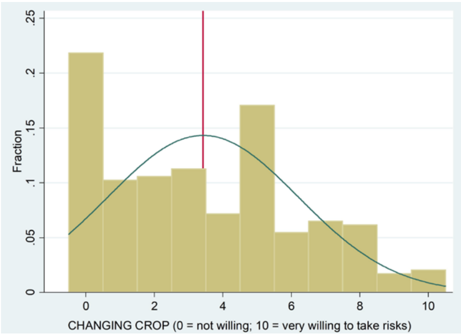
In Figure 2.3, we present the distribution of responses to the context-specific risk questions. We see variation in the different agriculture-specific attitudes. We find a normal distribution regarding risk-taking in financial decisions and when changing coffee variety (see 2.3.1 and 2.3.4). Farmers are relatively more risk averse with regards to changing crop (see 2.3.3), but are relatively more risk-seeking regarding farming decisions and the use of pest control and fertilizer (see 2.3.2, 2.3.5 and 2.3.6).



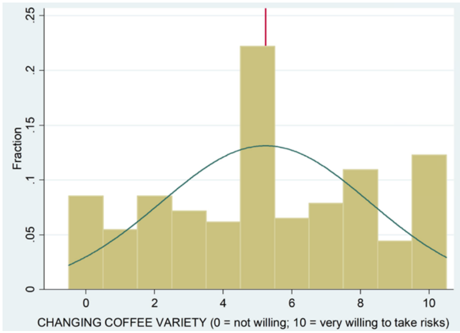
2.3.1 Risk attitudes toward financial decisions



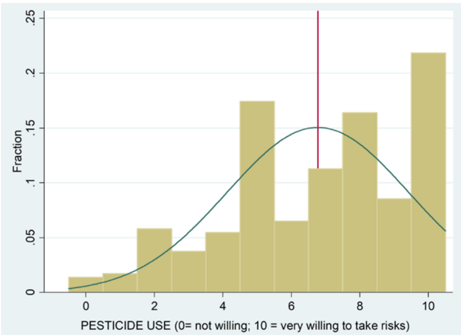
2.3.2 Risk attitudes toward farming decisions



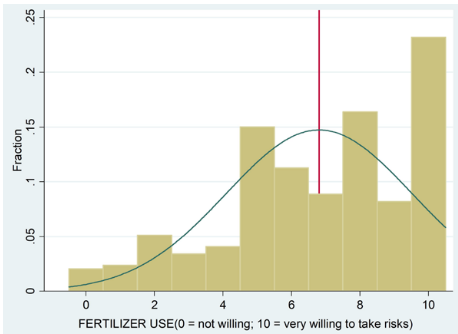
2.3.3 Risk attitudes toward changing or diversifying with another crop



2.3.4 Risk attitudes toward changing a coffee variety



2.3.5 Risk attitudes toward pesticide use



2.3.6 Risk attitudes toward fertilizer use

Figure 2.3 | Farmer's response to context-specific risk preferences questions.
Note: Red line indicates the average response to risk question and blue line show the adjusted probability distribution.

We present the nonparametric representation of the aggregated experimental data in Appendix 2.1. When comparing the overall mean CE to the average expected value (EV), farmers are found to be on average risk-seeking (mean CE > EV) for both the gain and loss domain[15]. This result is similar to Charness and Viceisza (2016) and Vieider et al. (2016, 2014), who find farmers are willing to take risks in Senegal, Vietnam, and Ethiopia, respectively. On the other hand, results differ from Verschoor et al. (2016), who find higher levels of risk aversion hypothetically and in their risk experiment. Overall, our study shows supporting evidence that farmers in developing countries are not necessarily more risk-averse than subjects from developed countries; on the contrary, people from rural areas seem to tolerate higher risk levels in their daily activities.

2.5.1 Correlations between general and context-specific survey risk attitudes

We want to know whether there is a general component of risk across our survey estimates of risk attitudes in different contexts. To do this, we use the Spearman's rank-order correlation coefficient measuring the strength and direction of a correlation between two variables measured on an ordinal scale. We test the hypothesis that the Spearman's correlation coefficient (ρ) is equal to zero and highlight when the significance level is a p-value < 0.05. Furthermore, given that we are testing multiple hypotheses, we report the adjusted p-values according to the Bonferroni correction method. When performing multiple hypotheses testing, it is argued that some correlations will have p-values lower than 0.05 purely by chance. The Bonferroni correction method uses a lower critical value according to the number of hypotheses tested. As a result, the probability of observing at least one significant result remains below the desired significance level (Dunn 1961).

We show correlations between the stated willingness to take risks in general and when asked in specific contexts (Table 2.3). The context-free survey estimate is significantly correlated (p-value < 0.05) only with the context-specific estimate related to finances (first column). The latter also significantly correlates (p-value < 0.05) with changing or diversifying with other crops (second column). We find no correlation between the general (no context) or finance risk-taking estimates and the estimates in the context of pest control and fertilizer. On the other hand,

15 Results for each prospect are also in line with Kahneman and Tversky (1979), where individuals underweight high probability events and overweight low probability events (Babcock 2015). Also, consistent with the cumulative prospect theory (CPT) pattern stated by Tversky and Kahneman (1992) and recently tested by Harbaugh et al. (2014), individuals are relatively more risk-seeking over low-probability gains, risk-averse over high-probability gains, risk-averse over low-probability losses, and risk-seeking over high-probability losses.

when asking farmers about their willingness to take risks framed in the farming context (third column), this correlates significantly with risk preferences estimates regarding pest control and fertilizer applications (p-value < 0.05).

Similar to previous studies, we also find that the specific context of finances correlates with the general estimate of risk preferences.[16] However, it is surprising that none of the other survey-based context-specific estimates correlate significantly with the general estimate and the estimate of risk regarding finances correlates only with changing crops. Furthermore, asking farmers for their risk preferences in the farming context correlates only with general farm management like pest control and fertilizer applications. We do not know of previous studies with such detailed results for the agriculture-specific context.

Table 2.3 | Correlations between survey-based estimates to take risks in general and agricultural specific contexts.

	General (1)	Finances (2)	Farming (3)	Changing crop (4)	Changing variety (5)	Pest control (6)	Fertilizer used (7)
General (no context)	1						
Finances	**0.4314***	1					
Farming	0.1585	0.1497	1				
Changing crop	0.1374	**0.2763***	0.1488	1			
Changing variety	0.1444	0.1603	0.0746	**0.2656***	1		
Pest control used	0.1646	0.1140	**0.2473***	0.0176	**0.3248***	1	
Fertilizer used	0.1078	0.0965	**0.2538***	-0.0450	**0.2888***	**0.8146***	1

Note: Coefficients refer to Spearman's ρ and are calculated using all non-missing observations between a pair of variables; * p-values < 0.05 using Bonferroni correction method for adjusted p-values.

To complement our analysis, we performed a principal component analysis to identify the factors or components that express the maximum information out of the survey estimates (Appendix 2.3). Following the *Kaiser rule,* we should retain the components with an eigenvalue larger than one (Kaiser 1960). The analysis shows that there are two components with an eigenvalue above one, together explaining 55% of the variation in the data. The general, the financial and the changing crop estimates of risk-taking are grouped under one component, while the other farm practices, input used and changing coffee variety, are gathered under the second component. The farming context remained highly unexplained (79%) and not grouped in either of the two components.

16 For example, Dohmen et al. (2011, p.537) report a significant correlation coefficient of 0.5036 between the general estimate of risk and risk-taking in financial matters.

These results tell a coherent story in which those practices that require more financial investment, such as changing or diversifying with other crops, relate more to the financial and to the context-free estimate of risk. Risk preferences regarding one farm management practice, such as changing varieties or expenditures on fertilizer or pest control, correlate with each other. Thus, it is essential to elicit risk attitudes for the specific context of interest, and one should be careful when extrapolating risk attitudes across contexts.

2.5.2 Experimental evaluation of survey estimates

To better understand what the survey risk estimates capture, we test whether survey data predict risk-taking behavior in the incentivized experiment. We focus the analysis on the survey-based willingness to take risks in general (with no context) because the experiment is context-free as well. Our experimental data allows us to perform this validation in two ways: using the certainty equivalents directly and using utility function parameters.

First, to test the predictive power of the survey question, we regress the average certainty equivalent in each domain (i.e., gain and loss) on the respondent's answer to the general risk question (Table 2.4). To ensure robustness, we cluster the standard errors at the village level (33 clusters) using the Wild bootstrap method (Cameron et al. 2008). We add household head and coffee farming characteristics as controls and include district fixed effects. The coefficient for the willingness to take risks is positive and significant (p-value < 0.05), in both the gain and loss domain.

Second, we use the utility function parameters estimated from the experimental data to assess the predictive power of the survey-based willingness to take risks estimate. We regress each of the expected utility function parameters on the general willingness to take risk question (Appendix 2.4). Farmers reporting more willingness to take risks in the survey question are relatively less pessimistic, less sensitive to changes in probabilities that increase the likelihood of a loss in the experiment, and more loss averse. We associate this last result with the endowment effect, where people willing to take risks to avoid losses (Kahneman and Tversky 1979).

Our results show the general survey-based estimate predicts risk-taking behavior in the incentivized experiment, confirming our hypothesis. At the same time, while the significant parameters help us to understand the intuition behind farmers' willingness to take risks, these effects are too small to provide an economic interpretation.

Table 2.4 | Predicting experimental choices with the general survey-based estimate risk attitudes.

Dependent variable: Certainty equivalent, meaning the value of the safe option at the switching point. A higher CE means more risk-seeking		
	Mean CE in the gain domain (1)	Mean CE in the loss domain (2)
Willingness to take risks in general (no context) *(0 "not at all willing to take risks" 10 "very willing to take risks")*	**90.043****	**59.455****
	[39.077]	**[25.399]**
Constant	2,038.748***	-1,792.880***
	[0.000]	[670.791]
Control variables	Yes	Yes
Village fixed effects	Yes	Yes
Observations	278	278
R-squared	0.110	0.091

Each coefficient estimate is based on a separate OLS regression of the respective dependent variable on this particular risk estimate and a set of controls. Control variables included: gender, age, education (years), household size, household head labor in another farm, total farm area (ha), number of bedrooms in the house, farm experience (years), percentage of income coming from coffee, total area planted with coffee (ha), whether farmers bring coffee to a cooperative, and whether farm was affected by the coffee leaf rust. Fixed effects at the district level (6 districts). Cluster standard error at the village level (33 villages). Wild bootstrap with 1000 replications following Cameron et al. (2008). Robust standard errors in brackets, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

2.5.3 Risk attitudes and real-life farming choices

Finally, we study how the different survey estimates relate to real-life choices. Our dependent variables are a number of real-life farming choices: i. changing crops or diversifying the farm by adding other crops in the last decade; ii. changing coffee variety in the last decade; iii. the number of fertilizer applications; and iv. the number of pest control applications in the year before the survey.

We analyze our data using a linear probability model and regress the probability of changing each of the real-life farm practices on survey-based risk preferences (7 separate regressions). In Table 2.5, we present the results for each of the dependent variables. All regressions include a set of control variables including district fixed effects. We ensure robustness by clustering the standard errors at the village level (33 clusters) and applying the wild bootstrap following Cameron et al. (2008).

We start by looking at the estimates for *change or diversify with other crops* (Column 1). The coefficients for the general survey-based estimate (with no context), the financial context, and the farming context are not are significantly associated with changed or diversified with other crops. However, when the survey risk question is asked specifically regarding changing or diversifying crops, the coefficient is positive and significant (p-value < 0.05). Estimates regarding the context of changing the coffee variety, fertilizer and pest control applications are not significant.

Table 2.5 | Risk survey-based estimates on real-life farming practices.

		(1)	(2)	(3)	(4)
		Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
a)	General (no context)	0.007	0.002	-0.001	0.001
		[0.006]	[0.007]	[0.010]	[0.009]
		Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
b)	Finances context	0.009	-0.000	**-0.016***	-0.007
		[0.005]	[0.007]	**[0.009]**	[0.012]
		Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
c)	Farming context	0.008	0.008	0.006	-0.007
		[0.005]	[0.006]	[0.009]	[0.011]
		Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
d)	Crop change/diversify context	**0.013****	-0.002	0.004	-0.015
		[0.006]	[0.008]	[0.012]	[0.015]
		Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
e)	Variety change context	-0.001	**0.015****	-0.009	0.014
		[0.005]	**[0.006]**	[0.010]	[0.009]
		Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
f)	Fertilizer context	0.009	**0.014****	**-0.020****	0.008
		[0.006]	**[0.007]**	**[0.009]**	[0.008]
		Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
g)	Pest control context	0.002	**0.018*****	**-0.025****	0.013
		[0.008]	**[0.006]**	**[0.011]**	[0.011]
Constant		*Yes*	*Yes*	*Yes*	*Yes*
Control variables		*Yes*	*Yes*	*Yes*	*Yes*
Village fixed effects		*Yes*	*Yes*	*Yes*	*Yes*
Mean dependent variable		0.075	0.115	0.651	0.428
Observations		278	278	278	278

Each coefficient estimate is based on a separate OLS regression of the respective dependent variable on this particular risk estimate and a set of controls. Dependent variables: *change crop/diversify* (Changed or diversified with other crops (yes/no)), *change variety* (Changed coffee variety (yes/no)), *fertilizer* (Application of fertilizer (high/low)) and *pesticide* (Application of pesticide (high/low)). Control variables included: gender, age, education (years), household size, household head labor in another farm, total farm area (ha), number of bedrooms in the house, farm experience (years), percentage of income coming from coffee, total area planted with coffee (ha), whether farmers bring coffee to a cooperative, and whether farm was affected by the coffee leaf rust. Fixed effects at the district level (6 districts). Cluster standard error at the village level (33 villages). Wild bootstrap with 1000 replications following Cameron et al. (2008). Robust standard errors in brackets, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

When looking at the results for reporting *changed coffee variety* in real life (Column 2), we find again that the coefficient estimates for the general survey-based risk question (with no context), the financial context and the farming context are not are significantly associated with changing the coffee variety in real life. However, when explicitly contextualized as a willingness to take risks in the context of changing coffee variety, the coefficient is positive and significantly (p-value < 0.05). The estimate is not significant when contextualized in the changing crop context, but, regarding other management farm practices such as fertilizer and pest control context, coefficients are also positive and significantly associated with changing the coffee variety in real life (p-value < 0.05 and p-value < 0.01, respectively).

We continue by analyzing real-life decisions regarding the application of fertilizer and pest control (Columns 3 and 4). We transform these dependent variables into dummy variables with reference to the mean. Farmers who apply below-average fertilizer or pest control are coded zero, and farmers who apply above-average fertilizer or pest control are coded one.

Regarding the *application of fertilizer* (Column 3), we find the coefficient estimated for the general survey-based risk question (with no context) is not significantly associated with the application of fertilizer. However, when the survey question is explicitly contextualized as a willingness to take a risk in the financial context, the coefficient is negatively and significantly associated with the application of fertilizer (p-value < 0.10). The coefficients regarding the farming context or contextualized as changing crops or changing the coffee variety are not significantly associated with real fertilizer use. Furthermore, when the survey risk question is asked specifically in the fertilizer and pest control context, the coefficients are negative and significant for the application of fertilizer (p-value < 0.05, for both).

Finally, when looking at the results for the *application of pesticide* (Column 4), none of the coefficients regarding the willingness to pay in different contexts is significantly associated with the real application of pesticide.

We confirm the hypothesis that the context-specific willingness to take risk is positively associated with the implementation of changing or diversifying with other crops and changing coffee varieties. However, we cannot confirm the same for the context-free survey estimate. Previous studies using context-free estimates found risk aversion is associated with less implementation of agricultural practices that required more farm investment (Brick and Visser 2015; Holden and Quiggin 2017; Verschoor et al. 2016).

More surprising, we observe a negative sign on the coefficient when regressing the willingness to take risk questions on the real-life application of fertilizer. Farmers who report being less willing to take risks are more likely to apply more fertilizer. We know that, in theory, risk aversion can affect fertilizer investment in different ways and depends on crop characteristics. Previous studies typically find the contrary: that risk aversion is associated with less fertilizer purchase (Khor et al. 2015; Roosen and Hennessy 2003; Verschoor et al. 2016), as fertilizer can increase not only output but also its variability (Vablauwe et al. 2016). Our findings are consistent with studies showing that applying fertilizer reduces the risk of pests and low yields (Avelino et al. 2015) and can be perceived as a risk-reducing strategy. Similar results regarding pesticide have been found in China (Liu and Huang 2013), with high availability and input used. Therefore, we expected the results for pesticide to be similar to those of fertilizer. Since this was not the case, we reflect on farmers applying pesticides in a reactive way rather than in a preventive manner. Furthermore, pesticide is a broad category that comprises a different kind of inputs (i.e., fungicide, insecticide, and nematicide), and varies widely with environmental and geographical conditions.

To complement our previous analysis, we take a look at how the utility function parameters derived from the experiment relate to real-life farming practices (Appendix 2.5). Interestingly, the experimental risk preferences parameters show that optimism (β^-) and the loss aversion parameter (λ) are positively and significantly associated with the high application of fertilizer (p-value < 0.10)[17]. In other words, optimistic farmers with higher loss aversion are the ones applying more fertilizer to avoid agricultural losses. Loss aversion leads people to value what they have more than equal things that they do not have (Levy 1996) and to over-value current possessions – an endowment effect (Thaler 1980). We think that, because coffee is a perennial crop, plantations are considered as possessions that increase the value of the land, reflecting an endowment effect. As a result, farmers apply more fertilizer to prevent losses.

17 We only find the optimism and loss aversion parameters associated with fertilizer application. No significant relationship is found with other parameters and farm practices. A recent paper by Verschoor et al. (2016) explains that some farming decisions are more likely to be a broadly bracketed decision (i.e., decisions involved a radical overhaul and were considered as part of an overall livelihood strategy). A practice like fertilizer application is a narrowly bracketed decision (i.e., a straightforward investment that can be applied on a modest scale). This distinction has consequences for which estimates are associated with real-life farming choices. Risky choices in an experiment based on reference-dependent utility theory will reflect only those real-life choices that maintain the assumption of narrow bracketing (i.e., fertilizer).

We did not foresee that the survey risk question would be associated with the real farm practice only when the question is explicitly contextualized for the specific farming practice, except for pesticide. We acknowledge that reverse causality might be a concern and prior experience with a particular farm practice – for example, crop diversification – might make a farmer report more willingness to take the risk of diversifying her farm. However, farmers' real decisions and our survey happened in totally different moments in time, and the argument that having good prior experience with a practice should make the farmer more willing to take risks could also be true for the opposite claim. (i.e., when a farmer has bad prior experience with crop diversification she should be less willing to take a risk at the time of the survey). Nonetheless, our argument is in favor of highly targeted risk preferences estimates, and we believe that claim still holds even if endogeneity could be a potential confounding factor. Still, we should not consider our results a predictive or causal analysis.

2.6 Conclusions and discussion

In this paper, we evaluate a survey-based method for estimating risk attitudes that can be easily implemented by practitioners in developing countries. We assess the correlations between a general survey-based risk estimate (with no context) and context-specific estimates. We test whether survey data predicts risk-taking behavior in an incentivized experiment. Finally, we show how the different survey-based estimates of risk attitudes relate to real-life agricultural choices in a population of coffee farmers in Costa Rica.

Our findings can be summarized in three main results. First, regarding the consistency of survey risk preferences across contexts, we find it is essential to elicit risk attitudes for the specific context of interest since there is no one domain-general component across specific contexts. A principal component analysis shows that we can group risky choices in two components: one grouping general-financial investment decisions and the other grouping crop-specific farm management. We do not know of previous studies showing the consequences for the agricultural specific context.

Second, the general survey-based estimate of risk (with no context) predicts risk-taking behavior in the experiment in line with previous studies (Dohmen et al. 2011; Vieider et al. 2015). Regarding the utility function parameters, we find that higher willingness to take risk is associated with less pessimism, less sensitivity to changes in probabilities that increase the likelihood of a loss, and more loss aversion, suggesting farmers are willing to take risk in order to avoid losses. We do not know of other studies relating survey risk estimates to risk preferences parameters.

Third, we find differences between survey estimates with and without a context in relationship with agricultural real-life behavior. The context-free survey estimate of risk-taking is not associated with actual risk-taking behavior, and only estimates in the specific agricultural context correlate with real-life farming choices. Furthermore, higher willingness to take risk is associated with the implementation of agricultural practices that required more farm investment, like changing or diversifying with other crops and changing to other coffee varieties. In contrast, farmers that report being less willing to take risks are more likely to have higher expenditures on fertilizer, and we found no significant relationship with the use of pesticide.

In the face of costly investment, farmers balance the advantages regarding reducing exposure to uncertainty in agriculture with the increased exposure to financial risk, for example acquiring loans to replace their plantation with improved coffee varieties that are resistant to pests. Hence, is not strange that practices that required more farm investment are associated with higher risk. On the other hand, coffee farmers in Costa Rica have good access to information regarding input use from cooperative organizations and extension agents, which contributes to their perception of fertilizer as a risk-reducing technology. Furthermore, differences in decision making can reflect a distinction between a broadly bracketed and a narrowly bracketed decision. Future research should take into account whether estimates of risk preferences are biased by incorrect assumptions about bracketing (Barseghyan et al. 2018).

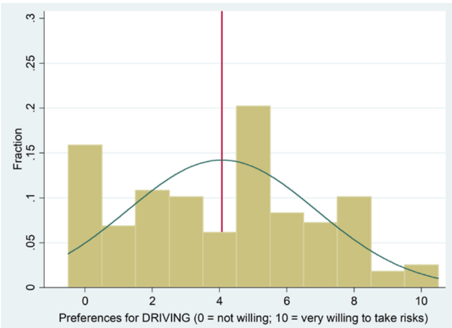
A common assumption in economics is the stability of risk preferences across decision contexts (Barseghyan et al. 2018). Our study shows that we cannot assume one general trait across different specific contexts within agriculture. Similar results have been found in other contexts, for example, insurance choices (Barseghyan et al. 2013; Einav et al. 2012) and social preferences (de Oliveira et al. 2012). Understanding how preferences are affected by context and the type of decision is key to use actions from one context to predict actions in another context (de Oliveira et al. 2012).

Projects or programs interested in using risk preferences as inputs into the design of policy instruments should make sure that preferences are elicited in the specific context targeted by the prospective policy instrument. If the policy instrument aims at influencing general financial decisions, the estimation of risk preferences without a context might suffice. However, if the policy instrument target a specific adoption, say fertilizer use or implementation of improved seeds varieties, risk preferences should be elicited in that particular context.

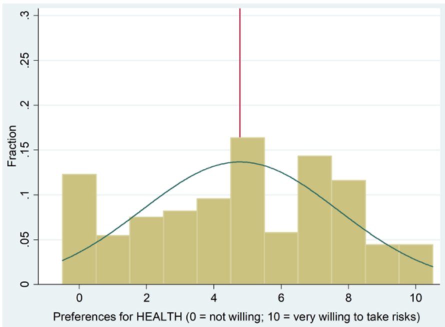
2.7 Appendices

Appendix 2.1 | Summary of aggregate experimental risk preferences estimates by prospect.

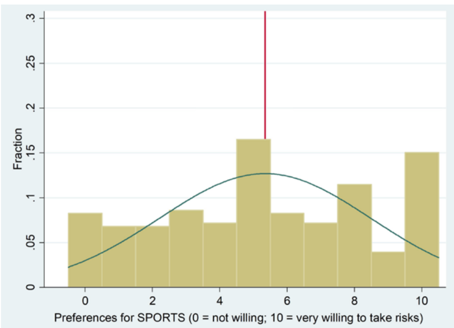
Gain domain								
prob.	*EV*	*mean CE*	*median CE*	*SD*		*test = EV*		
0.1	500	2022.47	1875.00	1582.64	t =	16.4665	p =	0.0000
0.5	2500	3257.74	3375.00	1256.36	t =	10.3238	p =	0.0000
0.9	4500	4327.11	4750.50	1042.05	t =	-2.8400	p =	0.0048
Mean	**2500**	**3202.44**	**3333.50**	**1293.68**	**t =**	**11.8228**	**p =**	**0.0000**
Loss domain								
prob.	*EV*	*mean CE*	*median CE*	*SD*		*test = EV*		
0.1	-500	-555.17	-249.00	880.21	t =	-1.0729	p =	0.2842
0.5	-2500	-1718.72	-1875.00	1176.94	t =	11.3629	p =	0.0000
0.9	-4500	-2834.46	-2875.00	1541.75	t =	18.4916	p =	0.0000
Mean	**-2500**	**-1702.78**	**-1666.33**	**1199.63**	**t =**	**14.6947**	**p =**	**0.0000**



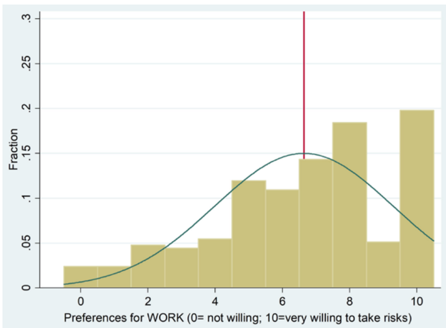
2.2.1 Farmer's preferences for driving vehicles



2.2.2 Farmer's preferences regarding health



2.2.3 Farmer's preferences for practicing sports



2.2.4 Farmer's preferences regarding work

Appendix 2.2 | Farmer's responses to other survey context-specific risk preferences estimates.
Note: Red line indicates mean response to risk question and blue line shows the adjusted probability distribution

Appendix 2.3 | Principal component analysis (PCA).

Appendix 2.3.1 | Principal components and correlation.

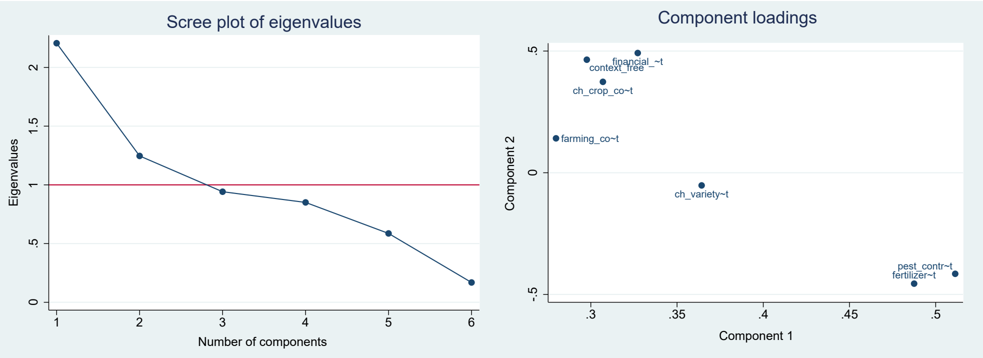
Component	Eigenvalue	Difference	Proportion	Cumulative
Comp1	2.35027	0.866991	0.3358	0.3358
Comp2	1.48328	0.532159	0.2119	0.5477
Comp3	0.951121	0.020768	0.1359	0.6835
Comp4	0.930353	0.324798	0.1329	0.8164
Comp5	0.605556	0.093846	0.0865	0.9029
Comp6	0.51171	0.344001	0.0731	0.976
Comp7	0.167708.		0.024	1

Number of observations 293. Trace = 7. Rotation: (unrotated = principal). Rho = 0.5477

Appendix 2.3.2 | Principal components (eigenvectors) and rotated components.

Variable	Component 1	Component 2	Unexplained
General (no context)		0.5490	0.4717
Finances		0.5889	0.3895
Farming			0.7863
Changing crop		0.4834	0.5716
Changing coffee variety	0.3183		0.6841
Pest control used	0.6587		0.1297
Fertilizer used	0.6649		0.1335

Number of observations 293. Number of comp. = 2. Trace = 7. Rotation: orthogonal varimax (Kaiser off). Rho = 0.5477. (blanks are abs(loading) < .3)



Appendix 2.3.3 | Screen plot of the eigenvalues and scatter plots of the loadings and score variables.

Appendix 2.3.4 |Kaiser-Meyer-Olkin measure of sampling adequacy.

Variable	(kmo)
General (no context)	0.608
Finances	0.6167
Farming	0.7484
Changing crop	0.6224
Changing coffee variety	0.7599
Pest control used	0.5619
Fertilizer used	0.5478
Overall	0.5975

Appendix 2.4 | Willingness to take risks in general and utility function parameters.

	Dependent variable: utility function parameters				
	α^+	β^+	α^-	β^-	λ
	(sensitivity gains)	*(pessimism)*	*(sensitivity losses)*	*(optimism)*	*(loss aversion)*
	(1)	(2)	(3)	(4)	(5)
Willingness to take risks in general (0-10)	0.00003	-0.00014***	-0.00009**	0.00013	0.00104***
	[0.00004]	[0.00005]	[0.00004]	[0.00008]	[0.00038]
Constant	0.60227***	0.54578***	0.41866***	1.37900***	3.88161***
	[0.00000]	[0.00000]	[0.00000]	[0.00000]	[0.00000]
Control variables	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes
Mean dependent variable	0.604	0.544	0.419	1.380	3.892
Observations	278	278	278	278	278
R-squared	0.088	0.123	0.096	0.060	0.097

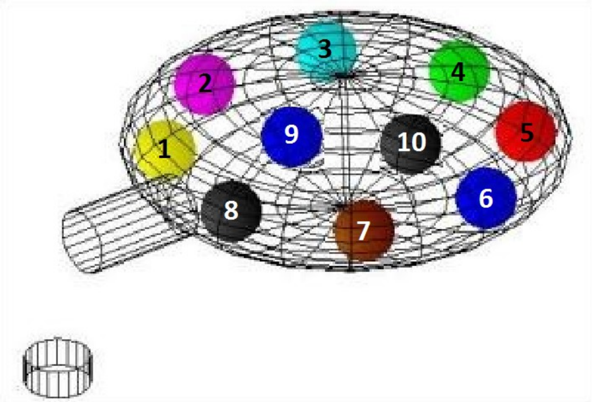
Each coefficient estimate is based on a separate OLS regression of the respective dependent variable on this particular risk estimate and a set of controls. Control variables included: gender, age, education (years), household size, household head labor in another farm, total farm area (ha), number of bedrooms in the house, farm experience (years), percentage of income coming from coffee, total area planted with coffee (ha), whether farmers bring coffee to a cooperative, and whether farm was affected by the coffee leaf rust. Fixed effects at district level (6 districts). Cluster standard error at the village level (33 villages). Wild bootstrap with 1000 replications following Cameron et al. (2008). Robust standard errors in brackets, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix 2.5 | Risk preferences parameters on real-life farming practices.

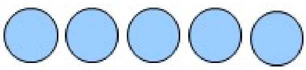
	(1)	(2)	(3)	(4)
	Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
$\boldsymbol{\alpha}^{+}$ *(sensitivity gains)*	-1.590	-9.988	-2.473	-4.779
	[10.884]	[9.778]	[15.831]	[11.201]
	Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
$\boldsymbol{\beta}^{+}$ *(pessimism)*	2.611	8.446	-11.151	7.312
	[10.191]	[13.629]	[19.206]	[17.827]
	Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
$\boldsymbol{\alpha}^{-}$ *(sensitivity losses)*	-3.505	-3.851	-27.168	19.706
	[12.534]	[22.802]	[27.380]	[21.579]
	Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
$\boldsymbol{\beta}^{-}$ *(optimism)*	-7.149	-1.439	**23.699***	8.143
	[10.437]	[7.601]	**[12.644]**	[12.227]
	Change crop/diversify	*Change variety*	*Fertilizer*	*Pesticide*
$\boldsymbol{\lambda}$ *(loss aversion)*	-1.073	-0.687	**4.276***	0.361
	[1.658]	[1.630]	**[2.246]**	[5.140]
Constant	*Yes*	*Yes*	*Yes*	*Yes*
Control variables	*Yes*	*Yes*	*Yes*	*Yes*
Village fixed effects	*Yes*	*Yes*	*Yes*	*Yes*
Observations	278	278	278	278

Each coefficient estimate is based on a separate OLS regression of the respective dependent variable on this particular risk estimate and a set of controls. Dependent variables: *change crop/diversify* (Changed or diversified with other crops (yes/no)), *change variety* (Changed coffee variety (yes/no)), *fertilizer* (Application of fertilizer (high/low)) and *pesticide* (Application of pesticide (high/low)). Control variables included: gender, age, education (years), household size, household head labor in another farm, total farm area (ha), number of bedrooms in the house, farm experience (years), percentage of income coming from coffee, total area planted with coffee (ha), whether farmers bring coffee to a cooperative, and whether farm was affected by the coffee leaf rust. Fixed effects at district level (6 districts). Cluster standard error at the village level (33 villages). Wild bootstrap with 1000 replications following Cameron et al. (2008). Robust standard errors in brackets, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

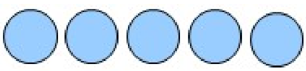
Appendix 2.6 | Example of decision task in the gain domain.



You win ¢ **5000** if one of the following balls is drawn from the urn:



You win ¢ **0** if one of the following balls is drawn from the urn:



	Lottery		Sure amount
1	O	O	250 ¢ for sure
2	O	O	500 ¢ for sure
3	O	O	750 ¢ for sure
4	O	O	1000 ¢ for sure
5	O	O	1250 ¢ for sure
6	O	O	1500 ¢ for sure
7	O	O	1750 ¢ for sure
8	O	O	2000 ¢ for sure
9	O	O	2250 ¢ for sure
10	O	O	2500 ¢ for sure
11	O	O	2750 ¢ for sure
12	O	O	3000 ¢ for sure
13	O	O	3250 ¢ for sure
14	O	O	3500 ¢ for sure
15	O	O	3750 ¢ for sure
16	O	O	4000 ¢ for sure
17	O	O	4250 ¢ for sure
18	O	O	4500 ¢ for sure
19	O	O	4750 ¢ for sure

3

Testing conditional cooperation

Local participation of farmers in agricultural cooperatives

Abstract

In this paper, we test the internal and external validity of the typology of a conditional cooperator classified by using a public goods game together with the strategy method. Individuals categorized as conditional cooperators adapt their behavior to the group to which they belong. In Costa Rica, coffee farmers are traditionally organized in agricultural cooperatives, a setting very similar to the scenario presented to an individual facing the strategy method in a public goods game: how much to cooperate, given what others do. Our results show that conditional cooperators believe they contribute to the public good by matching the contribution of others in the experiment. However, we find no evidence that those classified as conditional cooperators in the experiment also behave this way when it comes to bringing coffee to the local cooperative in real life. We show supporting evidence to conclude that the typology of a conditional cooperator is internally consistent, but do not find evidence that the typology of conditional cooperators is externally valid. Our paper is a contribution to the external validity of context-free experiments and helps in understanding cooperative behavior relevant to the sustainability of agricultural cooperatives in the developing world.

This chapter is based on:

Naranjo, M.A., Martinsson, P. Alpizar, F. (2019) “Testing conditional cooperation: Local participation of farmers in agricultural cooperatives.” (Working paper).

3.1 Introduction

Individual social preferences and cooperation influence collective action, with consequences for the sustainability of contracts and institutions (Charness and Rabin 2002; Fehr and Fischbacher 2002; Fischbacher and Gächter 2010; Fehr and Schmidt 1999), which are essential for community development. Social preferences and cooperation are often studied through public goods games. In this paper, we test the internal and external validity of the typology of conditional cooperator, identified by implementing a public goods game combined with the strategy method among farmers traditionally organized in agricultural cooperatives. Individuals characterized as conditional cooperators adapt their behavior to the group to which they belong (Fischbacher et al.2001). In other words, if other group members shirk, they shirk as well, and if others cooperate, conditional cooperators follow through (Gächter 2007). In real life, participation in an agricultural cooperative consists of taking the harvest for processing to the cooperative mill, in exchange for better prices and cheaper access to technical assistance, as well as other benefits. These benefits will happen only if enough farmers cooperate in sustaining the structure of the cooperative. In other words, the agricultural cooperative setting is similar to the scenario presented to an individual facing the strategy method in a public goods game: how much to cooperate, given what others do.

A farmers' agricultural cooperative is a typical example of an institution that strongly relies on the cooperation of its members. Cooperatives are an essential vehicle for development, mobilizing local resources for a common goal that provides benefits to their associates (Zeuli and Radel 2005). The participatory structure of coffee cooperatives in Costa Rica and many developing countries reflects some of the characteristics of a public good. Membership is voluntary[18] and it is not possible to exclude members from the services of the cooperative, nor from the benefits that a cooperative typically provides to the community. Like many other institutions in the developing world, agricultural cooperatives need active cooperation from farmers to be sustainable. They represent a policy-relevant setting to test the external validity of public goods games and the strategy method.

Previous literature has focused on the external validity of public goods games. Some studies have found a positive correlation between experimental and real-life behavior (Benz and Meier 2008; Carlsson et al. 2014; Frey and Meier 2004; Rustagi et al. 2010). Others have found that behavior in experiments is not consistent with real-life actions (Laury and Taylor 2008; Voors et al. 2011, 2012) . The evidence is not only mixed, but there is little evidence regarding the typology of conditional cooperators.

18 An exception is Chinese cooperatives, where membership is mandatory and universal so that every farmer in the village is automatically a member.

The objective of this paper is to test both the internal and external validity of the conditional cooperator typology among farmers who have to decide whether or not to cooperate with the coffee cooperative. Our study contributes to this debate by directly applying a public goods game combined with the strategy method to identify the conditional cooperators (Fischbacher et al. 2001), and comparing the farmers' pro-social behavior within the experiment and outside the laboratory.

To test for internal consistency, we explore whether those identified as conditional cooperators act as such when contrasting their contributions to their beliefs about the contributions of others in the experiment. In theory, conditional cooperators should believe in contributing by matching the contributions of others in the group. We hypothesize that the conditional contribution (from the strategy method) as a function of the individual beliefs of others in the experiment is correlated with the farmer's actual contribution to the public goods game.

To test for external consistency, we compare whether the type identified in the experiment is also a characterization of real-life behavior towards the cooperative. Theoretically, conditional cooperators will either cooperate or not, according to the behavior of others in the community. We hypothesize that farmers classified as conditional cooperators bring coffee to the cooperative if the share of other farmers bringing coffee to the cooperative increases. Accordingly, those who are not conditional cooperators should not be affected by the actions of other farmers in the community.

We find a moderate correlation between the conditional contribution as a function of the individual beliefs of others in the experiment and the majority of conditional cooperators believing they contribute to the public good by matching the contribution of others in the experiment. However, we do not find a significant interaction effect between those classified as conditional cooperators in the experiment and the share of other farmers bringing coffee to the cooperative in real life. We show supporting evidence to conclude that the typology of a conditional cooperator is internally consistent, but do not find evidence that the typology of conditional cooperators is externally valid.

The rest of this paper is organized as follows: The next section describes our theoretical framework complemented by a review of recent literature on the external validity of public goods experiments. The third introduces the background of coffee cooperative organizations in Costa Rica. Section four describes our experimental design and fieldwork implementation. Section five explains the sample selection and shows descriptive statistics. Section six presents our empirical strategy and the results, and the last section concludes the paper.

3.2 Related literature

Our empirical application is motivated by the seminal characterization of social preferences, a review of the literature focused on the external validity of public goods games, and the importance of studying social preferences heterogeneity in the context of agricultural cooperatives.

As defined by Fehr and Fischbacher (2002), individuals reveal social preferences when they do not care only about the resources allocated to them, but also about the resources allocated to other relevant agents. Evidence has shown that social preferences shape a substantial fraction of people's choices (Fischbacher et al. 2001; Fehr and Fischbacher 2002; Kocher et al. 2008; Martinsson et al. 2013).

Fischbacher et al. (2001) were the first to study the hypothesis that some people are *conditional cooperators*: they are willing to cooperate more, the more others contribute to a public good. Their work has been followed by many practical applications showing that conditional cooperators account for between 50% and 80% of the participants in the population (Fischbacher et al. 2001; Fehr and Fischbacher 2002; Kocher et al. 2008; Martinsson et al. 2013).[19]

Alternatively, individuals whose behavior does not change as a response to the behavior of others are called *unconditional cooperators* (Martinsson et al. 2009). If they place a positive value on the resources allocated to others, individuals are considered *altruistic* (Fehr and Fischbacher 2002; Putterman 2006). In contrast, when individuals behave in a purely selfish manner, they are called *free riders* if they cannot be excluded from the benefits of collective action and public goods. As a result, the interaction between conditional cooperators and free riders has an impact on the dynamics of markets and organizations (Fehr and Fischbacher 2002).

Inherently, conditional cooperation is a two-way road. Conditional cooperators adapt their behavior to the group to which they belong, meaning that if other group members defect, they defect as well; if others cooperate, conditional cooperators also cooperate (Gächter 2007). Therefore, conditional cooperation can either strengthen or undermine institutions that require cooperation to be sustainable. The composition of the group is decisive in maintaining cooperation (Rustagi et al. 2010), and cooperation is achieved in groups with a larger share of conditional cooperators (Gunnthorsdottir et al. 2007; Ones and Putterman 2007). Institutions where most individuals are conditional cooperators need to have policies that stand the beliefs for the cooperation of their members. If free riders dominate, procedures should involve monitoring and penalties to reinforce cooperation (Gächter 2007; Martinsson et al. 2009).

19 See Martinsson et al. (2013) for an overview of results found in different countries.

Regarding the external validity of public goods games, some studies have found a positive correlation between experimental and real-life behavior. For example, Benz and Meier (2008) find evidence that pro-social behavior is accentuated in the lab but is to some extent correlated with behavior in the field. Carlsson et al. (2014) find that correlations between public goods game contributions and real-life contributions are not only present but also stable over long periods and contexts. On the other hand, some studies have found that behavior in experiments is not consistent with real-life actions. A series of studies by Voors et al. (2012, 2011) finds no correlation between two experiments: a social intervention that mimics a public goods game and the conventional public goods game (Voors et al. 2012), nor a robust pattern between the previous experiments and real-life behavior towards forest conservation in Sierra Leone (Voors et al. 2011). Laury and Taylor (2008) suggest that not all estimates of social preferences from a laboratory setting predict contributions on naturally occurring public goods. They find some estimates of altruistic behavior carry over to real life, but it was not the same for the free riders.

Recent literature has shown the importance of studying preferences heterogeneity in the context of conditional cooperation. Rustagi et al. (2010) show evidence on the extent to which variation in the composition of the group explains the success of forest management in Ethiopia. They combine experimental estimates of conditional cooperation and survey data among organized forest user groups. Groups vary in the share of conditional cooperators, and groups with a larger conditional cooperator share are more successful in forest management. They also show that instruments such as costly monitoring are essential to enforcing cooperation among conditional cooperators. Frey and Meier (2004) find that charitable giving increases, on average, if people know that many others contribute as well.

Overall, we found few studies that assess the external validity of the typology of conditional cooperation (Frey and Meier 2004; Rustagi et al. 2010). Our study contributes to this debate by applying a public goods game following the strategy method (Fischbacher et al. 2001) and comparing the farmers' pro-social behavior in an experiment and outside the laboratory using a population of coffee farmers in Costa Rica.

In the next section, we introduce the background of coffee cooperatives in our study area, their participatory structure, and why conditional cooperation is essential for the sustainability of these organizations.

3.3 Background on coffee cooperatives

Traditionally, coffee farmers in Costa Rica are organized in cooperatives. The cooperative supports farmers with access to inexpensive technical assistance and training to improve productivity and reduce the risk of pests and diseases. Coffee growers who choose to get their beans processed at the cooperative mill add value to the product and get better prices through sales to international roasters if quality and quantity of the coffee are high. However, it is not possible to exclude members from the mill or other services if they produce poor quality beans. Moreover, members can choose between using the cooperative mill (receiving higher prices, but not immediate payment) and using private mills (lower prices, immediate payment). Furthermore, benefits from cooperatives reach the entire community. Agricultural cooperatives are well known for sponsoring sports and cultural and educational projects in the coffee regions, and it is not possible to exclude any member of the community from these benefits. Hence, our analysis looks at farmers' cooperation towards the cooperative regardless of membership, as benefits are shared by the entire community.

Our study took place in 2014 in two coffee regions in Costa Rica: Tarrazú and Brunca. In the Tarrazú area, there are three main cooperatives: Coope Llano Bonito, Coope Dota and Coope Tarrazú, which have 600, 900 and 4650 members respectively. In the Brunca region, there is one big cooperative call Coope Agri that registers a total of 10,162 associates and is the largest cooperative in the country.[20]

The structure of coffee cooperatives resembles the characteristics of a public good.[21] It is not possible to exclude those who do not cooperate. In principle, farmers should cooperate with one another by bringing their coffee to the local cooperative. However, in reality, many farmers bring all or part of their coffee harvest to private mills that attract farmers with direct cash payments. Prices offered by the private mills are not necessarily higher than the price provided by the cooperatives, but payments are given to the farmer on the spot. In contrast to the private mills, when farmers bring their coffee to a cooperative, they receive

20 Coope Agri also processes other agricultural products, not only coffee, and provides financial services.

21 The governance structure in these cooperatives is participatory. For example, in Coope Tarrazú, for every 15 associates, a delegate is appointed to be part of the General Assembly. The General Assembly's main functions are to approve the policies of the cooperative and to decide about surpluses, coffee processing, and commercialization. Also, the General Assembly appoints other governing bodies, including the Administration Board, the Surveillance Committee, the Education Committee and the Arbitration Board (CoopeTarrazú 2018). Once the Administration Board is elected, each of the administrative positions is appointed internally. The Board of Directors decides all matters related to the management of the cooperative, and selects the General Manager, the Internal Audit and the External Audit (CoopeTarrazú 2018).

payments distributed throughout the year (see Appendix 3.1 for a list of prices and Appendix 3.2 for an example of timing of the payments in 2014).[22]

During harvesting season, the coffee beans must be processed quickly, before they can ferment, and therefore the farmers bring their coffee to collection points available in each village soon after harvest. These collection points are set up by both the cooperative and the private mills, and are spread around the villages. Farmers gather in line at the end of the day to deliver the coffee at these collection points. Hence, the information about who brings their coffee where, either to the private or the cooperative mill, can be considered public information among farmers.

Agricultural cooperatives need cooperation from the local farmers to keep functioning. Contracts with international coffee roasters are made in advance (before the harvest), and quotas must be achieved. If the majority of farmers are conditional cooperators, they will bring coffee to the cooperative mill if others farmers in the community do so, but they will not participate if other farmers do not. Therefore, conditional cooperation can either enhance or weaken cooperation, depending on the proportion of farmers who are free riders and do not bring their coffee for processing.

The next section describes the experimental design and the procedures followed to derive a typology of social preferences among this population.

3.4 Experimental design and procedures

Our experiment is based on the experimental design by Fischbacher et al. (2001). Farmers have to decide how to divide an endowment of 20 points, which at the end of the game is converted to colones (1 point = 200 colones).[23] Farmers are told that they are simultaneously playing in a group with two other anonymous peer farmers from the community chosen at random.

Farmers' tasks consist of three decisions: the unconditional contribution, the conditional contribution and the guessed contribution (detailed instructions are given in Appendix 3.4). The first task is the unconditional contribution, i.e., farmers' willingness to contribute to the public good. Farmers decide how many of the 20 points to deposit into the project account, and the rest of the points go to the

22 Both cooperative and private mill prices are regulated by law. The final price is published before the harvest season and includes a 9% from the final liquidation price in favor of the coffee mill for the processing and marketing of coffee (MAG 2016).

23 In dollars, the 20 points are equivalent to 7 USD.

personal account with the following payoff function:

$$\pi_i = 20 - g_i + 0.5\sum_{j=1}^{3} g_j$$

where g_i is the unconditional contribution, and the public good pays back the sum of all contributions g_j multiplied by 0.5. The enumerator tells the farmers that he or she is going to send an SMS with their choices to the coordinator. The coordinator collects the information from the other participants and calculates the payoffs (see Appendix 3.5).

The second task is to complete the table of contributions. The farmer must indicate, for each possible average contribution of the other two farmers in the group, the number of points to put in the project account (see Appendix 3.6). Farmers are told that one member of each group is selected, and their income will be determined according to the table of contributions. For the other two farmers in the group, the income will be the unconditional contribution payoff.

In the final task, we asked the farmers how much they think the other two farmers of the group have contributed in task 1 (i.e., unconditional contribution) (see Appendix 3.7). If the farmer guesses the average contribution from the other two farmers to the project account, they could earn additional points. If the exact unconditional contribution of others is equal to the guessed contribution, the farmer earns four extra points. If the real contribution is a point above or below, the farmer earns three extra points. If the contribution is two points above or below, the farmer earns one extra point. At the end of the game, the coordinator arrives with the information collected from the other two farmers and payment is given in cash directly to the farmer.

3.5 Sample selection and data

We applied the public goods game as part of a survey. By doing this, we ensure a random selection of participants in the game as well as the survey.[24] Households were sampled through a stratified random sampling based on the density of coffee plots within six districts of the two coffee regions (three districts from each region).[25] Districts were chosen to capture the spread and variation of intensity of the coffee

24 A public goods game is commonly implemented as a lab-in-the-field experiment, gathering participants in a communal area and making an open call, or inviting subjects to participate in the game. This procedure can have a self-selection bias, which we avoid by applying the public goods game as part of the survey.

25 Costa Rica's national public administration divides the country into provinces, cantons, and districts.

rust epidemic in 2012-2013.[26] Household head farmers took part in the fieldwork data collection carried out by trained enumerators at each household. First, a survey questionnaire collected detailed household characteristics, farming practices and community participation in different organizations. Then, the enumerator introduced the public goods game.

In our final sample, we have 293 coffee farmers. Their household socioeconomic characteristics and coffee farm characteristics are presented in Table 3.1. In our sample, coffee farmers have on average only primary education, life experience in coffee farming, and on average 57% of their income is earned through selling coffee. For our main cooperation variable, we gathered information on where farmers delivered their coffee. Specifically, we asked for each of their plots whether the coffee was delivered to the cooperative mill, to a private mill or both. Hence, we know with certainty that those bringing coffee to the cooperative mill are members, but we are not able to identify those who are members and do not bring coffee to a local cooperative. Nonetheless, benefits of the cooperative reach the entire community – members and non-members – and so farmers who do not bring coffee could be considered free riders in the community.

Table 3.1 | Descriptive statistics of main variables.

	N	*mean*	*sd*	*min*	*max*
Household variables					
Willingness to contribute to public goods game	293	9.67	3.83	0	20
Household head female	293	0.10	0.30	0	1
Age (years)	293	51.77	13.64	19	86
Education (years)	293	5.80	2.61	0	15
Household size	293	3.33	1.38	1	10
Total farm area (ha)	293	5.49	9.96	0.03	109
Coffee farm characteristics					
Farmer experience (years)	293	25.50	14.48	1	71
% of income coming from coffee	278	56.78	36.20	0	100
Total area planted with coffee (ha)	293	3.48	4.62	0.09	41.77
Farm affected by coffee leaf rust	293	0.81	0.39	0	1
Have a credit with the cooperative	293	0.11	0.32	0	1
Farmers bringing any coffee to a private mill	293	0.26	0.44	0	1
Main variables					
Farmers bringing any coffee to a cooperative (P_i)	293	0.78	0.42	0	1
Share of other farmers in the village bringing coffee to the cooperative (PO_{ij})	293	0.78	0.17	0.29	1

26 Coffee rust epidemic was an exogenous shock which affected coffee plantations at an altitude between 700 and 2000 meters above sea level.

In our empirical model, farmers' cooperation is defined by bringing or not bringing coffee to the local cooperative (P_i). We focus on the interaction of the typology of social preferences with the cooperation of other farmers in the village. To measure the participation of other farmers in the village (PO_{ij}), we take the share of other farmers in the village who bring their coffee to the cooperative, excluding farmer *i*.

3.6 Empirical strategy and results

Following Fischbacher et al. (2001) and Fischbacher and Gächter (2010), we used the experimental data to classify farmers in distinct categories: the conditional cooperators, the unconditional contributing over the mean, the unconditional contributing below the mean, the free riders, and the remaining others (Figure 3.1). *Conditional cooperators* are farmers whose own conditional contribution increases weakly and monotonically with the average contribution of other members. These include subjects for whom the relationship between their average contribution and that of others is positive and significant at the 1% significance level, based on the Spearman correlation coefficient (Martinsson et al. 2013). The farmers who always report the same contribution from the first task, independently of the average contribution of the other two farmers, are classified as *unconditional cooperators* (Martinsson et al. 2009).

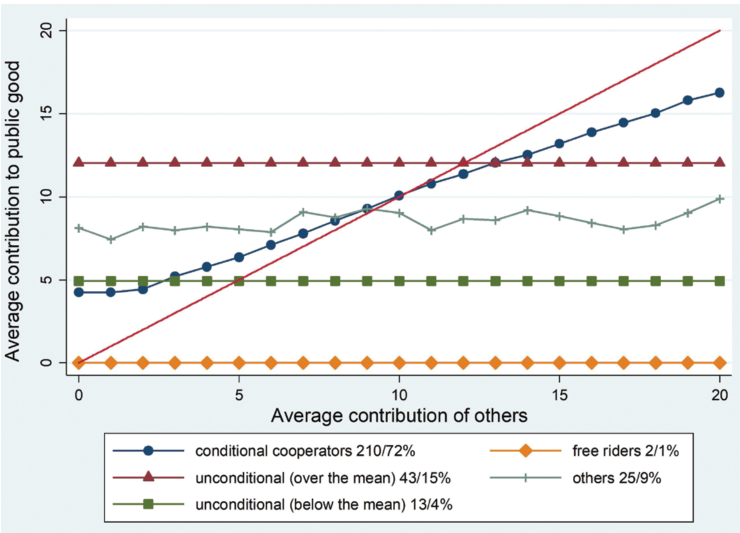


Figure 3.1 | Average own conditional contribution for each contribution level of the other group members (diagonal = perfect conditional cooperators).

3

Our biggest group is the *conditional cooperators* with 210 farmers (72%), who tend to match the expected contributions of others. This percentage is consistent with results from previous lab studies. However, in previous research, the majority of conditionally cooperative contributions tend to lie at or below the diagonal (in the selfish direction) (Fischbacher et al. 2001). Conversely, in our sample, conditional cooperator farmers start contributing on average above the diagonal, and, after the average contribution of 10 tokens, contributions fall below the diagonal.

Unconditional cooperators account for 19% (56 of the subjects). These include 43 farmers (15%) who always contribute above the mean and 13 who always contribute below the mean (4%). We found only two free riders displaying purely selfish behavior with a contribution of zero (1%).[27] Finally, 25 farmers (9%) are classified as *other*. For further analysis, we include the two free riders within the group of others.[28]

We start by testing the internal validity of the typology of conditional cooperators. As detailed in section 3.4, our experiment included three tasks. Task 1 asked farmers their willingness to contribute to a public good. In task 2, we elicited farmers' willingness to cooperate conditionally on the average cooperation of other farmers, and, in task 3, we asked farmers about their beliefs regarding the contribution of others.

We wanted to know if the conditional contribution (task 2), as a function of the individual beliefs of others (task 3), is correlated with the farmer's actual contribution to the public goods game (task 1). We test the null hypothesis that the public good contribution (task 1) and conditional contribution as a function of the individual beliefs about others (task 2(task 3)) are independent. We reject the null hypothesis and find a Spearman's correlation coefficient equal to 0.4126, showing a moderate correlation which is highly significant (p-value < 0.01).

Furthermore, we compared one by one to see whether those classified as conditional cooperators remained consistent in the experiment. For example, if a farmer believed that on average the other two farmers contributed five tokens (in task 3), we looked at how much the farmer was willing to contribute if the other two farmers contributed on average five (table of contributions, task 2), and then compared this result with their original contribution (task 1). Figure 3.2 shows the frequency of farmers matching their beliefs with their willingness to contribute to the public

27 Previous studies in the lab typically found a higher percentage of pure free riders (4-6%) (Martinsson et al. 2013).

28 Previous literature also identifies a category called the *hump-shape contributors* (Fischbacher et al. 2001; Fehr and Fischbacher 2002; Kocher et al. 2008; Martinsson et al. 2013). These are subjects showing a monotonically increasing contribution up to an average level of others, after which contributions decrease. From our data, we do not find farmers displaying this behavior.

good. The large majority of conditional cooperators, a total of 130 out of the 210 (62%), remained "consistent" within -/+ 1 standard deviation of 3.83 ≈ 4 from their original contribution. Furthermore, 31% perfectly matched their contribution to their beliefs. Our results show supporting evidence to conclude that the typology of a conditional cooperator is consistent within the experiment.

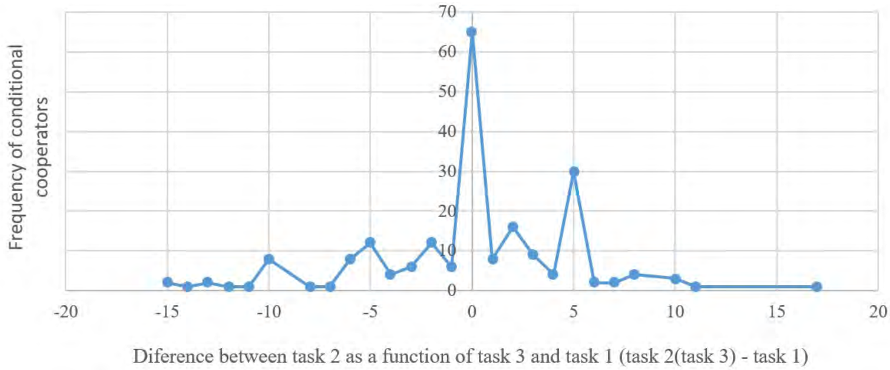


Figure 3.2 | Frequency of farmers matching their beliefs with their willingness to contribute to the public good.

To test for external consistency of the typology of conditional cooperation, we examine whether conditional cooperators will cooperate, according to the behavior of the other farmers in the community. We explore the aggregated behavior at the village level following the study by Rustagi et al. (2010). We look at the share of behavioral types and the outcome variable of cooperation in each village, estimated as the total share of farmers bringing coffee to the cooperative (Figure 3.3).[29]

We regress the share of farmers bringing coffee to the cooperative in each village (SP_j) on the share of conditional cooperators in each village (SCC_j). We control for relevant factors at the village level such as the share of female household heads, average elevation from farms in the village, average total area planted with coffee, and village sample size (X_j). The error term is denoted by ε_{ij} and the model is specified as follows:

$$SP_j = \alpha + \beta_1 SCC_j + X_i + \varepsilon_{ij}$$

Ordinary least squares (OLS) estimates are presented in Table 3.2. To ensure robustness, standard errors are clustered at village level (33 clusters), and we applied the wild bootstrap with 1000 repetitions (Cameron et al. 2008). The coefficient for

29 We present the frequency distribution of farmers' social preferences in Appendix 3.3.

the share of conditional cooperation is positive and not significant. Hence, we do not find evidence that the share of conditional cooperation in the village affects the participation of farmers in the local agricultural cooperatives. Our results are different from Rustagi et al. (2010), who find that groups with a larger share of conditional cooperators are more successful in forest management.

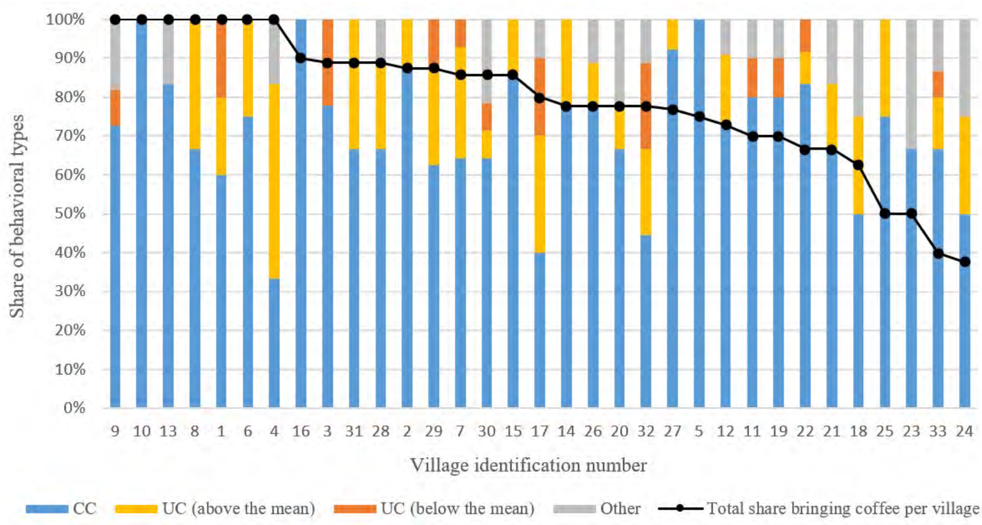


Figure 3.3 | Share of behavior types and village cooperation towards the cooperative.

Table 3.2 | Farmers' participation in local agricultural cooperatives and conditional cooperation.

	Dependent variable:		
	Share of farmers bringing coffee to local cooperative		
	(1)	(2)	(3)
Share of conditional cooperators (sample/ village)	**0.1418**	**0.1577**	**0.1577**
	[0.1885]	**[0.1978]**	**[0.2005]**
Constant	0.6921***	0.8436***	0.8436***
	[0.1489]	[0.2517]	[0.2475]
Other control variables	No	Yes	Yes
Wild bootstrap	No	No	Yes
Observations	33	33	33
R-squared	0.018	0.114	0.114

The independent variables are the average elevation in sample/village, the area planted with coffee in sample/village, and the number of farmers in sample/village. We apply the wild bootstrap following Cameron et al. (2008). Cluster standard error at the village level (33 villages). Robust standard errors in brackets, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Our hypothesis stated that farmers classified as conditional cooperators are more likely to bring coffee to the cooperative if the share of other farmers bringing coffee to the cooperative increases. Therefore, we add further analysis to specifically test this hypothesis and look at the interaction effect between the type of cooperator and the share of other farmers in the village bringing coffee to the local cooperative.

We regress as the dependent variable whether farmer *i* brings coffee to the cooperative in village *j* (P_{ij}), on the classification of pro-social behavior types: conditional cooperators (CC_{ij}), unconditional cooperators (UC_{ij}) and others (O_{ij}), the share of other farmers bringing coffee to the cooperative (PO_{ij}), and the complete interaction effects with the type of cooperator. We control for a set of household controls (X_i). The error term is denoted by ε_{ij}. To ensure robustness, standard errors are clustered at the village level (33 clusters), and we include fixed effects at the district level (6 districts) and apply the wild bootstrap with 1000 repetitions (Cameron et al. 2008). We set those classified as conditional cooperators as the reference group, and the model is specified as follows:

3

$$P_{ij} = \alpha + \beta_1 UC_i + \beta_2 O_i + \beta_3 PO_{ij} + \beta_4 (UC_i \times PO_{ij}) + \beta_5 (O_i \times PO_j) + X_i + \varepsilon_{ij}$$

Ordinary least squares (OLS) estimates are presented in Table 3.3. The coefficient for those classified as unconditional cooperators (UC_i) is negative but not significant, meaning that they are no different from those classified as conditional cooperators (CC_i). The same result is found for those in the category of others (O_i). The coefficient for the share of other farmers in the village bringing coffee to the cooperative (POij) is positive but not significant, indicating that the behavior of others has no significant effect on whether or not farmer *i* brings coffee to the cooperative, for the reference group of conditional cooperators. Furthermore, there are no significant differences when looking at the interaction effect with the unconditional cooperators (UC_i X PO_{ij}) and others (O_i X PO_{ij}). In summary, we do not find a significant impact of the share of other farmers bringing coffee on the participation of conditional cooperators, and this is no different for the unconditional cooperators.

Table 3.3 | Farmers' participation in local agricultural cooperatives, type of cooperator and interaction effect with the participation of other farmers in the village.

Base category: Conditional cooperator (CC_i)	Dependent variable: Dummy for farmers bringing any coffee to a cooperative (P_i)	
	(1)	(2)
Unconditional cooperator (UC_i)	-0.0959	-0.0959
	[0.4059]	[0.5357]
Others (O_i)	-0.5381	-0.5381
	[0.4607]	[0.7505]
Share of other farmers in the village bringing coffee to coop (PO_{ij})	0.1204	0.1204
	[0.3335]	[0.6033]
UC_i X PO_{ij}	0.1046	0.1046
	[0.4813]	[0.5843]
O_i X PO_{ij}	0.6865	0.6865
	[0.5622]	[0.8648]
Women	0.0730	0.0730
	[0.0579]	[0.0661]
Education (years)	-0.0031	-0.0031
	[0.0123]	[0.0142]
Household size	0.0118	0.0118
	[0.0209]	[0.0228]
Total farm area (ha)	-0.0036	-0.0036
	[0.0025]	[0.0032]
Farming experience (years)	0.0070***	0.0070***
	[0.0015]	[0.0000]
Total area planted with coffee (ha)	0.0074	0.0074
	[0.0053]	[0.0057]
Farm affected by the coffee leaf rust fungus	0.0703	0.0703
	[0.0714]	[0.0776]
Patience[1]	-0.0050	-0.0050
	[0.0090]	[0.0084]
Diversify with other crops	-0.0160	-0.0160
	[0.0482]	[0.0432]
Household received remittances	-0.1150	-0.1150
	[0.1402]	[0.1468]
Have a credit with the cooperative	0.2369***	0.2369***
	[0.0556]	[0.0000]
Constant	0.4091	0.4091
	[0.3401]	[0.5456]
District fixed effects	Yes	Yes
Wild bootstrap	No	Yes
Observations	293	293
R-squared	0.167	0.167

[1] A measure of patience from survey questionnaire, where 0 means very impatient and 10 means very patient. Fixed effects at the district level (6 districts). Wild bootstrap following Cameron et al. (2008). Cluster standard error at the village level (33 villages). Robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1.

To further understand cooperation towards the cooperative, we take a look at the control variables in the previous model. The coefficient for farming experience is positive and highly significant (p-value < 0.01). As reported in Table 3.1, farmers have on average a farming experience of 25.50 years; with an increase of one standard deviation (14.48), the probability of farmers bringing coffee to the local cooperative is predicted to increase by 11%. In further consultation with local cooperatives, it is clear that tradition is an essential factor explaining cooperation towards the cooperative (Murillo-Monge 2018). Farming experience, in this case, can work as a proxy for a farmer's tradition in the coffee business. Farmers with more farming experience are also the older farmers who traditionally have brought coffee to the local cooperative since its origins. An implication of this finding is that agricultural cooperative organizations should enhance policies to actively involve young coffee farmers in the cooperative organization.

The coefficient for having a credit with the cooperative is also positive and highly significant (p-value < 0.01). Farmers having a credit with the cooperative are 24% more likely to bring coffee, compared to the baseline probability of 40%. Agricultural cooperatives have grown strong over the years and many function as financial institutions that give credit to farmers. Our results suggest that cooperatives have the ability to engage farmers via credits to ensure part of their harvest as a payment mechanism.

3.7 Conclusions and discussion

In this paper, we test whether the typology of conditional cooperation, derived from a public goods game using the strategy method, is internally consistent, and whether the typology carries through to reality. In other words, if we find that a farmer is a conditional cooperator, what is the farmer's behavior in real life concerning supporting local agricultural cooperatives? If farmers are conditional cooperators, we expect they will match the behavior of others in real life.

Agricultural cooperatives need farmers to cooperate (e.g., bring coffee to the cooperative). Consequently, cooperatives should be concerned if there is a significant share of free riders (e.g., those who do not bring coffee to the cooperative). Free riders not only lower the total production process of the cooperative but also can bring down the contributions of the conditional cooperators in the community. Results from a public goods game using the strategy method show there is experimentally minimal pure free riding. However, the majority of farmers are conditional cooperators (72%), an important group that can enhance cooperation, but can also weaken the cooperative structure if farmers hesitate to bring coffee to the local cooperative.

We find a moderate correlation between conditional contribution as a function of individual beliefs about others in the experiment. The majority of conditional cooperators believe their contribution to the public good matches the contribution of others in the experiment. However, we do not find a significant interaction effect between those classified as conditional cooperators in the experiment and the share of other farmers bringing coffee to the cooperative in real life.

In other words, we find the typology of conditional cooperator to be consistent within the experiment, but it does not carry over outside the laboratory into a real-life setting. The lack of correlation suggests that social preferences can be related to a particular context or setting. Our results are in line with other types of social preferences measures in the lab that do not relate to pro-social preferences measured in real life (Laury and Taylor 2008; Voors et al. 2011, 2012). Similar results have also been found in the case of risk preferences in Chapter 2. Hence, one should be careful when extrapolating the typology of conditional cooperation measures in the lab to other real-life contexts.

Given our results, a question that arises is whether farmers see the action of bringing coffee to a local cooperative as a public good. We have no doubt that the agricultural cooperative has been an essential institution for development in the rural areas of Costa Rica, delivering benefits to households regardless of whether they are coffee farmers. However, cooperatives are also known for not excluding yields on the basis of quality. Therefore, there are cases where the only place that accepts the coffee is a local cooperative. As a consequence, there can be any typology (i.e., conditional, unconditional cooperators, altruists, and free riders) producing low-quality coffee and the only place they can deliver coffee is the local cooperative. Furthermore, in the last years, there has been a growing establishment of micro mills and farmers trying to differentiate themselves from the crowd by processing and marketing their coffee. These are details that together make it difficult to consider our real-life scenario a perfect public goods game with which to look for external validity.

3.8 Appendices

Appendix 3.1 | Final liquidation prices. Harvest 2013-2014.

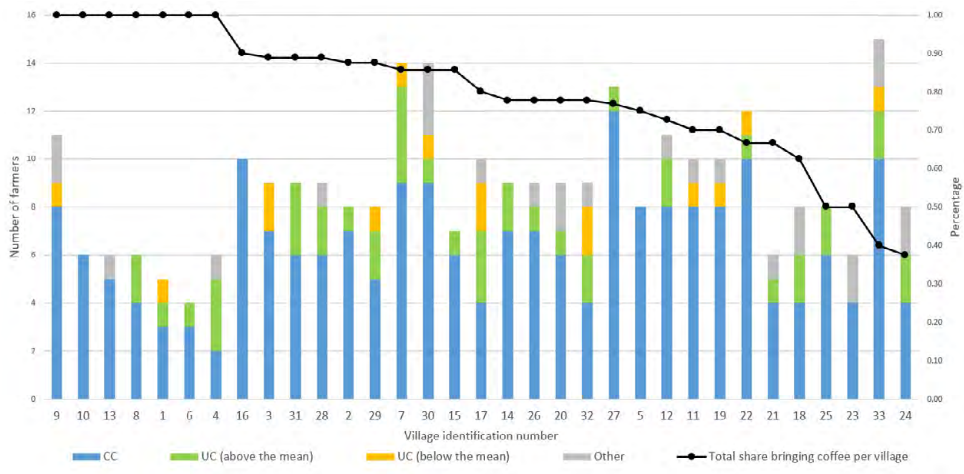
Mills reported in Tarrazú and Brunca Region		Ripe	Green
Cooperatives	Coope Dota R.L.	87,437.99	60,717.44
	Coope Llano Bonito R.L.	78,077.12	
	Coope Tarrazú R.L.	**69,646.40**	48,339.90
	Coope Agri El General R. L.	60,567.93	41,394.65
Private Mills	Beneficio la Candelilla de Tarrazú S.A.	101,915.99	
	Beneficio Volcafé (C.R.) S.A. San Diego	64,309.71	44,681.29
	Benefocio Volcafé (El General)	42,660.80	29,230.12
	F.J. Orlich & Hnos LTDA. (El Marqués)	52,444.58	36,260.78

Notes: Prices in Costa Rican Colones (CRC). The final price is published before the harvest season and includes a 9% from the final liquidation price in favor of the coffee mill for the processing and marketing of coffee (MAG 2016) in accordance with the provisions of Law 2762 of June 21, 1961 on the regime of relations between producers, mills and coffee exporters. The Costa Rican Institute of Coffee (ICAFE) communicates to the interested parties the final liquidation prices of the coffee delivered to each mill. Source: (ICAFE 2014).

Appendix 3.2 | Payments distributed through the year in Coope Tarrazú.

Harvest period	Advancement	Adjustment		Official liquidation price	Final liquidation price
		Amount in colones	Date of payment		
2012-2013	58,000	3,000	July		
		3,000	August		
		3,000	September		
		3,000	November		
		500	December	62,983.00	70,500.00
2013-2014		5000	September		
		5000	October		
		160	December	**69,646.40**	75,160.00

Source: CoopeTarrazú (2018) Note: In addition, the associate contributes 2% per fanega (coffee measurement unit that is approx. 250 kg) as a contribution to the capital of the cooperative. The amount collected over the years is returned when the associate retires or is delivered to the family when she/he dies.



Appendix 3.3 | Distribution of farmers' social preferences per village and participation in agricultural cooperatives.

Appendix 3.4 | Public Goods Game Instructions.
In this game we are going to talk about "points." At the end of the game, the total amount of points you get will be converted to colones. Each point you get is equivalent to 200 colones.

1 point = 200 colones

You will play considering the decisions of two other peer farmers in the community, but you do not know who they are. Only the coordinators of the game know who they are and they are chosen at random.
We are going to read the instructions together and present some examples. Please consider the numbers in the example as an illustration. The points you will obtain from the game will be different. At the end of the instructions, we are going to ask you a few questions that will help you to understand the game.

BASIC DECISION
You are a member of a group of three farmers. Each of you has to decide how to divide 20 points (20 points = 4000 colones) into two different accounts. You can put these 20 points in your private account, or you can invest them fully or partially into a project. Each point you do not invest in the project will automatically be transferred to your private account.

INCOME FROM THE PRIVATE ACCOUNT
For each point you put in your private account, you will earn exactly one point. For example, if you put twenty points on your private account (which implies that you do not invest anything in the project), you will earn exactly twenty points from the private account. If you put 6 points into the private account, you will receive an income of 6 points from the private account. Nobody except you earns something from your private account.

INCOME FROM THE PROJECT ACCOUNT
For each point you deposit into the project account, all members of the group receive the same income. That is, you will also earn income for the points that the other two farmers deposit into the project account. However, you do not know how many points that will be. The coordinators will collect the information from the other farmers and will notify me of the result when making payments at the end of the game.
For each member of the group, the income from the project account will be determined as the sum of the contributions of the three farmers multiplied by 0.5.

Project account income = the sum of the contributions of the 3 x 0.5
For example, if the sum of the three contributions to the project account is 60 points, you and the other people in the group will receive → 60 x 0.5 = 30
Another example, if the three farmers in the group deposit a total of 10 points, then you and all the others receive → 10 x 0.5 = 5

YOUR TOTAL INCOME

Your total income is the sum of your income from the private account and your income from the project account.

		20 points from your initial endowment
Income from the private account	→	–
		Your contribution to the project
		+
Income from the project account	→	*Sum of all contributions to the project x 0.5*
		Total income

3

CONTROL QUESTIONS
Of the 20 points available, suppose that no one, including you, puts points in the project account. Then…

- What is your total income? **R** / 20 points from the personal account
- What is the income of the other people in your group? **R** / Equal

Of the 20 points available, let's assume that everyone, including you, puts all the points in the project account. Then…

- What is your total income? **R** / 20 + 20 + 20 = 60 x 0.5 = 30
- What is the income of the other three people in your group? **R** / Equal

(Check farmers' responses to the control questions. If they do not give a correct answer, repeat the explanation from the basic decision.)

These are the extreme cases (all or nothing), but you and the other farmers in the group can decide how you want to distribute the points in any way.

Remember that what you leave in your personal account is yours. What you put into the project account will be returned to you according to the sum of the contributions of each farmer in your group.

UNCONDITIONAL CONTRIBUTION *(Show decision sheet 1)*
In the unconditional contribution, you must decide how many of the points you deposit into the project account. You must write a whole number that cannot be less than zero or greater than the 20 points you have to distribute. The rest of the points will go to your personal account.

CONDITIONAL CONTRIBUTION *(Show decision sheet 2)*
Your second task is to complete the table of contributions. You should indicate for each possible average contribution of the other two people in the group the number of points that you want to put in the project account.
The average contribution is the sum of the contributions of the other two producers divided by two.

- Example 1: the other two farmers contribute (20 + 20 = 40 ÷ 2 = 20)
- Example 2: the other two farmers contribute (5 + 5 = 10 ÷ 2 = 5)
- Example 3: the other two farmers contribute (5 + 20 = 25 ÷ 2 = 12.5 = 13). The nearest whole number would be used.

You have to write in the right column how many points you want to contribute to the project account given that the other farmers contribute on average (approximate) the number of points in the left column. That is, you will decide how much you want to contribute depending on what other people contribute.

After all the farmers have made their unconditional contribution and completed the table of contributions, one person from each group will be selected, and their income will be determined according to the table of contributions. For the other two farmers in the group, the income will be determined by the unconditional contribution. I will tell you if you were selected at the end of the games.

CONTRIBUTION OF OTHERS *(Show decision sheet 3)*
Now you will tell us how much you think the other two farmers of your group have written as their unconditional contributions. In other words, what number do you suspect they wrote on average?

If you correctly guess the average contribution of the other two farmers to the project account, you can earn additional points.

If the true unconditional contribution of others is equal to what you guessed, you will earn four extra points. If the real contribution is a point above or below, you earn three extra points. If the contribution is two points above or below, you earn one extra point.

Mark only one box with an X (single selection).

Appendix 3.5 | Decision sheet for unconditional contribution and final payment.

Unconditional contribution

***How many of the 20 tokens do you want to invest in the project?* ______**

(Number between 0 – 20)

Complete after receiving the information of the donations of the other members of the group: 1 point = 200 colones.

		Points			
Income from private account	→	________			
		+			
Income from the project account	→	________			
		+			
Income from guessing the contributions from the other farmers	→	________			
Total income	→	________	*x 200*	= ¢	________

Appendix 3.6 | Decision sheet for conditional contribution.

Conditional contribution

Average contribution (approximate) of the other two farmers to the project account	Your contribution to the project account is:
0	
1	
2	
3	
4	
5	
6	
7	
8	
9	
10	
11	
12	
13	
14	
15	
16	
17	
18	
19	
20	

Appendix 3.7 | Decision sheet for guessed contribution.

Guessing contributions

Average contribution (approximate) of the other two farmers to the project account	Mark with an X the box that you think corresponds to what the other two farmers contributed unconditionally to the project account. (JUST CHECK ONE BOX)
0	
1	
2	
3	
4	
5	
6	
7	
8	
9	
10	
11	
12	
13	
14	
15	
16	
17	
18	
19	
20	

4

Credit, insurance and farmers' liability

Evidence from a lab in the field experiment with coffee farmers in Costa Rica

Abstract

This paper examines the effect of farmers' liability on demand for credit with and without insurance. We test predictions of a theoretical model in a lab in the field experiment with coffee farmers in Costa Rica. Farmers choose how much to invest in six different settings, described on the one hand by whether the loan is insured or not, and on the other by their liability. Our results show that the uptake of loans bundled with insurance is significantly higher than uptake of loans without insurance, both when farmers are liable for sure for their debt, and interestingly when there is uncertainty about their liability. When farmers are not liable for their debt, i.e. under limited liability, the uptake of credit is high irrespective of whether the loans are insured or not. Our results suggest that in order to increase the uptake of insurance as a strategy to increase private investment and reduce the vulnerability of farmers to shocks, it is important that farmers are liable with at least some probability. In terms of policy design, our results show that the "principle" of limited liability does not have be abandoned altogether in order to generate an increase in the uptake of insured credit.

This chapter is based on:

Naranjo, M.A., Pieters, J. and Alpizar, F. "Credit, insurance and farmers' liability: evidence from a lab in the field experiment with coffee farmers in Costa Rica." *Environment for Development Discussion Paper Series*. March 2017. EfD DP 17-04. Submitted to the Journal of Economic Behavior and Organization.

4.1 Introduction

To cope with losses from extreme hydro-meteorological events, governments typically implement disaster relief programs and offer debt relief to affected parties (The World Bank 2007). For example, agricultural banks in developing countries frequently cooperate with poor agricultural borrowers after they experience a significant loss, restructuring their loans, and sometimes outright canceling outstanding debts (Carter et al. 2007). However, governments in general and in developing countries, in particular, have a limited capacity to help. Moreover, in the context of climate change, debt relief practices are becoming less viable because the risk is systemic and losses can easily surpass most governments' debt relief budgets. An example of systemic risk is exposure to increasingly frequent flood and droughts events, as predicted under climate change scenarios for Central America[30]. Insurance is potentially an alternative, but in developing countries insurance markets are thin (Carter et al. 2014).

Rural households in developing countries face a number of credit constraints and market imperfections that shape investment decisions (Karlan et al. 2014). In the absence of insurance markets, "risk rationing," as explained by Boucher et al. (2008), suggests that the borrower voluntarily withdraws from taking a loan, due to the risk of losing collateral (Giné and Yang 2009). Traditional formal insurance instruments can be used to manage risks, but such insurance services are basically non-existent in rural areas of developing countries (Carter et al. 2014). This lack of insurance markets might aggravate the effect of risk rationing on credit uptake (Boucher et al. 2008; Giné and Yang 2009). The combination of credit and crop insurance, therefore, could be applied as a mechanism to improve credit markets and encourage investment in the agricultural sector (Carter et al. 2014), but limited empirical research has been conducted (Marr et al. 2016).

A number of previous studies have focused on the combined effects of credit and insurance on investment, and the effects of insurance on credit demand and vice versa.[31] The evidence is mixed. When combining credit and insurance, some studies find credit with insurance increases investment (i.e., fertilizer purchase) (Hill and Viceisza, 2012), while others find that mandatory insurance actually reduces the demand for credit (Giné and Yang, 2009) or has no effect on investment and adoption of new technologies (Brick and Visser, 2014). Finally, Karlan et al. (2014) state that crop insurance alone increases farm investment, but when insurance is bundled with credit, it does not necessarily increase investment. Giné and Yang (2009) suggest that the bundling of insurance and credit may reduce investment due to farmers' limited

30 Systemic and highly covariate weather risks can be insured with the appropriate reinsurance. See Carter (2013) for a review on insurance partnerships for agricultural insurance markets.

31 See Marr et al. (2016) for a review of the most recent literature on index insurance and bundling insurance with credit.

liability (i.e. certainty of debt relief in case of default). The idea is intuitive, namely that limited liability provides implicit insurance; thus, when an insurance premium must be paid, this results in lower demand for loans.

Although in theory, limited liability plays an important role in the links between credit and insurance, empirical evidence on how farmers' individual liability affects the uptake of insured credit is scarce. Some studies are focusing on joint liability and credit, showing that joint liability promotes screening, monitoring, enforcement of repayment (Chowdhury 2005; Ghatak and Guinnane 1999)[32] and reduces moral hazard among borrowers (Flatnes and Carter 2015).

Our objective is to examine the effect of farmers' liability on the uptake of credit with and without mandatory insurance, using a lab-in-the-field experiment. We believe this is the first empirical study to address this question. We follow the theoretical model of Giné and Yang (2009), and conduct an experiment with coffee farmers in Costa Rica. Each farmer chooses how much to borrow in order to invest in his farm. Credit is offered either with or without mandatory insurance with a premium cost, under three types of liability scenarios. Under these scenarios, farmers have limited liability, or a 50% or 100% probability of full liability (i.e. the farmer is liable for sure). A laboratory approach allows us to isolate the impact of limited liability on the demand for loans with and without mandatory insurance. To abstract from other factors that are likely determinants of insurance uptake, our design involves an actuarially fairly priced insurance, with pay-out triggered by weather realization and without any basis risk.

Our results show uptake of credit with insurance is significantly higher than without insurance when farmers are liable for sure, but also when there is a 50% probability of full liability. Insurance has no effect on credit demand if farmers have limited liability (through the guarantee of public debt relief in case of bad weather). That is, we find no evidence that mandatory insurance reduces credit demand in case of limited liability, but we do see a positive effect on the demand for insured credit when farmers are liable, even if only in probabilistic terms. Frequently authorities fear that making farmers fully liable for their debt might put them in a disadvantageous situation in case of extreme events. Our results show that limited liability should not be abandoned altogether in order to generate an increase the uptake of insured credit. Introducing uncertainty on the promise of limited liability is enough to increase insured investment, thereby reducing the vulnerability of farmers to shocks. Moreover, providing limited liability only in probabilistic terms should ease the burden on the authorities should there be a systemic shock to farmers.

32 Joint liability is when borrowers receive individual loans but form a group in which all members are mutually responsible for the total repayment to the lender (Flatnes and Carter 2015).

The rest of this paper is organized as follows. The second section describes the literature on credit constraints, credit combined with insurance, and the role of limited liability; the third section presents a model on credit, investment and insurance, and develops our hypotheses; section four describes our experimental design and implementation procedures; section five presents the results; and the last section concludes the paper.

4.2 Literature review

In this section, we briefly review the relevant literature on credit market imperfections. We then discuss previous evidence on bundling credit with mandatory insurance and the effects on farm investment. Finally, we reflect on the role of limited liability.

In the absence of insurance markets, "risk rationing," as explained by Boucher et al. (2008), suggests that the borrower voluntarily withdraws from taking a loan, due to the risk of losing collateral (Giné and Yang 2009). Traditional formal insurance instruments can be used to manage risks, but such insurance services are basically non-existent in rural areas of developing countries (Carter et al. 2014). This lack of insurance markets might aggravate the effect of risk rationing on credit uptake (Boucher et al. 2008; Giné and Yang 2009). Hence, the combination of credit and crop insurance, therefore, could be applied as a mechanism to improve credit markets and encourage investment in the agricultural sector (Carter et al. 2014).

Some studies focus on bundling credit with mandatory insurance and the effects on risk rationality and farm investment. Regarding risk rationality, Cheng (2014) studies the effects of index insurance on risk rationed households in China. In his experiment, providing insurance to risk rationed farmers induced more than half of the farmers to apply for credit, with approximately two-thirds using the loan for productive investment rather than for consumption. Regarding farm investment, Carter et al. (2016) formally model and analyze the conditions under which index-based crop insurance can be most effective. They show that insurance will have no impact on investment and technology adoption when risk is low and the loan is covered by limited liability contracts. Then the impact of the insurance will depend strongly on the collateral requirements by the lender. Under low collateral requirements, bundling credit and insurance will foremost benefit the lenders by bringing stability to the loan portfolio. In high collateral situations, even stand-alone index insurance can considerably increase the adoption of new technologies through credit when the risk is covered by a well-designed index contract (Carter et al. 2016).

In an experimental study on the importance of capital constraints and uninsured risk, Karlan et al. (2014) examine if financial market imperfections discourage investment by smallholder farmers. They applied a randomized controlled trial with cash grants, rainfall insurance grants, and rainfall insurance sales in northern Ghana. They find strong responses of agricultural investment to the rainfall insurance grant, but relatively small effects of the cash grants. Hence, uninsured risk limits farmer investment, while farmers with insurance grants manage to find resources to increase investment on their farms. This clearly suggests that agricultural credit market policy alone is not sufficient to increase investment in the agricultural sector.

Brick and Visser (2014) use a lab in the field experiment in South Africa to examine whether the provision of index insurance induces farmers to opt for riskier activities. They find that providing a loan with insurance does not increase investment in new technologies. Furthermore, risk-averse farmers are more likely to opt for traditional seeds than for high-yield seeds, regardless of the presence of insurance. Their experimental design reflects the reality of an index insurance product that minimizes the risk of rainfall variability, but the design does not account for other risk factors (i.e., basis risk) that might have affected their results given the high degree of risk aversion in their sample.

Giné and Yang (2009) implemented a field experiment in Malawi to examine whether production risk suppresses the demand for credit. They offered credit to purchase high-yielding seeds to a control group of farmers and credit bundled with index insurance (at actuarially fair price) to a treatment group. Their results show that take-up is lower when credit is bundled with insurance. They argue, and show theoretically, that limited liability provides enough implicit insurance, so farmers will prefer loans without mandatory insurance, which are less costly.

To summarize, existing experimental and theoretical evidence is mixed. On the one hand, providing crop insurance increases farm investments (Elabed and Carter 2014; Karlan et al. 2014; Hill and Viceisza 2012). On the other hand, when credit and insurance are combined, investment does not necessarily increase (Brick and Visser 2015; Karlan et al. 2014) and may even decline (Giné and Yang 2009).

We now turn to a more extensive review of the role of limited liability. When production is low, farmers may be forced to default to maintain a subsistence level of consumption (Miranda and Gonzalez-Vega 2011). Default can occur involuntarily when associated with shocks or other risks that make borrowers unable to repay, but can be voluntary when lack of contract enforcement incentivizes borrowers to default even when they have the means to repay their loans (Ghosh et al. 2000). When contracts are subject to limited liability, borrowers are not forced to repay the bank if returns on investment are less than loan repayment obligations (Ghosh et al. 2000).

Agricultural banks and governments in developing countries often cooperate with poor agricultural borrowers to deal with losses from extreme events, by restructuring loans and through debt relief programs (Carter et al. 2007). This affects farmers' liability, even though governmental assistance is not guaranteed (Carter et al. 2007; Miranda and Gonzalez-Vega 2011). After the strong effects of "El Niño" 1998 in Peru, for example, a government decree forced lenders to reschedule, meaning that farmers in default could pay later. Lenders believed these public sector interventions damaged the credit culture that had been formed in previous years (Trivelli et al. 2006). In Costa Rica, the government applied debt relief six times between 2004 and 2012, to assist borrowers who had received credit from development banks and were struggling to repay their loans (Gutierrez-Vargas 2015).

Empirical evidence on the effect of farmers' liability on uptake of credit combined with insurance is scarce. There is some evidence that farmers' belies about availability of disaster relief is associated with less participation in insurance programs. A study by van Asseldonk et al. (2002) explores the role of producers' belies in disaster relief in the Netherlands. Farmers' willingness to pay to participate in a hypothetical insurance program is negatively and significantly associated with the producer's belief that disaster relief will be available in the future. In addition, a recent study by Deryugina and Kirwan (2016) hypothesizes a similar pattern by estimating whether the Samaritan's dilemma exists in U.S. agriculture.[33] They instrument for disaster payments using political variation at county level and then estimate how expectations of receiving these payments affect farmers' decisions. They find that bailout expectations reduce crop insurance coverage by reducing expenditures on premiums and inducing farmers to choose less generous insurance plans. At the same time, farmers also reduce farm labor and fertilizer use.

Giné and Yang (2009) explicitly focus on the bundling of credit and insurance and refer to the existence of limited liability as a possible explanation for lower credit demand when credit is bundled with insurance. They show theoretically that a loan contract with limited liability provides enough implicit insurance, and therefore credit demand is predicted to decline with mandatory insurance that increases the price of credit.

The next section presents their theory in more detail.

33 First described by Buchanan (1975), the Samaritan's dilemma explain how individuals who expect to be bailed out in times of crisis (e.g., natural disasters and financial crises) take on additional risk in response (Deryugina and Kirwan 2016).

4.3 Theoretical model

This section describes the theoretical model for credit demand and insurance, building on the model developed by Giné and Yang (2009). We start with the general model setup and then illustrate the simple case of loans without insurance, followed by the case of loans with mandatory insurance. Finally, we introduce differences in farmers' liability and discuss the hypotheses.

4.3.1 General model setup

To analyze farmers' demand for credit, we consider a risk-averse farmer who is offered credit under two types of contract (with and without mandatory weather insurance) and three types of liability (limited liability, uncertainty about liability, or full liability for farmers). Farmers use the credit to invest in their agricultural production. Farm output depends on the level of investment, the return on investment, and the state of the weather. We define $\boldsymbol{p}$ and (1-$\boldsymbol{p}$) as the probability of good (bad) weather. Following Giné and Yang (2009), we assume perfect correlation of investment returns and state of the weather, so that investment returns depend solely on the realization of the weather with a probability $\boldsymbol{p} = ½$.

Without investment, farmers can realize a base output level $\boldsymbol{Y_B}$ in case of bad weather or $\boldsymbol{Y_B} + \boldsymbol{a}$ in case of good weather[34], while investment will increase output to the level $\boldsymbol{Y_H}$ in case of good weather and reduce output to the level $\boldsymbol{Y_L}$ in case of bad weather.[35] We assume that expected output is higher when the farmer invests than without investment, so that

$$\boldsymbol{p}(\boldsymbol{Y_B}+\boldsymbol{a})+(1-\boldsymbol{p})\boldsymbol{Y_B} < \boldsymbol{p}\ \boldsymbol{Y_H}+(1-\boldsymbol{p})\boldsymbol{Y_L}$$

Output with investment, $\boldsymbol{Y_H}$ or $\boldsymbol{Y_L}$, depends on the amount invested, which is equal to the loan size $\boldsymbol{C}$. In case the weather is good, investment gives the farmer a positive return $\boldsymbol{r}$, so that $\boldsymbol{Y_H} = \boldsymbol{Y_b} + \boldsymbol{a} + \boldsymbol{rC}$. In case of bad weather, the return is negative $\boldsymbol{r}$, so that $\boldsymbol{Y_L} = \boldsymbol{Y_b} - \boldsymbol{rC}$.

To invest, the farmer needs to borrow from a bank. We define $\boldsymbol{i}$ as the interest rate, $\boldsymbol{W}$ as the value of famers' assets required as collateral for a loan of any given size, and $\boldsymbol{R}$ as the repayment of the loan, consisting of the amount borrowed and the interest due. We assume that the value of the collateral is enough to cover the repayment of the loan: $\boldsymbol{W} > (1+\boldsymbol{i})\boldsymbol{C} = \boldsymbol{R}$, and that output in the low state is not sufficient to repay

34 In the original model by Giné and Yang (2009), the base output (i.e. from traditional seeds) does not change with the probabilities, so that $Y_B < p\ Y_H + (1-p)Y_L$.

35 We give farmers the example of investing in new coffee trees (see Appendix 4.2), when indeed a bad weather shock can lead to negative returns on investment.

the bank **(Y_L ‹ R)**. The lender can always seize up to the full value of farm output $\boldsymbol{Y_L}$ or $\boldsymbol{Y_H}$ in order to secure repayment of the loan, but only seizes other assets $\boldsymbol{W}$ with a probability ϕ. The three scenarios we analyze are limited liability ($\phi = 0$), uncertain liability ($\phi = 1/2$), and full liability ($\phi = 1$).

4.3.2 Credit without insurance

First, consider the case when credit is offered without insurance and farmers decide whether to borrow and invest amount $\boldsymbol{C}$. When the farmer chooses not to invest, expected utility is defined as

$$U_B = \frac{1}{2}u\left(Y_B + a + W\right) + \frac{1}{2}u\left(Y_B + W\right) \tag{1}$$

When the farmer chooses to invest, output can be high or low, depending on the weather. Consumption in the high output state is $\boldsymbol{c_H} = \boldsymbol{Y_H} - \boldsymbol{R} + \boldsymbol{W}$. In the low output state, consumption depends on whether the bank seizes (part of) the collateral to recover repayment, which it does with probability ϕ. Hence, expected utility with investment in the case of credit without insurance is given by:

4

$$U_U = \frac{1}{2}u\left(Y_H - R + W\right) + \frac{1}{2}\left[\phi u\left(Y_L - R + W\right) + \left(1 - \phi\right)u\left(W\right)\right] \tag{2}$$

4.3.3 Credit with mandatory insurance

Second, consider the case when credit is offered only in combination with weather insurance provided by the bank. The insurance premium π is set at an actuarially fair price (following Giné and Yang 2009), so that, in order to invest level $\boldsymbol{C}$, farmers need to borrow an amount $\boldsymbol{C} + \pi$. The total repayment to the bank for a loan with insurance is therefore . In states of bad weather, the insurance pays out the total amount $\boldsymbol{R^I} = (1 + \boldsymbol{i})(\boldsymbol{C} + \pi)$. Given thectuarially frly priced insurance, the premium can be written as a function of repayment whout insurance (as in Giné and Yang, 2009), which gives $\boldsymbol{R^I} = \frac{\boldsymbol{R}}{\mathbf{p}} = 2\boldsymbol{R}$. (see Appendix.4). Hence, expected utility of investment when credit is combined with insurance is:

$$U_I = \frac{1}{2}u\left(Y_H - 2R + W\right) + \frac{1}{2}u\left(Y_L + W\right) \tag{3}$$

4.3.4 Differences ifarmers' liability

We evae three different liability scenarios: limited liability ($\phi = 0$), uncertain liability ($\phi = 1/2$), and full liability ($\phi = 1$). In general, credit demand with actuarially fairly priced insurance dends on the level of output in case of bad weather, $\boldsymbol{Y_L}$, and on farmers' risk averon. In the next section, we use a constant relative risk aversion utility function (CRRA)[36] and show the predictions of the theoretical model under the distinct features of our experimental design.

tuitively, when farmers have limited liability and income in the low state is lower than repayment wh insurance $\boldsymbol{Y_L} < \boldsymbol{R}$, loans without insurance should provide sufficient implicit insurance. Thus, demand for uninsured credit should be higher than demand for insured credit. Then, when farmers are uncertain about their liability or are liable for sure, low values of $\boldsymbol{Y_L}$ and a contract without insurance still provide implicit insurance and thus higher expected utility for uninsured loans. However, when $\boldsymbol{Y_L}$ increases, farmers' default costs also increase and expected utility is higher for loans with insurance (Giné and Yang 2009, p4).

4.4 Experimental design and implementation

To test our hypotheses in a controlled environment, we implemented a lab in the field experiment with coffee farmers in Costa Rica. The experiment is set up as a within-subject design in which each farmer faces six different treatments. In each treatment, the farmer chooses how much to borrow for investment in her farm, while facing ex-ante uncertainty about the weather, which can be good or bad. In the treatments, credit is offered either with or without mandatory insurance, and with farmers having limited liability ($\phi = 0$), uncertainty about their liability ($\phi = 1/2$), or full liability ($\phi = 1$). We explain that farmers' liability is the result of whether or not there will be debt relief by the government in case of bad weather. Each treatment is presented as a one-period decision-making game, independent from the other treatments.

The experimental design is developed in line with the previous model, in which good and bad weather occur with equal probability ($p = 1/2$). We determined our experimental parameters with a CRRA risk aversion parameter (σ) of ½ in mind but assess the model's predictions across all levels of risk aversion. Base output (without any investment) is $Y_b = 2$ in case of bad weather, while good weather will result in additional output over base output equal to $a = 1$ (Hill and Viceisza 2012). Farmers can choose to invest zero, one or two units of capital C. If the weather is good,

36 Constant relative risk aversion utility function: $u(c) = \frac{c^{1-\sigma}}{1-\sigma}; 0 < \sigma < 1; \sigma = 0$ indicates risk neutrality and $\sigma > 0$ indices risk aversion.

investment gives the farmer a positive return over the capital *(r = 5)*: $Y_H = Y_b + a + rC$. In case of bad weather, the return is negative *(r = -1)* and*:* $Y_L = Y_b - C$.

In each of the six treatments, farmers are given an endowment (W = 3) that can serve as collateral. This endowment is sufficient to guarantee the maximum uninsured repayment amount (W > R), with the interest rate fixed at $i = 0.10$ throughout the experiment. Farmers are told that their asset endowment can be seen as farmland, housing, or other properties that the lender can take in case of default. Farmers' consumption will depend on the amount invested C, the weher draw, and whether or not their collateral is seized by the bank. One unit of income or csumption in the experiment is set equal to 1,000 Costa Rican Colones (CRC).[37]

Figure 4.1 shows expected utility without credit (zero investment) and with maximum investment ($C=2$), with or without insurance, for different risk aversion parameter values and while lng the expected returns constant. As Figure 4.1 shows, for low levels of risk aversion, the expected utility associated with maximum investment is always higher than the expected utility without investment, whether or not credit comes with insurance. Yet when comparing insured and uninsured credit, it is clear that farmers' liability and risk aversion determine which type of credit is preferred.

In case of limited liability, uninsured credit provides higher expected utility than insured credit. With uncertain liability, insured credit provides higher expected utility for relatively risk-averse farmers. Finally, with full liability, insured credit is always the best option regardless of the risk aversion parameter.

Farmers in our experiment choose their level of credit under each type of loan (insured or uninsured) and liability scenario, rather than choosing between an insured and uninsured loan. Hence, our theoretical model predicts that, when farmers have limited liability, credit demand should be higher if credit is not bundled with insurance than if credit is bundled with insurance, unless farmers are very risk-averse in which case the difference becomes small. When farmers are uncertain about their liability and have relatively low risk aversion, credit demand should also be higher if credit is not bundled with insurance. With uncertain liability and high levels of risk aversion, credit demand should be higher if credit is bundled with insurance. When farmers are liable for sure, credit demand should be higher if credit is bundled with insurance.

Each of the six treatments was repeated three times to be able to perform several robustness tests. We explain that rounds are independent from each other, and that one round will be randomly selected for payment at the end of the experiment. The draw of the round for payment and weather is determined in private for each farmer. Selection of the payment round was done by taking one chip out of a bag with 18

37 1000 CRC equals approximately two US dollars.

chips numbered 1-18, while the weather draw was determined with the toss of a coin. Final payment consisted of a show-up fee of 2000 CRC plus the level of consumption the farmer reached in the selected round. Detailed instructions are included in the complete experimental protocol in Appendix 4.2.

Farmers participating in the experiment were selected from two coffee regions, Brunca and Tarrazu, using stratified random sampling according to the density of coffee plots. Regions were selected to capture the variation in altitude and effects of a coffee rust epidemic in 2012-13; all farmers were surveyed in 2014 as part of a different study (Chapter 2). We contacted all surveyed farmers and conducted thirteen experimental sessions at local primary schools during the second and third week of October 2015.[38] Sessions were organized one per day during the afternoon, with on average 10 farmers per session, who were assigned randomly to individual desks around the classroom. The order of treatments was selected randomly in the first two sessions, repeated in subsequent sessions, and alternating between the first and second order.

In total, 134 (46% of the 2014 survey participants) farmers participated in the experiment. Two farmers had incomplete responses for the experiment; these are excluded from the analyses. Table 4.1 presents farmers' characteristics for the survey and experimental participants. Differences in means (t-test) show no differences between the two groups for most of the variables, except that farmers participating in the experimental sessions have on average a smaller total area planted with coffee. We show in the next section that this has no effect on the outcomes of the experiment. Hence, our results can be seen as representative for the two coffee regions.

Table 4.1 | Variables and sample means for survey and experimental sample.

	Survey 2014		Experiment 2015		t-test	
	Mean	***sd***	***Mean***	***sd***	***Difference***	***p-value***
Age (years)	51.76	13.62	51.52	13.04	0.25	0.828
Women	0.10	0.30	0.11	0.31	-0.01	0.783
Education (years)	5.79	2.61	5.79	2.78	0.00	0.993
Region (% from Brunca)	0.47	0.50	0.39	0.49	0.08*	0.053
% income from coffee	56.94	36.23	57.83	35.63	-0.89	0.780
Total coffee area (ha)	3.48	4.61	2.53	2.45	0.96***	0.000
Affected by leaf rust	0.81	0.39	0.81	0.39	0.00	0.975
Observations	294		132			

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

38 Farmers were offered two possible dates to attend a workshop session at two nearby villages.

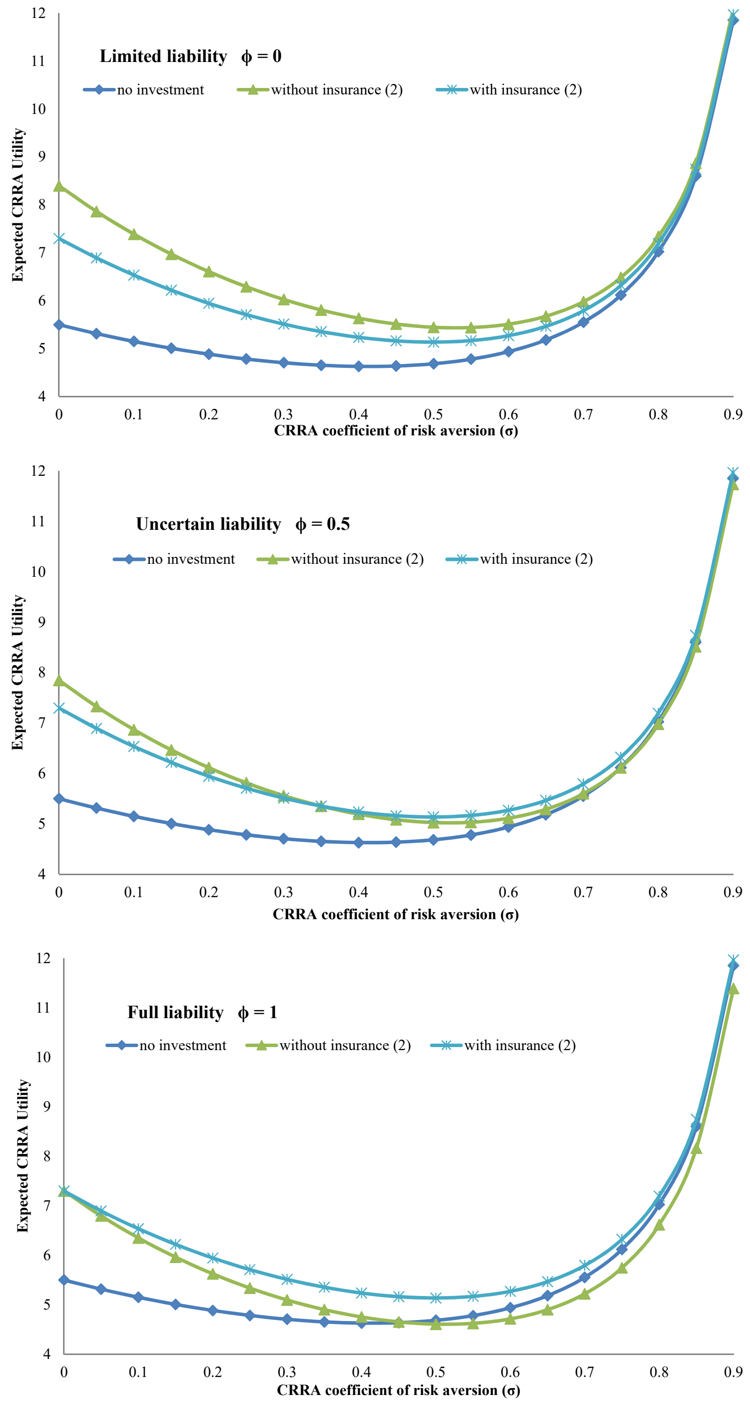


Figure 4.1 | Expected CRRA utility varying the risk aversion parameter.

4.5 Empirical strategy and results

To analyze the effect of farmers' liability on demand for credit with and without insurance, our main dependent variable is the average amount borrowed across the three repeated rounds within each treatment. Figure 4.2 presents the distribution of farmers' credit demand across the experimental treatments. Credit demand varies considerably across treatments. Farmers are more likely to demand the highest level of credit (2000 CRC) when governmental debt relief ensures limited liability for the two types of loans, with insurance (52%) and without insurance (58%).

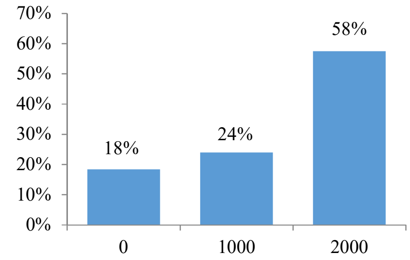
Comparing Figures 4.2a and 4.2b, there appears to be little impact of mandatory insurance on farmers' credit demand when farmers are not liable, in line with theoretical predictions at high levels of risk aversion (see Figure 4.1). Compared to limited liability, credit demand is lower when farmers are uncertain about their liability and especially when they are liable for sure. Comparing Figures 4.2c and 4.2d, as well as Figures 4.2e and 4.2f, we see that, with uncertain or full liability, mandatory insurance increases demand for credit. Again, this is in line with predictions from the model, and suggests the farmers in our sample have intermediate to high levels of risk aversion.

Table 4.2 and Figure 4.3 explores differences in means between treatments using a paired t-test. Comparing means across the rows of Table 4.2 again shows that liability decreases total credit demand. We also confirm that uptake of loans with insurance is significantly higher than without insurance when farmers are liable or when there is uncertainty about their liability. We find no significant differences between demand for loans with and without insurance in case of limited liability.

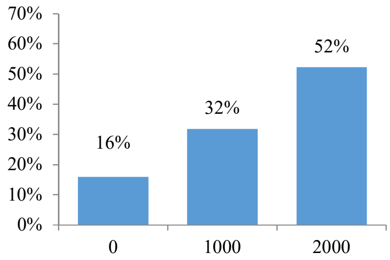
Table 4.2 | Paired t-test for differences in credit demand means across treatments.

	Without insurance		With insurance		t-test		N
	Mean	*Std. Error*	*Mean*	*Std. Error*	*Difference*	*p-value*	
Limited liability	1.39	0.053	1.36	0.052	-0.03	0.593	132
Uncertainty	0.62	0.050	1.14	0.052	0.53***	0.000	132
Full liability	0.51	0.051	0.96	0.053	0.45***	0.000	132

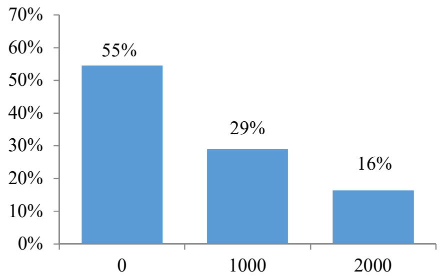
Note: *** p < 0.01, ** p < 0.05, * p < 0.1



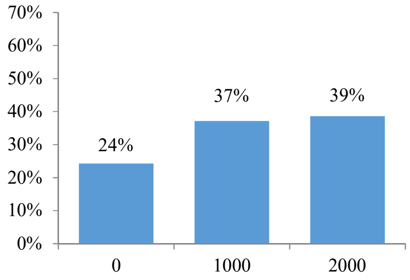
4.2a Loan **without** insurance and limited liability



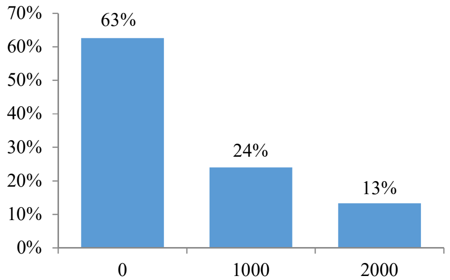
4.2b Loan **with** insurance and limited liability



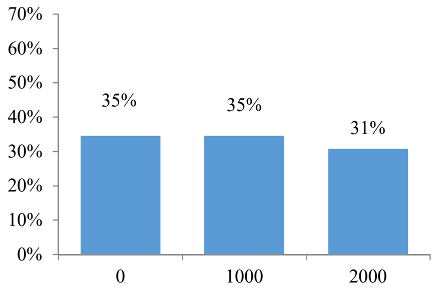
4.2c Loan **without** insurance and uncertain liability



4.2d Loan **with** insurance and uncertain liability



4.2e Loan **without** insurance and full liability



4.2f Loan **with** insurance and full liability

Figure 4.2 | Credit demand by treatment.

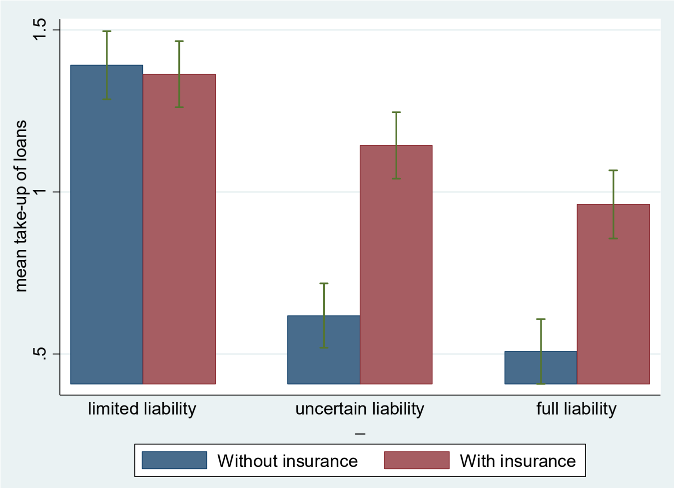


Figure 4.3 | Differences in credit demand across treatments.

To formally analyze the effect of mandatory insurance and liability, we estimate the following equation:

$$Y_{ijk} = \alpha + \beta_1 Insurance_j + \beta_2 Uncertain\_liability_k + \beta_3 Full\_liability_k + \beta_4 Insurance_j \times Uncertain\_liability_k + \beta_5 Insurance_j \times Full\_liability_k + \gamma_i + \varepsilon_{ij} \quad (4)$$

Our dependent variable Y_{ijk} is the arage amount borrowed by farmer *i* in insurance treatment *j* and under liability treatment *k*, γ_i are farmer fixed effects, and ε_{ij} is the error term. The average amount borrowed is measured as the average of all three rounds within a treatment. The treatment with no insurance and limited liability is taken as the reference. Standard errors are clustered by farmer.

In Table 4.3, the first column shows that the introduction of mandatory insurance has no significant effect on credit demand when liability is limited. This result differ from Giné and Yang (2009) where take-up was significantly lower among farmers offered insurance with the loan in the presence of limited liability. Hence, we do not find evidence for their prediction that insurance reduces credit demand in case of limited liability.

On the other hand, the coefficients on the interaction terms show that, when there is uncertainty about liability or full liability, the effect of mandatory insurance is positive and highly significant. Moreover, the effect is large: insurance increases credit demand by around 0.5 (or 500 CRC), which is more than one-third of the sample average (1.38), and close to one standard deviation (0.60).

Table 4.3 | Impact of insurance and liability on credit demand.

Dependent variable: average amount borrowed	Full sample	First round per treatment dropped	Subsample treatment order 1	Subsample treatment order 2
Insurance	-0.03	0.00	-0.02	-0.03
	[0.05]	[0.06]	[0.07]	[0.08]
Uncertain liability	-0.77***	-0.83***	-0.88***	-0.66***
	[0.06]	[0.07]	[0.08]	[0.09]
Full liability	-0.88***	-0.95***	-0.97***	-0.79***
	[0.06]	[0.07]	[0.08]	[0.10]
Insurance * Uncertain liability	0.55***	0.52***	0.58***	0.52***
	[0.06]	[0.07]	[0.09]	[0.09]
Insurance * Full liability	0.48***	0.47***	0.53***	0.43***
	[0.06]	[0.07]	[0.09]	[0.08]
Constant	1.39***	1.41***	1.41***	1.38***
	[0.04]	[0.05]	[0.05]	[0.06]
Fixed effects	Y	Y	Y	Y
Mean dependent variable	1.38	1.41	1.39	1.36
Observations	792	792	414	378
R-squared within subjects	0.417	0.385	0.487	0.348
Number of subjects	132	132	69	63

Note: standard errors clustered by respondent are shown in in brackets, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

We perform a number of robustness checks. First, we drop the first observation of each round, as this may be considered a practice round, after which farmers are better able to determine their preferred level of credit. Hence, the dependent variable is the average amount borrowed across the second and third round of the respective treatment (results in the second column of Table 4.3). Second, to ensure results are not driven by the order of the rounds in the experiment, we split the sample according to the order of treatments: each session followed one of two possible (randomly determined) treatment orders, so we analyze whether results differ between the two groups of experiment sessions (third and fourth columns of Table 4.3). In all estimations, we find very similar results.

4.5.1 Heterogeneous treatment effect

In this section, we present results for heterogeneous effects of treatment. We start with verifying whether treatment effects depend on farmers' total area planted with coffee. Recall from Table 4.1 that coffee area is significantly smaller for farmers in the experiment sample, compared to the total random sample of farmers that were invited to participate. Results in Table 4.4 show there are no differences by farmers' total coffee area. This suggests our results are representative of farmers in the two regions, even though our sample is not representative in terms of coffee area planted.

Table 4.4 | Heterogeneous effects for coffee area.

Dependent variable: average amount borrowed	(1)
Insurance	-0.06
	[0.07]
Uncertain liability	-0.70***
	[0.08]
Full liability	-0.77***
	[0.09]
Insurance*Uncertain liability	0.52***
	[0.09]
Insurance*Full liability	0.46***
	[0.09]
Coffee area*Insurance	0.01
	[0.02]
Coffee area*Uncertain liability	-0.03
	[0.02]
Coffee area*Full liability	-0.04*
	[0.02]
Coffee area*Insurance*Uncertain liability	0.01
	[0.02]
Coffee area*Insurance*Full liability	0.01
	[0.02]
Constant	1.39***
	[0.04]
Fix effects	Y
Mean dependent variable	1.38
Observations	792
R-squared with-in subjects	0.425
Number of subjects	132

Note: standard errors clustered by respondent are shown in in brackets, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

We analyze other heterogeneous effects across farmers' social and financial indicators (Appendix 4.1). Evidence from previous studies suggests there is a positive relationship between credit take-up and farmer's education and income (Giné and Yang 2009), and that previous exposure to weather shocks can affect the take-up of credit bundled with insurance (Hill and Viceisza 2012). We analyze heterogeneity by farmers' age, gender, years of schooling, percentage of income coming from coffee harvest, and having been affected by other shocks in the past, including their experience with the recent coffee leaf rust epidemic in 2012-13. However, we do not find significant effects of these variables in interaction with treatments (Appendix 4.1, Table A1).

Our lab in the field experiment design was relatively simple and easy to understand for farmers, compared to previous field experiments evaluating existing insurance programs (which typically carry basis risk and involve trust concerns due to lack of information). This may explain why we find no differences based on education, income, and previous experience with shocks. Another explanation could be that our experimental sample and the farmers' population in our study regions in general is very homogeneous in their socioeconomic characteristics, and hence we do not have sufficient variability in the data to detect heterogeneous effects.

4.6 Conclusions and discussion

In this paper, examine the effect of farmers' liability on the uptake of credit with and without mandatory insurance, using a lab-in-the-field experiment. We follow the theoretical model of Giné and Yang (2009) and conduct an experiment with coffee farmers in Costa Rica. Using their conceptual model, we explore the effect of insurance on the credit demand under different degrees of liability.

Our results show that the uptake of loans bundled with insurance is significantly higher than uptake of loans without insurance, both when farmers are liable for sure for their debt, and interestingly when there is uncertainty about their liability. When farmers are not liable for their debt, i.e. under limited liability, the uptake of credit is high irrespective of whether the loans are insured or not. This last result differs from Giné and Yang (2009) where take-up was significantly lower among farmers offered insurance with the loan in the presence of limited liability. A possible explanation is that by design, in our experiment there was never a binary choice between loans with and without insurance. Farmers were offered one option at a time, and then decided on the level of investment. In the presence of limited liability, acquiring debt is very attractive, and the saliency of costly insurance is not the decisive factor. Moreover, it could also be that farmers considered that governmental support (reflected as limited liability for farmers) and insurance are not necessarily substitutes of each other, but complementary policies.

Our results suggest that in order to increase the uptake of insurance as a strategy to increase private investment and reduce the vulnerability of farmers to shocks, it is important that farmers are liable with at least some probability. Governments in developing countries have accustomed farmers to enjoy limited liability when in reality there is always uncertainty about the level of governmental resources. We show that clearly and credibly communicating this level of uncertainty can result in increased uptake of insured credit and hence in farmers being better covered against risk.

In terms of policy design, our results show that the "principle" of limited liability does not have be abandoned altogether in order to generate an increase in the uptake of insured credit. Because of public pressure and a concern for the well-being of farmers in rural areas, authorities are typically reluctant to make farmers fully liable for debt that was validly acquired by farmers in the pursuit of their trade, but that has gone wrong because of extreme hydro-meteorological events or pests. According to our results, introducing uncertainty on the promise of limited liability is enough to increase insured investment, thereby reducing the vulnerability of farmers to shocks. Moreover, providing limited liability only in probabilistic terms should ease the burden on the authorities should there be a systemic shock to farmers.

An important question we do not address in this study is how to design insurance products. More research needs to be done on the supply side, taking into account the lender's standpoint and other conditions under which financial instruments like the weather index insurance is likely to work. In theory, well-designed insurance can incentivize investment in new technologies by small-scale farmers, but the impact of the insurance depends strongly on the collateral requirements by the lender (Carter et al. 2016). Lenders in developing countries are diverse, from private companies to state support entities and from informal to informal lenders, with different characteristics and requirements that shape the environment for the sustainability of credit and insurance instruments.

4.7 Appendices

Appendix 4.1 | Additional results.

Table A1 | Heterogeneous effects by farmers' characteristics.

Dependent variable: average amount borrowed	Interaction variable (X)				
	Age (2)	Female (3)	Schooling[1] (4)	Income[2] (5)	Shocks[3] (6)
Insurance	0.1830	-0.0339	-0.0746	-0.0313	-0.0145
	[0.1978]	[0.0539]	[0.1500]	[0.1127]	[0.1333]
Uncertainty	-1.3330***	-0.7684***	-0.4653***	-0.7045***	-0.9420***
	[0.2191]	[0.0571]	[0.1376]	[0.1128]	[0.1236]
Full liability	-1.8480***	-0.8870***	-0.5074***	-0.7490***	-1.0000***
	[0.2301]	[0.0642]	[0.1649]	[0.1303]	[0.1404]
Insurance*Uncertain liability	0.4735*	0.5706***	0.5636***	0.5234***	0.6377***
	[0.2530]	[0.0637]	[0.1546]	[0.1254]	[0.1744]
Insurance*Full liability	0.8242***	0.4802***	0.2668*	0.3320***	0.4348***
	[0.2326]	[0.0645]	[0.1535]	[0.1194]	[0.1441]
X*insurance	-0.0041	0.0577	0.0081	-0.0002	-0.0161
	[0.0037]	[0.1953]	[0.0259]	[0.0016]	[0.1448]
X*Uncertain liability	0.0109**	-0.0412	-0.0531**	-0.0008	0.2050
	[0.0042]	[0.2625]	[0.0206]	[0.0017]	[0.1395]
X*Full liability	0.0187***	0.0299	-0.0650**	-0.0023	0.1407
	[0.0044]	[0.2412]	[0.0276]	[0.0018]	[0.1566]
X*Insurance*Uncertain liability	0.0015	-0.1659	-0.0018	0.0006	-0.1025
	[0.0050]	[0.2469]	[0.0226]	[0.0018]	[0.1864]
X*Insurance*Full liability	-0.0066	0.0198	0.0372	0.0028	0.0576
	[0.0044]	[0.2365]	[0.0242]	[0.0017]	[0.1599]
Constant	1.3914***	1.3914***	1.3914***	1.3942***	1.3914***
	[0.0383]	[0.0391]	[0.0384]	[0.0406]	[0.0390]
Fixed effects	Y	Y	Y	Y	Y
Mean dependent variable	1.38	1.38	1.38	1.38	1.38
Observations	792	792	792	756	792
R-squared within subjects	0.451	0.420	0.438	0.411	0.421
Number of subjects	132	132	132	126	132

Note: standard errors clustered by respondent are shown in in brackets, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

[1] Years of schooling

[2] Percentage of the total household income coming from coffee farming

[3] Dummy variable of being affected by shocks in the past including their experience with the recent coffee leaf rust epidemic in 2012-13

Appendix 4.2 | General Experimental Instructions.

I. Welcome procedures

1. *Meet people at the door; request ID.*
2. *Match ID with survey ID.*
3. *Write the ID survey number in the decision sheet booklet.*
4. *Give them the closed decision sheet booklet. Stress that they can't open the booklet until indicated by the coordinator.*
5. *Invite them to sit, assigning them randomly across the room.*

II. General instructions for farmers and experimenters

[START POWERPOINT PRESENTATION]

☼ **[Slide 1]** *Good afternoon. Today you will participate in a decision-making workshop. You are invited as a follow-up to a survey conducted last year. The exercises are based on real-life decisions that will allow us to learn from your experience, according to the decisions made during the workshop. The workshop will last about two hours, and we need to stay together until the end. At the end of the workshop, you will be compensated with real money, the amount of which will depend on the decisions made and on chance. You will receive a minimum payment of 2,000 colones, plus the result of your decisions in the workshop exercises.*

We are going to read the instructions together. First the general instructions and then gradually through the decision game rounds. Listen carefully to the instructions for each choice. Look carefully at the possible payments and the probabilities associated with each choice before making a decision. Remember that your final earnings will depend on the decisions you make and on chance.

If you have any questions, please raise your hand, and one of my colleagues or I will come to help you! Please do not hesitate to ask a question if you do not understand. There are no right or wrong answers. Your decisions are personal and depend on your own preferences. Your decisions are also anonymous. This means the decisions can only be yours and your choices will remain private. So, please remain quiet and do not share your decisions or talk to the person sitting next to you. ***This is very important!***

[GO THROUGH INSTRUCTIONS WITHOUT INVITING QUESTIONS. AVOID PUBLIC QUESTIONS]

☼ **[Slide 2]** **<u>To borrow or not to borrow money from the bank</u>**

You choices today consist of deciding whether or not to take a loan to invest in your farm. If you decide to invest, just like any loan contract, you must have illiquid assets as a guarantee in case you cannot pay the loan. These assets can be your own farmland, house or other properties that are taken by the lender if you can´t pay back what you borrowed.

Since we cannot quantify what you possess, today you all have the same value of wealth as a guarantee, equal to ***3,000 colones,*** *before you make your decision.*

☼ **[Slide 3]** **You pay an interest rate**

You decide how much to borrow. You can borrow nothing, 1,000, 2,000 or 3,000 colones. The decision is yours. Remember there are no right or wrong answers.

Like any credit, you must pay an interest rate to the Bank*. The interest rate is* ***10%*** *of the amount you decide to borrow. This means that, according to the table below, if you decide to invest and borrow* ***1,000 colones*** *from the bank, you have to pay back* ***1,100 colones****. If you invest and borrow* ***2,000 colones****, you have to pay back* ***2,200 colones****. If you decide* ***not to borrow****, then you pay nothing back to the bank.* ***Do you have any questions?***

You invest and borrow	You pay back to the bank
₡0	₡0
₡1,000	₡1,100
₡2,000	₡2,200

☼ **[Slide 4]** **Your investment is risky and depends on the weather**

Note that the result depends on the weather. For example, consider renewing your farm with a new variety of coffee. If things go well and the weather conditions are favorable, you get a profit. However, if things go wrong and there is a lack of rain or a hurricane to damage your new coffee plantation, then you will have a much lower output than if you had not invested.

The probability of a good or bad result is 50/50. That is, after deciding how much to borrow to invest, you have to throw a coin. If the coin marks "Crown," that means there will be good weather and if the coin marks "Shield," that means there will be bad weather. If you choose not to invest, you will have an output of 3,000 colones if there is good weather and production of 2,000 colones if weather conditions are not favorable and affect the harvest.

On the other hand, if you decide to borrow and invest, there is a chance that things will go well and that you will earn more money, or that things will go badly and you will be worse off than without investing. If you borrow and invest 1,000 colones and there is good weather, you might get 8,000 colones and if bad weather 1,000 colones. If you borrow and invest 2,000 colones and there is good weather, you might get 13,000 colones, and if bad weather zero colones. ***Do you have any questions?***

You invest and borrow	Good weather	Bad weather
₡0	₡3,000	₡2,000
₡1,000	₡8,000	₡1,000
₡2,000	₡13,000	₡0

Do you have any questions? [WAIT AND EXPLAIN AGAIN IF NECESSARY]

☼ **[Slide 4] Production + Capital – Payment to the bank**

After the good or bad weather determines the outcome of your investment, you will still have to pay the Bank according to your loan. Remember that everyone has the same initial capital as collateral, which is equal to ₡ 3,000. Therefore, your final output is production + capital – what you have to pay the bank.


You invest and borrow	Good weather	Bad weather
₡0	₡3,000 + ₡3,000 – ₡0 = **₡6,000**	₡2,000 + 3,000 – ₡0 = **₡5,000**
₡1.000	₡8,000 + ₡3,000 – 1,100 = **₡9,900**	₡1,000 + ₡3,000 – 1,100 = **₡2,900**
₡2.000	₡13,000 + ₡3,000 – 2,200 = **₡13,800**	₡0 +₡3,000 – 2,200 = **₡800**

Any questions? [WAIT AND EXPLAIN AGAIN IF NECESSARY]

☼ **[Slide 5] Weather insurance**

Pay attention to the instructions. Sometimes the loan offered is bundled with insurance. This means that, when you take the loan, it includes ***mandatory insurance****. The benefits from the insurance are that it takes care of repaying the bank when bad weather events occur, securing your assets. However, the insurance is costly. Therefore, when the weather is good, there is a cost reflected by the amount to repay to the bank.*


You invest and borrow	You pay back the bank with NO insurance	You pay back to the bank with insurance
₡0	₡0	₡0
₡1.000	₡1,100	₡2,200
₡2.000	₡2,200	₡4,400

Do you have any questions? [WAIT AND EXPLAIN AGAIN IF NECESSARY]

☼ **[Slide 6] Government help in case of bad weather**

Sometimes when a bad weather event occurs and affects an entire sector, for example, coffee production, the government takes action to relieve the consequences of the shock. In the past, the government has applied debt forgiveness on credit loans when farmers affected by shocks can´t pay back the banks. Please pay attention to the instructions, since in some rounds the government will apply debt forgiveness when bad weather events occur and sometimes it might help according to a probability. ***Do you have any questions?***

[WAIT AND EXPLAIN AGAIN IF NECESSARY]

Debt forgiveness when bad weather events occur	
	• No help → You have to pay the bank • Debt forgiveness → You don't have to pay the bank • Depends on a probability → - You have to pay the bank - You don't have to pay the bank 

☼ **[Slide 7]** **<u>Payment procedure</u>**

*You will take **18 decision tasks.** After you have taken all the decisions, one of your decisions will be drawn for real payment. This means the amounts indicated in the decision problem will be paid out for real.*

*At the end of this workshop, one of the **18 decision tasks** will be drawn at random by each of you, by taking one chip out of this bag with equal probability for each decision task to be extracted for payment. You can check that in the bag there will be precisely **18 numbered chips,** one for each decision previously taken. Then, you will draw a coin to pay you according to the good weather or bad weather. **Do you have any questions?***

[WAIT AND EXPLAIN AGAIN IF NECESSARY]

Appendix 4.3 | Example of decision sheet.

EXAMPLE

✓ Credit <u>does not require insurance</u>

✓ The <u>government cannot help</u> and the Bank will seize your properties if no payment

POSSIBLE RESULTS			
Amount borrowed 	Good weather 	Bad weather 	Mark your answer 
₡ 0	₡ 6,000	₡ 5,000	☐
₡ 1,000	₡ 9,900	₡ 2,900	☐
₡ 2,000	₡ 13,800	₡ 800	☐

Appendix 4.4 | Case of loan bundled with insurance.

To analyze the case of loans with mandatory insurance, we need to define the joint probabilities of income and weather. Using the definition of correlation between income and weather $\varepsilon = \rho\sqrt{p(1-p)q(1-q)}$, the joint probabilities can be rewritten as (Giné and Yang, 2009, p.3):

$Pr(Y_H, h) = pq + \varepsilon$

$Pr(Y_H, l) = p(1\text{-}q) - \varepsilon$

$Pr(Y_L, h) = (1\text{-}p)q - \varepsilon$

$Pr(Y_L, l) = (1\text{-}p)(1\text{-}q) + \varepsilon$

Following Giné and Yang (2009), the insurance always pays in states of bad weather, both the loan $(1 + i)C$ and the insurance premium π. The farmer repayment to the bank is: $R^I = (1 + i)(C + \pi)$ for a loan with mandatory insurance and the priced fair premium for the insurance is: $(1 + i)\,\pi = (1 - q)\,R^I$ and simplifies to $\pi = \frac{(1-q)C}{q}$ as follows:

$$\pi = \frac{(1-q)R^I}{(1+i)} = \frac{(1-q)(1+i)(C+\pi)}{(1+i)} = (1-q)(C+\pi) = C - qC + \pi - q\pi$$

$$\pi - \pi + q\pi = C - qC \rightarrow \pi = \frac{(1-q)C}{q}$$

Then, the amount to repay the bank with mandatory insurance writes as a function of the loan without insurance $R^I = \frac{R}{q}$.

$$R^I = (1+i)(C+\pi) = (1+i)\left(C + \frac{(1-q)C}{q}\right) = (1+i)\frac{C}{q} = (1+i)C = \frac{R}{q}$$

We assume $p = q = ½$ and rewrite the joint probabilities. Then $\varepsilon = \rho\sqrt{p(1-p)q(1-q)} = \frac{\rho}{4}$ and the joint probabilities are:

$Pr(Y_H, h) = pq + \varepsilon = \frac{(1+\rho)}{4}$

$Pr(Y_H, l) = p(1\text{-}q) - \varepsilon = \frac{(1-\rho)}{4}$

$Pr(Y_L, h) = (1\text{-}p)q - \varepsilon = \frac{(1-\rho)}{4}$

$Pr(Y_L, l) = (1\text{-}p)(1\text{-}q) + \varepsilon = \frac{(1+\rho)}{4}$

Finally, the repayment of the loan with insurance becomes $R = \frac{R}{q} = 2R$.

5

Does Eco-Certification Have Environmental Benefits?

Organic Coffee in Costa Rica

Abstract

Eco-certification of coffee, timber, and other high-value agricultural commodities is increasingly widespread. In principle, it can improve commodity producers' environmental performance, even in countries where state regulation is weak. However, eco-certification will have limited environmental benefits if, as one would expect, it disproportionately selects for producers already meeting certification standards. Rigorous evaluations of the environmental effects of eco-certification in developing countries that control for selection bias are virtually nonexistent. To help fill this gap, we use detailed farm-level data to analyze the environmental impacts of organic coffee certification in central Costa Rica. We use propensity score matching to control for selection bias. We find that organic certification improves coffee growers' environmental performance. It significantly reduces chemical input use and increases adoption of some environmentally friendly management practices.

This chapter is based on:

Blackman, A. and Naranjo M.A. (2012), "Does Eco-Certification Have Environmental Benefits? Organic Coffee in Costa Rica". *Ecological Economics*. vol: 83: 60–68.

5.1 Introduction

Certifying agricultural commodities as having been produced in an environmentally friendly manner is increasingly popular. For example, in the last half of the 2000s, sales of organic, Rainforest Alliance, and other types of eco-certified coffee quadrupled and now account for 8% of global exports. During this same period, the area certified by the two largest forest eco-labeling umbrella organizations, the Programme for the Endorsement of Forest Certification and the Forest Stewardship Council, doubled and now comprises 18% of globally managed forests (Potts et al. 2010).

According to proponents, eco-certification has the potential to improve commodity producers' environmental performance (Giovannucci and Ponte 2005; Rice and Ward 1996). In theory, it can do this by enabling the consumer to differentiate among commodities based on their environmental attributes. This capability facilitates price premiums and better market access for certified producers, which, in turn, create financial incentives for them to meet certification standards.

If that logic holds, eco-certification may help address pressing environmental problems associated with agricultural commodities in developing countries. Growing and processing bananas, cocoa, coffee, timber, and other high-value agricultural products in poor countries often entail deforestation, soil erosion, and agrochemical pollution. These problems are difficult to tackle using conventional command-and-control regulation because producers are typically small, numerous, and geographically dispersed while regulatory institutions are undermanned and underfunded (Wehrmeyer and Mulugetta 1999). Eco-certification has the potential to sidestep these constraints by creating a private-sector system of economic incentives, monitoring, and enforcement.

Certification programs that aim to improve commodity producers' environmental performance also faces critical challenges. They must ensure that standards, monitoring, and enforcement are stringent enough to exclude poorly performing producers. Also, they must offer price premiums high enough to offset the costs of certification. Even if these two challenges are met, eco-certification schemes still can be undermined by selection effects. Commodity producers already meeting certification standards have strong incentives to select into certification programs: they need not make additional investments in environmental management to pass muster and can obtain price premiums and other benefits. However, certification programs that mainly attract such producers will have limited effects on producer behavior and few environmental benefits.

Although a growing academic literature examines commodity certification, we still know little about whether it affects producers' environmental performance. As discussed below, few studies evaluate the environmental impacts of certification and many of those that do rely on problematic methods that bias their results. To identify certification impacts, an evaluation must construct a credible counterfactual outcome, that is, an estimate of what environmental outcomes for certified entities would have been had they not been certified. However, most evaluations use problematic counterfactual outcomes: either certified producers' precertification outcomes or uncertified producers' outcomes. In the first case, results are biased whenever outcomes change during the study period because of factors unrelated to certification (including changes in commodity prices, input prices, weather conditions, and technology, all of which are common). In the second case, results are biased whenever commodity producers are already meeting certification standards disproportionately select into certification.

A variety of ex-post statistical methods are available to overcome these problems including propensity score matching and instrumental variables (Ferraro 2009; Frondel and Schmidt 2005). A comprehensive review of the published empirical studies of certification of agricultural commodities and tourism operations found only two that use such methods to identify environmental impacts (Blackman and Rivera 2011). (Most empirical studies focus on certification's effects on the socioeconomic status of the producer, e.g., on-farm profit, household income, health, education, etc.) Neither concerns the certification of coffee—one of the most prominent high-value agricultural commodities produced developing countries, both in terms of economic value and eco-certification (Blackman and Rivera 2011; Potts et al. 2010).

As a first step toward filling that gap, this paper presents an evaluation of the environmental impacts of organic coffee certification in central Costa Rica. We use rich farm-level data from a recent census of coffee growers and a geographic information system (GIS) that comprises detailed geophysical data. We rely on propensity score matching to control for selection bias. We find that certification does have an environmental benefit. It significantly reduces the use of all three chemical inputs for which we have data (pesticides, chemical fertilizers, and herbicides) and spurs adoption of at least one of the four environmentally friendly management practices for which we have data (organic fertilizer).

The remainder of this paper is organized as follows. The second section briefly reviews the literature evaluating the environmental effects of coffee certification. The third section presents background on coffee production, organic certification, and our study area. The fourth section discusses our empirical strategy and data. The fifth section presents our results, and the last section discusses their policy implications.

5.2 Literature

Rigorous evaluations of the environmental impacts of certification are rare, and those that have been conducted often fail to find significant effects. Blackman and Rivera (2011), reviewed more than 200 published studies of agricultural commodity and tourism certification and identified only two ex post quantitative studies using farm-level data that both constructed a reasonable counterfactual and focused on environmental (versus socioeconomic) impacts: Rivera and de Leon (2004) and Rivera, de Leon and Koerber (2006). Both studies conclude that the environmental effects of certification are negligible. They analyze the Sustainable Slopes Program, a voluntary certification program established by the U.S. ski areas' industry association. Using a Heckman procedure to control for self-selection bias, they compare third-party environmental performance ratings of certified and uncertified ski areas. They find that in the Sustainable Slopes Program's early years, uncertified areas actually had better environmental performance than certified areas, and subsequently, they had equivalent but not superior levels.

As for ex-post quantitative studies using farm-level data focusing on coffee eco-certification, to our knowledge, all existing published studies that construct a reasonable counterfactual focus on socioeconomic impacts (Blackman and Rivera 2011). Three less rigorous studies analyze environmental impacts by comparing environmental outcomes for certified farms and unmatched uncertified farms and reach mixed conclusions despite the fact that failure to control for self-selection bias typically generates overly optimistic results. Jaffee (2008) compare environmental (and social) outcomes for Fair Trade and certified organic growers and unmatched uncertified growers in Oaxaca, Mexico, and find that certified growers adopt more soil conservation practices. Similarly, Martínez-Torres (2008) compares ecological indicators (soil erosion, number of shade species, and leaf litter depth) for certified organic and unmatched uncertified growers in Chiapas, Mexico, and finds that organic growers perform better. However, Philpott et al. (2007) compare ecological indicators for Fair Trade/organic certified growers and unmatched uncertified growers in Chiapas, Mexico, and find no differences between the two subsamples.

Additional unpublished or purely qualitative studies of the environmental effects of coffee eco-certification also generate mixed results. As for unpublished studies, Quispe-Guanca (2007) uses survey data on changes in environmental management practices before and after certification (organic, FT, Rainforest Alliance, Utz Kapeh, and C.A.F.E. Practices) or a sample of 106 certified farms in Costa Rica. He finds that although all farms reduced herbicide use after certification, most did not reduce other agrochemicals. Similarly, Martínez-Sánchez (2008) compares ecological indicators for ten certified organic and ten unmatched uncertified farms in northern

Nicaragua. He finds that organic farms do not have significantly different shade levels, bird diversity, or bird abundance.

As for qualitative studies, Bray et al. (2002) focus on the environmental (and socioeconomic) effects of organic certification among producers belonging to a single cooperative in Chiapas, Mexico and conclude that producers perceived that certification led to the elimination of "environmentally harmful" practices. Utting-Chamorro (2005) and Utting (2009) examine the environmental (and socioeconomic) effects Fair Trade certification among producers belonging to two cooperatives in northern Nicaragua. She finds only that certification has increased awareness of environmental protection. Finally, van der Vossen (2005) reviews the literature and catalogs a number of adverse environmental and economic effects of the organic certification of small-scale coffee farms in developing countries.

5.3 Background

5.3.1 Coffee in Costa Rica

Although coffee is no longer the backbone of Costa Rica's economy, it remains a leading agricultural commodity. In the 2003/2004 harvest year—the year in which the census data used in our analysis were collected—roughly 60,000 growers produced 2.7 million fanegas (100-pound bags) of coffee beans that earned US $200 million, equivalent to 14% of total agricultural export revenues and 3% of total export revenues. The coffee sector was dominated by small-scale growers: more than 93% produced less than 100 fanegas of coffee per year (ICAFE 2004; 2010).[39]

Coffee growing in Costa Rica has serious environmental consequences that at least partly offset those economic benefits. Traditionally, Costa Rican coffee, like most coffee in Latin America, was grown alongside shade trees, an agroforestry system that predated the development of agrochemicals and therefore did not rely on them. However, since the 1980s, 90% of the country's coffee has been converted to a high-yielding "technified" monocrop in which coffee is grown with minimal shade cover and intensive application of agrochemicals, a system that was pioneered in Costa Rica (Adams and Ghaly 2007; Rice and Ward 1996). The switch to technified coffee has hastened soil erosion and contributed to such off-site negative externalities as the contamination and sedimentation of surface and groundwater (Adams and Ghaly 2007; Babbar and Zak 1995; Loria 1992).

39 In harvest year 2010/2011, roughly 48,000 growers produced 2 million fanegas of coffee beans that earned US$ 232 million, equivalent to 12% of total agricultural export revenues and 3% of total export revenues (ICAFE, 2010). A fanega is a standard unit of volume to measure coffee in Central America, of approximately 250 kg.

5.3.2 Organic coffee certification

We focus on organic certification because it is one of the leading eco-certification schemes worldwide (Potts et al., 2010) and emphasizes environmental, not socioeconomic, performance. Organic agriculture certification requires producers to adhere to five broad production principles (IFOAM 2010; Van der Vossen 2005).

- use of composted organic matter instead of chemical fertilizers to maintain soil quality;
- use of natural methods for controlling disease, pests, and weeds instead of synthetic pesticides and herbicides;
- use of soil conservation practices, including contour planting, terracing, planting cover crops, mulching, and planting shade trees;
- minimum use of fossil fuels in the production process; and
- minimum pollution during postharvest handling.

Several international organic certifying bodies, the largest of which is the International Federation of Organic Agriculture Movements (IFOAM, 2010), formulate basic organic standards for various commodities. These large organizations accredit smaller national ones that in turn certify producers and conduct follow-up monitoring. Organic certifications require growers to complete a transition period of two to three years during which they must discontinue use of chemical inputs and adopt various conservation and pollution prevention practices. Certified producers are regularly monitored to ensure they continue to meet organic standards.

From the coffee growers' perspective, organic certification has both benefits and costs (Calo et al. 2005; Giovannucci and Ponte 2005; Van der Vossen, 2005). Of the main benefits is the price premium, which is set in international markets and averages 10 to 20%, depending on coffee quality (although, not all growers receive premiums). In addition, the certification also may improve access to markets, production inputs, and agricultural extension services. Finally, organic production reduces the costs of purchased inputs for growers who formerly depended on chemical inputs.

On the cost side, organic production typically increases labor costs and reduces yields for growers who formerly depended on chemical inputs. Also, transaction costs—for initial certification and subsequent annual monitoring and reporting—are significant. Annual costs easily can amount to 5% of sales. Although subsidies are sometimes available, these costs often are borne by the grower. Note that the transition period implies that the grower must pay them for two to three years without one of the principal benefits of certification—a price premium.

5.3.3 Study area and period

We examine organic coffee certification in Turrialba, Costa Rica, a rural canton (an administrative unit that would fall between a state and county in the United States) in the country's central valley, located about 64 km east of Costa Rica's capital.[40] The leading organic certifying organization in Turrialba is a Costa Rican organization called Eco-Logica, which is accredited by the U.S. Department of Agriculture, among other organizations (Eco-Logica 2011). Certified farmers in this region belong to the Association of Organic Producers of Turrialba (APOT 2001). For the reasons discussed below, we analyze coffee certification in 2003, the year of our farm-level census data. In this year, Eco-Logica had certified 38 growers and was tracking 44 more in the transition phase. APOT's organic production standards are included in Appendix A.

5.4 Empirical strategy and data

5.4.1 Propensity score matching

Our analysis of organic certification's impact on environmental performance confronts the usual program evaluation challenge (Holland 1986; Rubin 1974). Ideally, the impact of a program would be measured by comparing the outcome of interest for each agent both with and without program participation. However, we never actually observe both outcomes. In practice, therefore, a program's impact is typically measured by comparing the average outcome for participants and a control group of non-participants—with the latter average serving as the counterfactual. However, as discussed in the introduction, this approach can be undermined if certain types of participants who tend to have certain outcomes disproportionately select into the program. For example, in our case, small, undercapitalized farms that cannot afford to use chemical inputs may self-select into organic certification because the net benefits are high: they can meet organic standards and obtain price premiums without having to discontinue chemical input. Alternatively, farms on steeply sloped land that already use soil conservation measures may self-select into certification because they do not have to adopt them to meet organic standards. An evaluation that failed to control for such selection would conflate the effects of certification on outcomes with the effects of preexisting differences between certified and uncertified farms.

40 We choose this study area because we had excellent access to it. At the time the study was conducted, both authors were working in Turrialba and were therefore able to conduct focus groups with coffee growers, visit farms, and interview local stakeholders to help design the empirical strategy and interpret the results.

To address this selection problem, we use a matching estimator. That is, following Rosenbaum and Rubin (1983) and more recently Blackman et al. (2010), List et al. (2003) and Dehejia and Wahba (2002), we construct a matched control sample of uncertified farms that are very similar to the certified farms in terms of observable characteristics. We measure program impact as the average treatment effect on the treated (ATT)—the difference between the percentage of certified farms that use a management practice and the percentage of matched uncertified farms that use it.

This approach depends on two identifying assumptions. The first assumption, "ignorability" or "conditional independence," is that conditional only on farms' observed characteristics, the certification decision is ignorable for purposes of measuring outcomes. That is, we can observe and control for all variables that simultaneously affect the certification decision and the outcome variables. This first assumption is untestable. The second assumption, "common support" or "overlap," is that the distribution of observed characteristics for uncertified farms is similar to that for certified farms, such that farms with similar characteristics have a positive probability of being certified and uncertified.

Creating a large set of matched pairs of farms with the exact same observed characteristics is challenging when, as in our case, these characteristics are numerous. However, Rosenbaum and Rubin (1983) demonstrate that we need to match farms only based on their propensity score—that is, their likelihood of certification as predicted by a regression model—which amounts to an index of farm and grower characteristics weighted by their importance in predicting certification. The propensity score method collapses the difficult problem of matching all observable characteristics to a much simpler one of matching a single summary variable.

Various methods are available to match certified and uncertified farms based on propensity scores (Caliendo and Kopeinig 2008; Morgan and Harding 2006). To ensure robustness, we report results from five: (i) nearest neighbor 1-to-1 matching, wherein each certified farm is matched to the uncertified farm with the closest propensity score; (ii) nearest neighbor 1-to-4 matching, wherein each certified farm is matched to the four uncertified farms with the closest propensity scores and the counterfactual outcome is the average across these four; (iii) nearest neighbor 1-to-8 matching; (iv) nearest neighbor 1-to-16 matching; and (v) kernel matching, wherein a weighted average of all uncertified farms is used to construct the counterfactual outcome. For all five models, we enforce a common support and allow matching with replacement.

Calculating standard errors for ATT estimated using propensity score matching is not straightforward because these errors should, in principle, account for the fact that propensity scores are estimated and for the imputation of the common support (Heckman et al. 1998). Therefore, following Dehejia and Wahba (2002) and others, we bootstrap standard errors (using 1,000 replications).

5.4.2 Data

The data used for our analysis come from three sources. The first is a national census of Costa Rican coffee growers conducted by the National Statistics and Census Institute (*Instituto Nacional de Estadística y Censos, INEC*) in collaboration with the Costa Rican Coffee Institute (*Instituto del Café de Costa Rica, ICAFE*). Data for Turrialba, with more than 6000 farms, were collected in 2003. The INEC/ICAFE census includes dichotomous dummy variables that indicate whether farms use seven of the agriculture practices monitored by organic certifiers.[41] We divide these into three "negative" practices that must be discontinued for APOT organic certification and four "positive" practices that must be adopted.

The negative practices are the use of:

- chemical pesticides;
- chemical fertilizers; and
- chemical herbicides.

The positive practices are the use of:

- soil conservation measures such as deviation canals, water collection holes, water ladders, and vegetative barriers;
- shade trees;
- windbreaks; and
- organic fertilizer.

In addition to information on these practices, the INEC/ICAFE data include information on grower characteristics (age and education), farm characteristics (e.g., geolocator information, size, and coffee variety), and geophysical characteristics (e.g., temperature and precipitation).

Our second source of data is a GIS complied from a variety of sources. It comprises spatial data on geophysical characteristics of coffee farms, including elevation, aspect (directional orientation), slope, Holdridge life zone, and distances to coffee markets and population centers.

41 Unfortunately, the INEC/ICAFE data do not include measures of the extent of use of management practices such the percentage of the growing area with contour planting, or the percentage with shade.

Our final source of data is a list of 82 APOT farmers for 2003, the year of the INEC/ICAFE census, including 38 certified organic farms and 44 that were in transition. Because the APOT and INEC/ICAFE databases do not include a common identifying code, records were matched by owner name and farm size.

Although the INEC/ICAFE census for Turrialba covered more than 6000 farms, responses to certain questions are missing in some records. We drop all records for which responses needed to generate the variables used in our regressions are missing. The resulting dataset contains 2603 observations: 36 certified organic farms and 2567 uncertified farms.[42]

5.4.3 Variables

Table 5.1 lists, defines and presents summary statistics for the variables used in our matching analysis, including both outcome variables and grower and farm characteristics. In addition to the seven dichotomous outcome variables listed above, we include counts of negative and positive practices on each farm—the sum of the three dichotomous outcome variables for negative practices, and the sum of the four dichotomous outcome variables for positive practices. Mean use rates for the negative practice outcome variables range from a low of 16% for chemical pesticide use to a high of 73% for herbicide use. On average, farms use 1.48 of the three negative practices for which we have data. The mean use rates for the positive practice outcome variables range from a low of 10% for organic fertilizer to 95% for use of some shade cover. On average, farms use 1.59 of the four positive practices for which we have data. Hence, the proportion of growers using practices consistent with organic certification is substantial. As a result, it is reasonable to expect that a disproportionate share of de facto organic growers—that is, those already meeting organic standards—self-selected into organic certification, implying that certification only had limited effects on the environmental performance of the average grower in our sample. Our empirical analysis aims to determine whether that in fact was the case.

5

42 Sample attrition is significant. The six regressors responsible for roughly two thirds of this attrition are AGE, the three education dummies (ED_PRIMARY, ED_SECONDARY, ED_SUPERIOR), OTHER_LOT, and VARIETY_CATA. That is, survey response rates for these variables were relatively low. As reflected in the results of the probit regression used to generate propensity scores (Table 5.2), three of these six variables—AGE, OTHER_LOT, and VARIETY_CATA—are important determinants of organic certification. Therefore, we are reluctant to drop them in order to reduce sample attrition. However, to check the robustness of our findings to sample attrition bias, we dropped all six these variables from our analysis. The resulting sample has 4763 observations. The qualitative propensity score matching results using this larger sample (available from the authors upon request), are identical to those using the smaller sample (reported in Section 5.5). Hence, sample attrition does not drive our qualitative results.

Table 5.1 | Variables, definitions and means.

Variable	Definition	Mean All (n = 2,603)	Mean Certified (n = 36)	Mean Uncert. (n = 2567)
OUTCOME VARS.				
Negative practices				
NEMATICIDE	applies nematicide (0/1)	0.16	0.00	0.17
CHEM_FERT	applies chemical fertilizer (0/1)	0.58	0.11	0.59
HERBICIDE	applies herbicide	0.73	0.11	0.74
COUNT_NEG	count above negative practices	1.48	0.22	1.50
Positive practices				
SOIL_CON	uses soil conservation practices (0/1)	0.46	0.58	0.46
SHADE	uses shade (0/1)	0.95	1.00	0.95
WINDBREAK	uses windbreaks (0/1)	0.14	0.14	0.14
ORG_FERT	applies organic fertilizer (0/1)	0.10	0.67	0.10
COUNT_POS	count above positive practices	1.59	2.36	1.58
GROWER/FARM CHARACTERISTICS				
Grower				
AGE	age (years)	50.61	46.11	50.67
ED_NONE	no education (0/1)	0.09	0.06	0.09
ED_PRIMARY	primary education (0/1)	0.71	0.64	0.71
ED_SECONDARY	secondary education (0/1)	0.08	0.25	0.08
ED_SUPERIOR	> secondary education (0/1)	0.11	0.06	0.11
Farm				
APOT	organic cert or transition (0/1)	0.01	1.00	0.00
AREA_COFFEE	area coffee on the farm (ha.)	1.29	1.64	1.28
AREA_COFFEE_SQ	area coffee on the farm (ha.) squared	5.66	3.97	5.68
OTHER_LOT	2 separate plots of coffee (0/1)	0.37	0.08	0.37
VARIETY_CATA	coffee variety = caturra (0/1)	0.89	0.97	0.89
VARIETY_CATI	coffee variety = catuai (0/1)	0.06	0.03	0.06
VARIETY_CR95	coffee variety = costa rica-95 (0/1)	0.02	0.00	0.02
VARIETY_CATE	coffee variety = catimore (0/1)	0.02	0.00	0.02
Geophysical [b]				
PRECIPITATION	rainfall (mm)	2994.83	2997.25	2994.80
PRECIPITATION_SQ	rainfall (mm) squared	9139495	9102179	9140018
ELEVATION	elevation (m. above sea level)	894.66	811.03	895.83
TEMPERATURE	avg. annual temperature (C°)	22.89	23.09	22.89
A_LEVEL	% farm level	0.05	0.03	0.05
A_NORTH	% farm facing north	0.07	0.06	0.07
A_NORTHEAST	% farm facing northeast	0.15	0.14	0.15
A_EAST	% farm facing east	0.15	0.14	0.15
A_SOUTHEAST	% farm facing southeast	0.14	0.10	0.14
A_SOUTH	% farm facing south	0.12	0.17	0.12

Variable	Definition	Mean All (n = 2,603)	Mean Certified (n = 36)	Mean Uncert. (n = 2567)
A_SOUTHWEST	% farm facing southwest	0.11	0.12	0.11
A_WEST	% farm facing west	0.08	0.12	0.08
A_NORTHWEST	% farm facing northwest	0.10	0.06	0.10
SLOPE	average slope (%) [a]	27.12	27.36	27.12
SLOPE_MAX	maximum slope	53.21	54.15	53.20
SLOPE_SD	standard deviation slope	10.10	9.79	10.10
LZP_BMHP	% farm v. humid premontane	0.71	0.81	0.71
LZP_BPP	% farm rain forest premontane	0.17	0.03	0.18
LZP_BHTTP	% farm v. humid trans premontane	0.02	0.00	0.02
LZP_BHP	% farm humid premontane	0.03	0.00	0.03
DISTANCE_SJOSE	ln road distance San José (minutes)	4.79	4.80	4.79
DISTANCE_CANCAP[b]	ln road distance nearest of 15 Canton capitals (minutes)	3.27	3.11	3.27

[a] % Slope = 100*tan(π angle/180). 100% slope = 45°.
[b] The 15 canton capitals are Aserrí, Cartago, Desamparados, Juan Viñas, Pacayas, Paraíso, Parrita, Quepos, San Ignacio, San Marcos, San Pablo, Santa Maria, Siquirres, Tejar, and Turrialba.

To match certified and uncertified farms, we used propensity scores generated by regressing an organic certification dummy onto a rich set of grower, farm, and geophysical characteristics from our coffee census and GIS data. The grower characteristics are AGE, the age of the farmer in years, and four dichotomous dummy variables that indicate the farmer's highest level of education: ED_NONE for no formal education, ED_PRIMARY for primary education, ED_SECONDARY for secondary education, and ED_SUPERIOR for more than secondary education.

The farm characteristics are AREA_COFFEE, the number of hectares planted in coffee; AREA_COFFEE_SQ, the square of the number of hectares planted; OTHER_LOT, a dichotomous dummy variable that indicates whether the farmer has noncontiguous patches of coffee in the same "work area"; and four dichotomous dummy variables that indicate the variety of coffee planted on the farm: VARIETY_CATA for caturra, VARIETY_CATI for catuai, VARIETY_CR95 for Costa Rica-95, and VARIETY_CATE for catimore.

The farm-level geophysical variables are PRECIPITATION, the average annual rainfall in millimeters; PRECIPITATION_SQ, the square of average annual rainfall; ELEVATION, the average elevation in meters above sea level; TEMPERATURE, the average annual temperature in degrees Celsius; SLOPE, the average slope in percent; SLOPE_MAX, the maximum slope in percent; SLOPE_SD, the standard deviation of slope; DISTANCE_SJOSE, the natural log of travel time in minutes from the farm centroid to San José; and DISTANCE_CANCAP, the natural log of the travel time from the farm centroid to the nearest of 15 canton (county) capitals in Turrialba.[43] The geophysical variables also include several aspect variables (precisely defined in Table 5.1) that indicate the percentage of the farm-oriented in different directions: A_LEVEL, A_NORTH1, A_NORTHEAST, A_EAST, A_SOUTHEAST, A_SOUTH, A_SOUTHWEST, A_WEST, A_NORTHWEST, and A_NORTH2. Finally, we include four variables that indicate the percentage of the farm that falls within the most common Holdridge life zones in our study area: LZP_BMHP, very humid premontane forest; LZP_BPP, premontane rain forest; LZP_BHTTP, very humid transpremontane forest; and LZP_BHP, humid premontane forest.[44]

5.5 Results

5.5.1 Propensity Scores and Balance Tests

Table 5.2 presents the results from the probit regression (of organic certification on grower and farm characteristics) used to generate propensity scores. The results indicate that compared with average growers in our sample, certified growers tend to be younger, and that compared with average farms in our sample, certified farms tend to be larger (although not extremely large), have contiguous growing areas, grow the caturra variety of coffee, and be located at low altitudes and in certain life zones. Also, certified farms tend not to have a large percentage of their farms sloped in certain directions.

Having generated propensity scores and we use them to match certified and uncertified farms. For the kernel estimator, all 36 observations are on the common support and for the remaining estimators, 35 observations are on the common support (Table 5.3).

We performed balance tests for the five matching estimators. All except the kernel estimator achieved balance (a statistically insignificant difference in covariate means for certified and matched uncertified plants) for all 29 covariates (Table 5.3). The kernel estimator achieves balance for all 29 covariates except OTHER_LOT. Table 5.3 reports median standardized bias—Rosenbaum and Rubin's (1983) balance statistic—across all covariates for each matching estimator.[45] The highest

43 % Slope = 100*tan(π angle/180). 100% slope = 45°. The 15 canton capitals in our study area are Aserri, Cartago, Desamparados, Juan Viñas, Pacayas, Paraiso, Parrita, Quepos, San Ignacio, San Marcos, San Pablo, Santa Maria, Siquirres, Tejar, and Turrialba.

44 The Holdridge life zone system is a widely used method of classifying land on the basis of climate and vegetation (Holdridge 1979).

45 Standardized bias is the difference of the sample means in the certified and uncertified subsamples as a percentage of the square root of the average of sample variances in both groups.

median standardized bias is 11.659 for the nearest neighbor 1–1 estimator, and the lowest is 2.694 for the nearest neighbor 1–16 estimator. Although a clear threshold for acceptable median standardized bias does not exist, according to Caliendo and Kopeining (2008), a statistic below 3 to 5% is generally viewed as sufficient. These encouraging balance statistics are likely due to the fact that even though our probit selection model has 29 explanatory variables, our sample includes 75 uncertified farms for each certified farm. As a result, we are able to find close matches for each certified farm.

Table 5.2 | Probit regression results (dependent variable = organic certification).

Variable	Coefficient	S.E.
Grower		
AGE	-0.150***	0.066
ED_PRIMARY	-0.049	0.324
ED_SECONDARY	0.494	0.367
ED_SUPERIOR	-0.148	0.454
Farm		
AREA_COFFEE	0.541***	0.206
AREA_COFFEE_SQ	-0.083*	0.044
OTHER_LOT	-0.644***	0.227
VARIETY_CATA	0.641*	0.368
Geophysical		
PRECIPITATION	1.115	3.162
PRECIPITATION_SQ	-0.252	0.548
ELEVATION	-1.486**	0.568
TEMPERATURE	0.294	0.832
A_LEVEL	-1.436	1.088
A_NORTH1	-1.426	0.928
A_NORTHEAST	-0.998	0.649
A_EAST	-1.001	0.656
A_SOUTHEAST	-1.454**	0.723
A_SOUTH	-0.377	0.644
A_SOUTHWEST	-1.140*	0.665
A_WEST	-0.656	0.649
A_NORTHWEST	-2.032**	0.933
SLOPE	-0.044	0.104
SLOPE_MAX	0.006	0.008
SLOPE_SD	-0.346	0.316
LZP_BMHP	-0.053	0.252
LZP_BPP	-0.742*	0.417
DISTANCE_SJOSE	-0.006	0.395
DISTANCE_CANCAP	-0.015	0.144

Variable	Coefficient	S.E.
CONSTANT	-1.698	4.283
Observations	2,603	
Pseudo R2	0.194	
LL	-153.097	

***, **,* = significant at 1%, 5%, 10% level.

Table 5.3 | Matching quality: Number of treated observations on common support (CS); Number of covariates achieving balance (N); median standardized bias (SB) after matching; for five propensity score matching methods.[a,b,c,d]

Method	CS	N	SB[d]
(i) Nearest neighbor 1-1	35	29	11.659
(ii) Nearest neighbor 1-4	35	29	4.679
(iii) Nearest neighbor 1-8	35	29	4.284
(iv) Nearest neighbor 1-16	35	29	2.694
(v) Kernel	35	29	7.530

[a] The sample includes 36 treated observations.
[b] The model includes 29 covariates.
[c] For a given covariate, the standardized bias (SB) is the difference of means in the certified and matched uncertified subsamples as a percentage of the square root of the average sample variance in both groups. We report the median SB for all covariates.
[d] Median SB before matching is 16.422.

5.5.2 Average treatment effect on the treated

Table 5.4 presents results from the five matching estimators for the negative practices—chemical pesticides, fertilizers, and herbicides—and for a count of negative practices.[46] The results strongly indicate that certification significantly reduces the use of negative practices. For each negative practice, ATT is negative and significant for all five matching estimators. In each case, the magnitude of the effect is substantial. For pesticide, it ranges from 14 to 18 percentage points; that is, the rate of pesticide use is 14 to 18 percentage points lower among certified growers than among matched uncertified growers who represent the counterfactual. For chemical fertilizers, ATT ranges from 43 to 45 percentage points, and for herbicides, it ranges from61 to 71 percentage points. Finally, for the count of negative practices, ATT ranges from 1.2 to 1.3, implying that on average, certified growers use 1.2 to 1.3 fewer negative practices than matched uncertified growers.

46 Note that the mean of the outcome variables for certified farmers is positive, albeit small, implying that a handful of the 32 certified growers in our sample used chemical inputs in 2003. APOT organic standards allow the occasional use of chemical inputs when deemed necessary and preauthorized by a local Eco-Logica inspector (see Appendix A, items 1g and 5c).

Table 5.4 | Negative practices: average treatment effect on treated (ATT) estimates, by outcome variable and matching method; critical value of Rosenbaum's Γ.

Propensity score matching method	Mean treated	ATT	S.E.[a]	P-value	Γ[b]
Pesticide (Nematicide)					
(i) Nearest neighbor 1-1	0	-0.143	0.074	0.053	3.0
(ii) Nearest neighbor 1-4	0	-0.150	0.052	0.004	10.6
(iii) Nearest neighbor 1-8	0	-0.179	0.041	0.000	17.6
(iv) Nearest neighbor 1-16	0	-0.157	0.030	0.000	17.2
(v) Kernel	0	-0.152	0.012	0.000	17,2
Chemical fertilizer					
(i) Nearest neighbor 1-1	0.114	-0.429	0.118	0.000	4.6
(ii) Nearest neighbor 1-4	0.114	-0.464	0.086	0.000	6.2
(iii) Nearest neighbor 1-8	0.114	-0.454	0.075	0.000	5.4
(iv) Nearest neighbor 1-16	0.114	-0.448	0.064	0.000	8.0
(v) Kernel	0.114	-0.449	0.058	0.000	10.0
Herbicides					
(i) Nearest neighbor 1-1	0.114	-0.714	0.105	0.000	7.8
(ii) Nearest neighbor 1-4	0.114	-0.643	0.080	0.000	10.0
(iii) Nearest neighbor 1-8	0.114	-0.607	0.074	0.000	10.0
(iv) Nearest neighbor 1-16	0.114	-0.582	0.064	0.000	11.0
(v) Kernel	0.114	-0.595	0.058	0.000	10.0
Count negative practices					
(i) Nearest neighbor 1-1	0.229	-1.286	0.193	0.000	9.8
(ii) Nearest neighbor 1-4	0.229	-1.257	0.153	0.000	16.8
(iii) Nearest neighbor 1-8	0.229	-1.239	0.129	0.000	13.4
(iv) Nearest neighbor 1-16	0.229	-1.188	0.110	0.000	13.4
(v) Kernel	0.229	-1.197	0.095	0.000	12.2

[a] Computed using bootstrap with 1000 repetitions.
[b] Critical value of odds of differential assignment to organic certification due to unobserved factors (i.e., value above which ATT is no longer significant).

Table 5.5 presents results from the five matching estimators for the positive practices—soil conservation, shade, windbreaks, and organic fertilizer—and for a count of positive practices.[47] The results provide strong evidence that organic certification increases the use of only one positive practice: organic fertilizer. For this practice, ATT is positive and significant for all five matching estimators, and the magnitude of the effect is substantial, ranging from 59 to 63 percentage points.

47 Note that the mean of the outcome variables for certified farmers is less than 1, implying that some of the certified growers in our sample had not adopted the four environmental management practices we consider. In particular, less than one-sixth of certified farmers adopted windbreaks. Eco-Logica inspectors relax certification requirements in certain cases—for example, when winds are so inconsequential that windbreaks are not needed. In general, inspectors enforce prohibitions against negative practices (use of agrochemicals) more stringently than they require the positive ones (soil conservation, etc.) (Soto, 2009).

Table 5.5 | Positive practices: average treatment effect on treated (ATT) estimates, by outcome variable and matching method; critical value of Rosenbaum's Γ.

Propensity score matching method	Mean treated	ATT	S.E.[a]	P-value	Γ
Soil conservation					
(i) Nearest neighbor 1-1	0.571	0.286	0.128	0.026	1.6
(ii) Nearest neighbor 1-4	0.571	0.143	0.102	0.162	-
(iii) Nearest neighbor 1-8	0.571	0.132	0.092	0.150	-
(iv) Nearest neighbor 1-16	0.571	0.146	0.087	0.094	1.4
(v) Kernel	0.571	0.134	0.087	0.125	-
Shade					
(i) Nearest neighbor 1-1	1.000	0.029	0.042	0.501	-
(ii) Nearest neighbor 1-4	1.000	0.043	0.032	0.818	-
(iii) Nearest neighbor 1-8	1.000	0.043	0.022	0.056	6.2
(iv) Nearest neighbor 1-16	1.000	0.045	0.017	0.008	11.6
(v) Kernel	1.000	0.049	0.007	0.000	17.2
Windbreak					
(i) Nearest neighbor 1-1	0.143	0.000	0.087	1.000	-
(ii) Nearest neighbor 1-4	0.143	0.014	0.075	0.849	-
(iii) Nearest neighbor 1-8	0.143	0.043	0.064	0.501	-
(iv) Nearest neighbor 1-16	0.143	0.032	0.064	0.616	-
(v) Kernel	0.143	-0.010	0.065	0.877	-
Organic fertilizer					
(i) Nearest neighbor 1-1	0.657	0.629	0.084	0.000	13.8
(ii) Nearest neighbor 1-4	0.657	0.614	0.086	0.000	9.0
(iii) Nearest neighbor 1-8	0.657	0.604	0.085	0.000	4.4
(iv) Nearest neighbor 1-16	0.657	0.589	0.084	0.000	3.4
(v) Kernel	0.657	0.587	0.082	0.000	3.6
Count positive practices					
(i) Nearest neighbor 1-1	2.343	0.914	0.211	0.000	3.8
(ii) Nearest neighbor 1-4	2.343	0.793	0.173	0.000	3.8
(iii) Nearest neighbor 1-8	2.343	0.811	0.158	0.000	3.6
(iv) Nearest neighbor 1-16	2.343	0.814	0.161	0.000	3.2
(v) Kernel	2.343	0.775	0.145	0.000	2.6

[a] Computed using bootstrap with 1000 repetitions.
[b] Critical value of odds of differential assignment to organic certification due to unobserved factors (i.e., value above which ATT is no longer significant).

The results provide much weaker evidence that organic certification increases the use of shade cover and soil conservation. For shade cover, ATT is significant for three of the five matching estimators (all but nearest neighbor 1–1 and 1–4). However, the magnitude of the effect is small, ranging from 4 to 5 percentage points. For soil conservation, ATT is significant for two of the five matching estimators (nearest neighbors 1–1 and 1–16). In each case, ATT is significant, ranging from 15 to 29 percentage points. Lack of consistently significant ATT for shade and soil conservation may be partly because our sample includes a relatively low number of treatment observations (certified farms). As a result, our difference in means test has less power than it otherwise would. This is a limitation of our analysis. For windbreaks, none of the matching estimators generate a significant ATT. Finally, for the count of positive practices, ATT is positive and significant for all five matching estimators, although the magnitude of the effect is not large, ranging from 0.8 to 0.9.

Hence, our results suggest that organic certification has a stronger causal effect on negative practices than positive ones. This finding comports with anecdotal evidence that Eco-Logica inspectors do not enforce all of the organic certification standards listed in Appendix 5.1 equally: enforcement is more stringent for standards prohibiting negative practices than for those requiring positive ones (Soto 2009).

5.5.3 Sensitivity Analysis

Might endogeneity drive our results? As noted above, the effectiveness of our matching estimators in controlling for selection bias depends on the untestable identifying assumption that we are able to observe confounding variables that simultaneously affect growers' decisions to obtain organic certification and to use or not use the production practices that serve as our outcome variables. That is, we essentially assume endogeneity is not a problem. We calculate Rosenbaum bounds to check the sensitivity of our results to the failure of this assumption (Aakvik 2001; Rosenbaum 2002)[48] Rosenbaum bounds indicate how strongly unobserved confounding factors would need to influence growers' decisions to obtain organic certification in order to undermine the matching result. To be more specific, the Rosenbaum procedure generates a probability value for Wilcoxon sign-rank statistic for a series of values of Γ, an index of the strength of the influence that unobserved confounding factors have on the selection process. $\Gamma = 1$ implies that such factors have no influence, such that pairs of growers matched on observables do not differ in their odds of obtaining organic certification; $\Gamma = 2$ implies that matched pairs could differ in their odds of certification by as much as a factor of two because of unobserved confounding factors; and so forth. The probability value on the Wilcoxon sign-rank statistic is a test of the null hypothesis of a zero ATT given unobserved confounding variables that have an effect given by Γ. So, for example, a probability value of 0.01 and a Γ of 1.2 indicate that ATT would still be significant at the 1% level even if matched pairs differed in their odds of certification by a factor of 1.2 because of unobserved confounding factors.

48 An example of an unobserved confounder might be environmental consciousness or managerial skill. Each could cause growers to select into organic certification and— independent of certification—to use fewer negative practices and more positive ones.

We calculate Γ^*, the critical value of Γ at which ATT is no longer significant at the 10% level in each case where ATT is significant (that is, for each combination of production practice and matching estimator) (Tables 5.4 and 5.5, last column). Except in the case of soil conservation, Γ^* is at least 3.0, and in most cases, it is considerably larger. For the pesticide estimators, Γ^* is at least 10.6 for four of the five matching estimators; for the chemical fertilizer models, it is at least 4.6; for the herbicide models, it is at least 7.8; and for the count of negative practices models, it is at least 9.8. Except in the case of soil conservation, the results for positive practice ATTs are similar. For shade, Γ^* is at least 6.2; for organic fertilizer, it is at least 3.4; and for a count of positive practices, it is at least 3.2. Hence, our sensitivity tests suggest that unobserved confounders would need to be quite strong to undermine our statistically significant results. In other words, endogeneity is unlikely to drive our results.

5.6 Conclusion

We have used detailed cross-sectional data on more than 2600 coffee farms in central Costa Rica to identify the environmental impacts of organic coffee certification in 2003–2004. We have used propensity score matching techniques to control for self-selection bias. Our findings suggest that in our study area and period, certification significantly reduced the use of all three chemical inputs for which we have data (i.e. chemical pesticides, fertilizers, and herbicides) and increased adoption of at least one of the four environmentally friendly management practices for which we have data—organic fertilizer.

Our findings contrast with those from the only two methodologically rigorous studies of eco-certification environmental impacts, both of which find that eco-certification has no causal effects. They also contrast with findings from several less rigorous studies of coffee certification. What might explain these differences? First, we have examined a certification scheme that has relatively well defined, stringent standards enforced by independent third-party monitors. The tourism certification schemes summarized in Section 3 do not have these attributes—the Sustainable Slopes Program examined by Rivera and de Leon (2004), and Rivera et al. (2006) have relatively lax standards enforced by a trade association.

Second, in Costa Rica's coffee sector, opportunities for certification impacts to be undermined by self-selection—that is, opportunities for growers already meeting organic standards to obtain certification—may be relatively limited. As noted in Section 3, coffee growing in Costa Rica is heavily technified. Most farmers use chemical inputs, and few use organic fertilizers (Table 5.1). Therefore, relatively few farms can obtain certification without significantly changing their management practices. This is not the case in the regions of Nicaragua and Mexico studied by Philpott et al. (2007) and Martínez-Sánchez (2008). Here, most growers use rustic practices and few chemical inputs (Rice and Ward, 1996).

Finally, our study has looked at the impact of certification on various management practices, not on ecological indicators like bird diversity, the focus of studies by Philpott et al. (2007) and Martínez-Sánchez (2008). Presumably, certification can alter management practices more easily than it can change in ecological indicators.

What are the policy implications of our findings? They suggest that commodity certification schemes that require adherence to well-defined stringent standards are enforced by independent third-party monitors, and are implemented in areas where producers do not already adhere to these standards can have significant environmental benefits. That said, certification schemes meeting these criteria may have an important disadvantage: they are likely to entail significant costs for producers. Absent high price premiums or other benefits from certification, these costs will discourage certification. Indeed, the relatively small number of certified organic producers in our sample (1%) likely reflects this phenomenon (among other factors such as low variable profits from certification).

5.7 Appendices

Appendix 5.1 | Organic Producers' Association of Turrialba Standards for Organic Production (APOT 2001).

1. **Soil conservation**
   a. Must use soil conservation practices: drains (drenajes), canals (canales de agua), terraces (terrazas), contour planting (siembra en contornos). contamination barriers (barreras de contencion), and overflow ditches (zanjas de ladera para cortar escorrentia).
   b. Must not use herbicides, pesticides, or synthetic goods that damage the soil.
   c. Must use a diverse variety of shades (legumes, fruits, leñosas, musacaeas, etc.) that will be shade useful to the family, coffee and nature.
   d. Must not permit the soil to be exposed to the sun, using soil covers, such as shade, coffee, grass or dead cover. It is recommended to use dead cover in the case of selected vegetables.
   e. Must use windbreakers when necessary, with a preference for species that are useful for the family and the farm.
   f. Must incorporate organic material in the soil, such as bokashi, compost, and compost with worms and others.
   g. Must give preference to always using the resource of the farm, but in the case when it is necessary can use external inputs always of the natural origen, such as products minerals like lime, rock phosphate, dolomite, K-Mg, zinc sulfate, and magnesium sulfate, in cases of documented deficiency.
   h. If cultivation requires it and conditions permit, it's permitted to use a plow.
   i. It is permitted to plant without contours only in plantation already established, but in new plantations, contour planting is required.

2. **Protection and management of water**
   a. Must take care so that there is a good management of water in the farm: reforestation around the rivers, *"acequias o quegbradas,"* to avoid erosion and contamination of the waters with agrochemicals and trash.
   b. Must undertake a rational use of water in the case of the use of irrigation.

3. **Care of biodiversity**
   a. Must take care the farm has a variety of trees, birds, plants and insects: o protect the nature and aid in the control of natural plagues.
   b. Must have diversity in the foods in the farms: for animals and humans.
   c. Must have biodiversity that permits having different income/inputs for the producer and his family in different seasons of the year.
   d. Must have a diversity of cultivation for example: rotation of crops
   e. Using the coffee variety *"catimor"* is not permitted for new planting and replanting coffee.

4. **Care of farm animals**
   a. Must provide the animals with a good space for moving: ventilated and clean, and cannot be in stables all the time.
   b. Must provide to animals clean and organic food.
   c. Must have a diversity of food for animals on the farm.
   d. Must have a proper management of the animal wastes of the farm: must avoid contamination.
   e. Must use natural control of sicknesses: medicinal plants, natural control of parasites.

5. **Management of plagues and sicknesses**
   a. Must favor diversity of cultivation that aids in the natural control of plagues and sicknesses.
   b. Must manage the soil with a diversity of organic material.
   c. The case where it is deemed necessary and with previous authorization of the local inspector, it is permitted to use "copper sulfate" but it is not permitted to apply more than 6.2 kg/ha/year.
   d. It is not permitted to use gasoline for burning of "zomopas" (Atta Cephalotes)

6. **Contamination of the farm**
   a. It is not permitted to throw wastes of containers of agrochemicals in the farm or in sources of water.
   b. It is not permitted to apply synthetic agrochemicals 36 months before the harvest.
   c. Must maintain the distance and the live barrier necessary to avoid contamination that comes from neighboring lots that use agrochemicals.

7. **Post-harvest management practices**
   a. Must only take fresh, mature coffee to the mill.
   b. Must not use sacks contaminated with synthetic agrochemicals.
   c. Cannot mix the organic product with transitional product.
   d. In cases where are producing organic coffee and coffee in transition, must carefully label the organic coffee to avoid mixing it with other types of coffee.
   e. The transport of organic products must be clean and free of contamination.

8. **General care of the farm**
   a. Must plan the farm well and have a road that does not promote erosion
   b. The organic producer must not have the same cultivated field in conventional and organic (parallel production).
   c. The organic producer that already has conventional parcels on which grows crops other than coffee must have a plan for converting the entire farm to organic within the next five years.
   d. Must have a clear separation between live barriers between organic lots and conventional lots.

9. **Norms for the management of the quality of life of producers**
   a. The organic producer must understand the principals of organic agriculture.
   b. The organic producer must undertake training periodically.

6

Synthesis

6.1 Introduction

The success of development projects and programs depend on characteristics of the population, including their choice over the available alternatives and in interaction with the institutions and government policies in place. Hence, the design of policies requires understanding the motivations behind individual decisions, measuring the impact of those decisions and effectively communicating the tradeoffs, so it becomes more clear to the decision maker.

The overall aim of this thesis was to enhance the understanding of farm-level decision making to manage risks in interaction with the institutions and government policies that can influence these decisions. It assessed how individual preferences shaped these decisions and endeavored to show how, through understanding farmers' choices, research can contribute to policy design in agriculture. In addition, it highlighted the role that lab-in-the-field experiments play in informing policy making (Viceisza 2016).

The thesis combined laboratory and field settings to gain insight into whether individual preferences can be directly applied to make real-life predictions. Chapter 2 explored alternatives to elicited risk preferences in the field by evaluating survey estimates next to experimental estimates and showing how different estimates of survey risk preferences relate to real-life farming choices. Chapter 3 focused on the internal and external validity of social preferences, focusing in particular on conditional cooperation. Chapter 4 tested a developed model and examined the effect of farmers' liability on demand for credit with and without insurance. Lab-in-the-field experiments allow us to redefine choices and isolate mechanisms to control the decision environment and study individual behavior at a relatively low cost (Falk and Heckman 2009; Viceisza 2016) and, in combination with other empirical methods, help us understand mechanisms observed in the field, such as preferences and decisions regarding credit and insurance.

In Chapter 5, a quasi-experimental method —matching— allowed us to construct a credible counterfactual outcome to assess impact. In other words, an estimate of what the environmental outcomes for certified entities would have been had they not been certified, in order to measure the impact of the organic certification program.

This last chapter discusses the lessons learned and policy recommendations from individual chapters; then, results are reframed in a thesis-wide perspective and, finally, it presents general conclusions drawn from the research work as a whole.

6.2 Eliciting risk attitudes in the field

In Chapter 2, we evaluated a survey-based method for estimating risk attitudes— a method that can be more easily implemented in the field in comparison to traditional risk experiments. We find no domain-general component across willingness to take risks in specific contexts. A context-free survey estimate of risk preferences predicts risk-taking behavior in the risk experiment. Higher willingness to take risk is associated with the implementation of agricultural practices that required more farm investment, but farmers that report less willingness to take risks are more likely to spend more on fertilizer use.

We learned that when using survey-based instruments, it is necessary to elicit risk attitudes for the specific context of interest, and that estimates can be related to real-life behavior in different ways. Farmers balance the advantages of reducing exposure to uncertainty in agriculture with the increased exposure to financial risk. Hence, it is not strange that practices that required more farm investment, such as investment in new varieties, are associated with a higher willingness to take risks. On the other hand, coffee farmers in Costa Rica have good access to information regarding input use from cooperative organizations and extension agents. Information and training add certainty to the use of inputs like fertilizer, which is perceived as a risk-reducing technology. As a result, farmers who are less willing to take risks apply more fertilizer.

Understanding how risk preferences relate to real life-choices is a complex matter. Research has found that the capacity of risk preferences estimates to address farm-level decision-making varies according to method (Charness and Viceisza 2013; Verschoor et al. 2016), the context in the risk question is framed (this thesis), and the type of technology under assessment (Verschoor et al. 2016, and this thesis). Furthermore, risk preferences vary significantly across countries (Vieider et al. 2015) and subject pools (Vieider, Beyene, Bluffstone, Dissanayake, Gebreegziabher, Martinsson, et al. 2018). Differences can be explained by the mind discernment between decisions made taking into account many aspects of life (broadly bracketed) and decisions made in isolation from all others (narrowly bracketed) (Barseghyan et al. 2018; Verschoor et al. 2016). Future research should take into account whether estimates of risk preferences are biased by incorrect assumptions about bracketing (Barseghyan et al. 2018, 558), which can be crop and location specific.

Our focus on the agricultural context has important policy implications. Not only is it important to take into account risk preferences when assessing the impact of agricultural and development programs promoting technology adoption, but it is also essential to make sure that preferences are elicited in the specific context targeted by the prospective policy instrument. If the policy instrument targets a particular adoption, say fertilizer or implementation of improved seeds varieties, survey risk preferences should be elicited in that particular context. However, for a detailed insight into risk preferences parameters, context-free risk experiments become most likely necessary.

6.3 Testing conditional cooperation

In Chapter 3, we looked at social preferences and focused on the typology of conditional cooperation. We tested if the characterization of conditional cooperation is internally consistent and whether it carries through real-life cooperation decisions. We found that conditional cooperators believe they contribute to the public good by matching the contribution of others in the experiment. However, we see no evidence that those classified as conditional cooperators in the experiment also behave this way when it comes to bringing coffee to the local cooperative in real life.

We learned that the typology of conditional cooperation is internally consistent within a public goods game experiment, but we did not find evidence to support that the typology of conditional cooperators carries on from the lab to reality. Our results are in line with other social preferences measured in the lab that do not relate to prosocial behavior measured in real-life (Laury and Taylor 2008; Voors et al. 2011; Voors et al. 2012).

The lack of external validation suggests that social preferences may be specific to a particular context or setting (Voors et al. 2012). Thus, we may relate our results to those found in Chapter 2, where only context-specific estimates of risk preferences relate to real-life choices. Hence, one should be careful to extrapolate the typology of conditional cooperation measured in the lab to other real-life contexts. Future research should consider studying conditional cooperation in different contexts.

Using agricultural cooperatives to test external validity highlighted a few potential policy implications. Agricultural cooperatives seek that farmers cooperate, e.g., bring coffee to the cooperative. The existence of free riding, e.g., not bringing coffee to the cooperative, not only lowers the total product processed by the cooperative but can also bring down the contributions of conditional cooperators in the community. Our results show that the majority of farmers in rural Costa Rica are conditional cooperators, an important group that can enhance cooperation, but also weakens the cooperative structure if farmers hesitate to bring their coffee to the local cooperative. Farmer socio-economic characteristics showed that older, more experienced farmers, are the ones who by tradition bring coffee to the local cooperative. Hence, agricultural cooperative organizations should implement and enhance policies that encourage young coffee farmers' active involvement in the cooperative organization.

6.4 Credit, insurance and farmers' liability

In Chapter 4, we deviated from individual preferences and examined the effect of farmers' liability on the uptake of credit with and without mandatory insurance. We found that the uptake of loans bundled with insurance is significantly higher than the uptake of loans without insurance, both when farmers are sure to be liable for their debt and, interestingly, when there is uncertainty about their liability, as well. When farmers are not liable for their debt, i.e., under limited liability, the uptake of credit is high irrespective of whether the loans are insured or not.

We learned that to increase the uptake of insurance as a strategy to increase private investment and reduce the vulnerability of farmers weather events which may lead to serious crop failure, it is important that farmers are liable with at least some probability. Governments in developing countries have accustomed farmers to enjoy limited liability when in reality there is always uncertainty about the level of governmental resources. We show that communicating this level of uncertainty clearly and credibly can result in increased uptake of insured credit and hence in farmers being better covered against risk.

Regarding policy implications, our results show that limited liability does not have to be eliminated in order to generate an increase in the uptake of insured credit. Authorities are typically reluctant to make farmers fully liable for their debt, because of public pressure and concerns for the well-being of farmers in rural areas. However, sometimes governments lose the capacity to support farmers because of excessive rains, droughts or pests. According to our results, introducing uncertainty on the promise of limited liability is enough to increase insured investment, thereby reducing the vulnerability of farmers to weather events which may lead to serious crop failure. Moreover, providing limited liability only in probabilistic terms should ease the burden on the authorities should there be a systemic shock to farmers.

6.5 Does Eco-Certification have environmental benefits?

In Chapter 5, we examined the environmental impacts of organic coffee certification, a farm-level decision to differentiate the output, gain higher premiums and manage risks in an environmentally friendly way. The findings showed that, in the area and period studied, certification significantly reduced the use of all three chemical inputs: pesticides, fertilizers, and herbicides; and increased adoption of at least one of the four environmentally friendly management practices, organic fertilizer.

We learned that organic coffee certification has environmental impacts by reducing chemical inputs and fostering the adoption of environmentally friendly practices. However, our findings suggest that to have a significant impact on changing farming practices, commodity certification schemes must require adherence to well-defined stringent standards; enforcement at the individual farm-level by independent third-party monitors, and implementation in areas where producers do not already adhere to these standards.

Such certification requirements have significant policy implications, since certification schemes meeting these criteria may bring forth a substantial disadvantage— they are likely to entail significant costs for producers. Furthermore, without significant premiums or other benefits from certification, these costs will discourage certification. The relatively small number of certified organic producers in our sample reflect these conditions.

6.6 General conclusions

Daniel Kahneman's explanation on how the mind makes decisions in two systems: the automatic and impulsive vs. the conscious, aware and considerate; and how the two systems do not alternate or work together is a reminder that understanding how people act and behave is challenging to analyze (Kahneman 2011). Furthermore, preferences are endogenous to the local environment and institutions in place, and at the same time, the local environment and institutions are reshaped continuously from those individual choices and preferences (Bowles et al. 2003; Cecchi 2015; Williamson 2000).

The overarching aim of this thesis was to enhance the understanding of farm-level decision making to manage risks. It assessed how individual preferences shaped these decisions and explored individual decision-making in interaction with the institutions and government policies that can influence these decisions. Finally, it endeavored to show how, through understanding farmers' choices, research can contribute to policy design in agriculture.

Preferences are addressed directly or indirectly throughout several chapters. We assessed individual risk preferences from a methodological point of view and questioned what we are measuring when eliciting risk preferences in the field and how estimates can vary when evaluating real-life choices (Chapter 2). We showed in a model that when comparing insured and uninsured credit, farmers' liability and risk aversion can determine which type of loan is preferred (Chapter 4). Also, the decision to certify or not the coffee as organic could, on the one hand, increase vulnerability to pests and diseases, and on the other hand, could reduce the dependence on costly inputs and include premiums (Chapter 5), and in both cases, risk preferences play a role. Thus, further research should insist on including the study of risk preferences in the adoption of risk management strategies, including voluntary certification schemes.

Furthermore, we inquired the external validity of preferences and compared farmers' pro-social behavior within the experiment and outside the laboratory. We learned about the importance of context to study not only risk preferences but for social preferences, as well. On the one hand, only when risk preferences are elicited in a particular context, estimates relate to real-life agricultural choices (Chapter 2). On the other hand, the lack of external validation for conditional cooperation suggests that social preferences can also vary among settings (Chapter 3). The external validity of context-free experiments is a relevant discussion topic, and researchers should be careful when context-free experiments are conducted to inform policy. Future research should focus on the external validity of risk and social preferences in different contexts to address specific policies.

A lab-in-the-field experiment framed in the context of credit for farm investment, helped us control the decision environment and isolate the treatment effects of studying farmer choices for credit (Chapter 4). Even so, it used a simple and easy to explain index insurance that does not consider basis risks. Which again question its real-life applicability. Further research must contemplate the behavioral consequences of basis risk on the design of index insurance.

An effective risk management policy should be based on how farmers are exposed to risk and which are their preferences, and policies in place should not constrain farmers' strategies. The role of policy is to ensure that farmers receive the information and tools necessary to manage their risks, with emphasis on the dissemination of relevant information, training, and education (OECD 2011).

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Summary

This thesis contributes to the understanding of individual preferences and farm-level decision making, in interaction with the institutions and government policies that can influence these decisions. Decisions on investments and input used, decisions on whether or not to support local agricultural cooperatives, decisions the demand of credit with or without insurance, and decisions of voluntary certification schemes that have an impact on the livelihood of small farmers in developing countries. I used standard econometric methods to analyze the survey and data collected from three lab-in-the-field experiments, and a one quasi-experiment all implemented with real subjects: coffee farmers in rural Costa Rica.

Chapter 1 starts with an overview of the main topics: individual preferences and farm-level decisions. It introduces the concept of risk preferences, social preferences, formal market-based strategies and certification schemes, that can be adopted by farmers to manage risks. More specifically, it presents the research questions, methodology, empirical strategy the study area and explains how the data was collected for the subsequent main chapters of the thesis.

In Chapter 2, we evaluate a survey-based method to other methods that elicit farmers' risk attitudes. For both researchers and practitioners, surveys can be easier to implement than field experiments in developing countries. We first assess correlations between a context-free survey estimate of risk-taking and context-specific risk preferences. Then, we test whether survey data predicts risk-taking behavior in an incentivized experiment. Finally, we show how the different estimates of survey risk preferences relate to real-life farming choices in a population of coffee farmers in Costa Rica. Our results indicate that one should be careful when extrapolating risk attitudes across contexts. Context-neutral and context-specific survey questions elicit different risk preferences. While the context-free survey estimate of risk preferences predicts risk-taking behavior in a context-free risk experiment, and context-specific estimates are associated with risk-taking in the same agricultural real-life context , the context-free survey estimate of risk-taking is not associated with actual risk-taking behavior in the agricultural setting. Connecting these methods to farm practices, we find that higher willingness to take risk is associated with the implementation of agricultural practices that require more farm investment. In contrast, farmers who report less willingness to take risks are more likely to have higher expenditures on fertilizer use. Researchers interested in using risk preferences as inputs into the design of policy instruments should make sure that preferences are elicited in the specific context targeted by the potential policy instrument.

Chapter 3 tests the internal and external validity of the typology of a conditional cooperator classified by using a public goods game together with the strategy method. Individuals categorized as conditional cooperators adapt their behavior to the group to which they belong. In Costa Rica, coffee farmers are traditionally organized in agricultural cooperatives, a setting very similar to the scenario presented to an individual facing the strategy method in a public goods game: how much to cooperate, given what others do. Our results show that conditional cooperators believe they contribute to the public good by matching the contribution of others in the experiment. However, we find no evidence that those classified as conditional cooperators in the experiment also behave this way when it comes to bringing coffee to the local cooperative in real life. We show supporting evidence to conclude that the typology of a conditional cooperator is internally consistent, but do not find evidence that the typology of conditional cooperators is externally valid. Our paper is a contribution to the external validity of context-free experiments and helps in understanding cooperative behavior relevant to the sustainability of agricultural cooperatives in the developing world.

In Chapter 4, we examine the effect of farmers' liability on demand for credit with and without insurance. We test predictions of a theoretical model in a lab in the field experiment with coffee farmers in Costa Rica. Farmers choose how much to invest in six different settings, described on the one hand by whether the loan is insured or not, and on the other by their liability. Our results show that the uptake of loans bundled with insurance is significantly higher than uptake of loans without insurance, both when farmers are liable for sure for their debt, and interestingly when there is uncertainty about their liability. When farmers are not liable for their debt, i.e. under limited liability, the uptake of credit is high irrespective of whether the loans are insured or not. Our results suggest that in order to increase the uptake of insurance as a strategy to increase private investment and reduce the vulnerability of farmers to weather events which may lead to serious crop failure, it is important that farmers are liable with at least some probability. In terms of policy design, our results show that the "principle" of limited liability does not have be abandoned altogether in order to generate an increase in the uptake of insured credit.

Chapter 5 evaluates the environmental impacts of organic coffee certification. Eco-certification of coffee, timber and other high-value agricultural commodities is increasingly widespread. In principle, it can improve commodity producers' environmental performance, even in countries where state regulation is weak. But eco-certification will have limited environmental benefits if, as one would expect,

it disproportionately selects for producers already meeting certification standards. Rigorous evaluations of the environmental effects of eco-certification in developing countries that control for selection bias are virtually nonexistent. To help fill this gap, we use detailed farm-level data to analyze the environmental impacts of organic coffee certification in central Costa Rica. We use propensity score matching to control for selection bias. We find that organic certification improves coffee growers' environmental performance. It significantly reduces chemical input use and increases adoption of some environmentally friendly management practices.

To conclude, Chapter 6 presents a synthesis and the previous core chapters. Conclusions, lessons learned and policy recommendations of individual chapters are reconsidered. Subsequently, results are placed in a thesis-wide perspective and finally, general conclusions made.

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Completed Training and Supervision Plan
Wageningen School of Social Sciences (WASS)

Wageningen School of Social Sciences

Name of the learning activity	Department/Institute	Year	ECTS*
A) Project related competences			
Advanced Econometrics, AEP 60306	WUR	2013	6
Advanced Microeconomics, ECH 32306	WUR	2013	6
Advanced Macroeconomics, ENR 30806	WUR	2013	6
Behavioral and Experimental Economics, ECH 51306	WUR	2015	2
Experimental Development Economics Lab in the Field Workshop	University of East Anglia	2015	1
Summer school in Development Economics	IDEAS, University of Verona	2016	1
B) General research related competences			
WASS Introduction Course	WASS	2012	1
Scientific Publishing	WGS	2013	0.3
Research Methodology: From topic to proposal	WASS	2013	4
Systematic Literature Review	WASS	2013	2
Techniques for Writing and Presenting a Scientific Paper	WGS	2016	1.2
Research Proposal	WUR	2013	6
C) Career related competences			
'Credit, insurance and farmers' liability: evidence from a Lab in the field experiment with coffee farmers in Costa Rica'	EAERE, Athens, Greece	2017	1
'Risk elicitation in the field: Evidence from coffee farmers in Costa Rica'	SEEDEC, Wageningen University, The Netherlands	2018	1
Total			38.5

*One credit according to ECTS is on average equivalent to 28 hours of study

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