On the design and implementation of environmental conservation mechanisms

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On the Design and Implementation of Environmental Conservation Mechanisms: Evidence from field experiments

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Proefschrift

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Chapter 1:

General Introduction

Sustainable development has become an important lens through which environmental conservation is being viewed (Arrow et al., 1995; Dasgupta, 2013). For sustainable environmental conservation to be a plausible economic policy, Bromley (2005) argues that it has to address two realms: (1) how humans interact with nature (including how people value the environment) and (2) how interactions between humans affect their attitudes to nature (e.g., how social norms shape environmental valuation within a society). In order to address these two realms in economic policy, it is crucial to understand the interactions of people with nature as well as with each other with respect to nature with help of economic mechanisms.

However, design and implementation of mechanisms to understand these two realms are not always straightforward. For instance, in relation to the first realm, i.e. understanding people's valuation of environmental goods, it is clear that valuation depends on the type of environmental services (use vs. non-use value) under consideration (Assessment, 2005; Freeman III, Herriges, & Kling, 2014). In the case of use values such as provisioning services and cultural services of environmental goods, usually, there are markets. For example, people have to decide how much to pay for clean water or to receive recreational services. For non-use values such as bequest and existence values, there are no naturally occurring markets as these services are non-excludable and non-rival in nature. However, valuation techniques (like the Contingent Valuation Method) have been developed to address the first realm. Similarly, scrutinizing the second realm requires an understanding of social norms that may shape the valuation of environmental good in the community. In relation to this, there are numerous studies on the relationship of social norms with the decision to cooperate in the domain of public good provision – the second realm (Bouma, Bulte, & van Soest, 2008; Fehr & Fischbacher, 2004). However, to what extent such norms are stable has not been explored in great detail yet (Ostrom, 2014).

This thesis has two main themes. The first main theme is examining the design and implementation of two different valuation mechanisms to deepen the understanding of the first realm. As noted earlier, in the absence of conventional markets for non-use value of environmental goods, it is not possible to trace the value placed on them. One way of overcoming this problem is designing valuation mechanisms in the form of artificial markets. The so-called Contingent Valuation Method (CVM) was designed to do that and is the most widely applied method in the literature of valuation (Carson & Czajkowski, 2014; Haab, Interis, Petrolia, & Whitehead, 2013; Hanemann, 1994; Oerlemans, Chan, & Volschenk, 2016). Accounting for these values through creating artificial market has indeed affected policies (even when stakes are high) by bringing to attention the different services an environment provides to a society (see Carson et al., 2013). While the application of CVM may improve environmental policy by accounting for non-use values, critics of the CVM have argued that the preferences measured under CVM are not correlated with the value that people attach to the good (Hausman, 2012). Given that CVM is the most widely used mechanism in the market for mentioned valuation, there is a need to deepen the understanding of its application while simultaneously aiming to improve of the mechanism (Oerlemans et al., 2016).

In chapter 2 I examine how variations in information and context affect the outcomes of a valuation exercise. Stated preferences for environmental goods are assumed to be independent of the process via which these environmental outcomes have been produced. Contrary to this assumption, however, empirical findings by Bulte, Gerking, List, and de Zeeuw (2005) show that not only outcomes matter in valuation, but also what mechanisms gave rise to the environmental outcome. In case of environmental degradation, people's valuation of a policy to improve outcomes is found to depend on what factor caused the degradation – nature, or mankind. In this thesis, I extend the work of Bulte et al. (2005) as follows. Using a field experiment I test whether drawing people's attention to the role they play in the process of environmental degradation affects their willingness to pay for mitigation, and how this compares to the activities undertaken by others. I do so by eliciting contributions to a reforestation program in an environmentally valuable area in Ethiopia. I implement three different conditions which allow me to measure the impact on contributions emphasizing (i) the contributions of others to forest conservation, and (ii) the role of the respondents themselves in the forest degradation process. I find that learning about the efforts of others does not affect contributions while emphasizing the

respondents' negative role in the process actually increases contributions. Extant literature notes that including information on human-caused environmental damage in contingent valuation surveys does indeed increases the WTP values. This was, however, attributed to 'outrage effect' – that is, because respondents are upset, they contribute more to environmental goods. In a somewhat different setting this study finds evidence that people's contributions also increase significantly and substantially if attention is drawn to their own responsibility in the deforestation and desertification process, suggesting, the 'responsibility effect' is also important in valuation.

Chapter 3 also sheds light on the first realm as it revisits Payment for Ecosystem Services (PES). PES is a recent mechanism that enables the purchase of eco-system services from willing service providers (Wunder, 2012). One of the main challenges in the design of PES schemes is how to determine the optimal price to offer. With high prices many private land owners are willing to participate in the PES scheme, but the budget will run out sooner too. Few studies (Berry, Fischer, & Guiteras, 2015; Jack, 2013), have compared several pricing policy mechanisms that are theoretically similar. Both of these studies found divergence in performance of the pricing mechanisms when implemented in the field (Berry et al., 2015, p. 14; Jack, 2013, p. 120), pointing out the need for further investigation concerning the causes of such divergence. This chapter builds on Kelsey Jack's (2013) study to test whether incentive-compatible valuation mechanisms – more specifically so-called Uniform Price Auctions (UPA) – are able to predict uptake in the field. We replicate Jack's results that UPA outcomes are a poor predictor of actual participation rates, and find suggestive evidence that the difference in actual and predicted take-up rates is due to more deliberate decision making in UPA than with take-it-or-leave-it offers. We subsequently design and conduct a laboratory experiment to further test this hypothesis. A field experiment was conducted on tree planting projects to test the above hypothesis (with Ghanaian farmers), and a lab experiment was implemented with European students who were endowed with chocolate bars (with unique feature). Using both lab and field experiments, it is found that given the same level of price, the sign-up rate differs between the two mechanisms. More subjects are willing to sign-up in TILI than was predicted in UPA. Our findings also suggest that this disparity can be explained by the hypothesis of more deliberate decision making in UPA than TILI. The argument is that decision making under TILI is not explained by the key economic variables and behavioral preferences (impatience, risk aversion, and loss aversion) of

the subjects whereas these variables seem to be important in decision making under UPA. In the same way, the time taken to make a decisions varies between the two mechanisms; that more time of reflection is taken in uniform price auction than TILI. This further supports the hypothesis of less deliberate decision making in TILI, but not in UPA. Even though decision making under TILI is relatively less complicated, there is a need for careful selection of implementation schemes. Hence, policymakers have to make a trade-off between simplicity and efficiency when deciding on the mechanism they want to implement.

As the second main theme of this thesis, the emphasis is shifted to understanding the human interaction with each other with respect to own actions of environmental conservation. Ostrom (1991) suggests that engaging local community in the conservation can be; (i) cheaper than top-down management style, and (ii) feasible to overcome the tragedy of the commons. More interestingly, people's interaction with each other might evolve depending on their contribution to environmental conservation, if the proper social institution that supports such conservation actions is in place. Even though this line of thinking suggests social norms as a strong instrument in common good conservation, the role of these norms in ensuring sustainable conservation behavior is debatable from a standard economic theory point of view (see Hardin, 1978, 1979; Olson, 2009).

Hence, it is crucial to examine how certain norms such as fairness, trust, and trustworthiness evolve in the community (engaged in conservation) to predict the sustainability of common good conservation. Thus, chapter 4 of the thesis deals with trust and trustworthiness, as important social norms, between the cooperators and non- cooperator in common good conservation. The empirical examples of the community contributing to public goods, contrary to the predictions of the standard economic theory (zero contribution) are numerous (Bouma et al., 2008; Narloch, Pascual, & Drucker, 2012; Pagiola & Rios, 2013; Papacostas, 2014). In the literature, this is often explained by the existence of conditional cooperators in the population, in addition to the selfish agents among others. These conditional cooperators tend to initiate collective actions, trust more and are more trustworthy-behavior that diverge from standard economic prediction (Algan & Cahuc, 2010; Andreoni & Samuelson, 2006; Fehr & Schmidt, 1999; Fischbacher, Gächter, & Fehr, 2001; Knack, 2001). Following the disparity of behavior from the prediction of the standard economic theory, Ostrom (2014) proposed the revised theory of collective action that uses an indirect evolutionary approach (taking into account the existence

of different types of agents in the economy). This theory assumes that players receive objective payoff but the decisions are based on the alteration of the objective payoff into intrinsic value (Güth & Yaari, 1992). Moreover, under full information about the types of agents (say in a trust game), the conditional cooperators will consistently receive a higher payoff whereas, the selfish agents will receive consistently lower payoff since no one will trust them (Ostrom, 1991). Hence, only the cooperators will survive with the complete information process as selfish agents are discriminated (selected) against (Ostrom, 2000). Social identity theory, on the other hand, predicts a different result as far as in-group and out-group discrimination is concerned (Tajfel, 1974). The prediction of this theory is that group members will have more affinity towards their fellow group members over non-members. The two theories, seem to predict two different results. The former theory predicts that all types favor the cooperator type, while the latter theory predicts each group has more affinity towards the fellow member (same type). Here, I examine the case of collaborative forest management (CFM), as it is often called, which is advocated because of its ability to engage the grass root stakeholders in the conservation. I conducted an incentivized field experiment with a slightly modified standard trust game (Berg, Dickhaut, & McCabe, 1995). The results of the experiment indicate that (i) both CFM members and non-CFM members send more money to CFM members than to non-CFM members, and (ii) the non-CFM members, in general, send more money than the CFM members. The difference is mainly caused by the behavior of CFM members towards non-CFM members. In a similar way, the CFM and non-CFM behaved differently with respect to the amount returned. The average share returned by CFM members is lower than the share returned by non-CFM members, which is again as the result of the lower amount returned to non-CFM by CFM members. These findings suggest the difference in the decisions made is driven by both with whom the player is matched and the identity of the player itself, i.e. whether the player belongs to the CFM group. This result supports the hypothesis that high trust is placed on the CFM since CFM members are viewed as more trustworthy than non-CFM members. Therefore, the CFM type receives more money, but return less (particularly to the non-CFM). This allows them to receive consistently higher pay off. Hence, at first it seems that there is some kind of paradox. CFM members may trust non-CFM members less (and thus send less), but in fact, the non-CFM are more trustworthy (return more). The findings of this experiment support the hypothesis that higher trust is placed on the cooperators than non-cooperators. Therefore, it seems that the cooperator type receives more

money, but sends and returns less to non-cooperators which allow the cooperator type to receive consistently higher payoff.

In general, this dissertation deals with different methods of forest conservation and contributes to the understanding of designing appropriate mechanisms. It addresses, in particular, the conditions that influence the functioning of these various conservation mechanisms. It should be noted that the third chapter is the result of collaborative research efforts with other co-authors. As such, the paper is formulated in the first person plural (i.e. “we”) rather than first person singular (i.e. “I”).

Chapter 2:

On the valuation of the causes and consequences of environmental damages: Evidence from a Field Experiment

2.1. Introduction

One of the key assumptions of standard economic theory is that agents attach value to (economic) outcomes, and not to the process by which the outcomes are generated (Sen, 1995). If this were the case, people's willingness to financially contribute to the development of a cure for brain damage would be the same independent of whether accidents or excessive drinking are the main cause of the brain damage. Similar considerations would apply to the appreciation and/or provision of public goods as well – for example, whether the demise of a seal population is due to a natural disease or the consequence of fossil fuel extraction at sea, people's willingness to pay for a seal regeneration project should be the same. In fact, Bulte et al. (2005) find that people's willingness to pay (WTP) for a seal population recovery program is indeed higher when the demise of the species is due to human activity. They attribute this difference to a mechanism labeled as "outrage effect", a term first coined by Kahneman, Ritov, Jacowitz, and Grant (1993) — people are more upset if they think the damage to the environment is caused by human activities they are not directly engaged in themselves.

In this paper, I extend the work by Bulte et al. (2005) by addressing the question of whether people's willingness to pay for protecting the environment is also higher if not other humans but they themselves are at least partly responsible for the current (degraded) state of the environment. More specifically, I analyze whether people are more willing to contribute to an environmental good if their own role in the environmental degradation process is emphasized. I do so by eliciting contributions to a reforestation program among farmers in an environmentally

valuable area, the Bale Eco-region in Ethiopia, by emphasizing that one of the activities that they engage in, logging, is one of the main causes of local forest loss.

Research on people's preferences and environmental valuations is often difficult because there is no direct relationship between people's preferences and their environmental behavior – people may have a strong preference for the environment but still decide not to undertake environmentally friendly actions. The difference between preferences and behavior may be the result of the environment being a public good. An individual engaging in environmentally friendly behavior incurs costs while her private benefits of the improved environmental outcome are typically small. Revealed preference techniques may thus not always be applicable, but unfortunately, survey methods to elicit valuation, the so-called stated preferences techniques, are not without problems either. Hypothetical bias is one of the most important problems with stated preferences valuation techniques. If asked to value an item, people tend to overstate their true willingness to pay if they think that they will not actually be forced to financially contribute.

To mitigate this issue, I decided to financially incentivize farmers' decision by (i) endowing them with a budget that is, in principle, theirs to keep, and (ii) subsequently asking them how much of their budget they are willing to invest in a local reforestation project. Asking farmers for their financial contribution implies that farmers will think carefully about their decision of how much to contribute. Step (ii) is implemented under three different conditions (using a between-subjects design). The first condition is one in which respondents receive a full account regarding forest-related activities. The scenario in this condition provides information on the efforts of other countries in the region to protect the forest, and it also emphasizes the fact that small-scale logging is one of the main causes of local forest degradation. Information on the effort other countries undertake to protect the forest is left out in the scenario of the second condition. The difference in contributions between the first and the second condition allows me to infer whether efforts by others tend to result in a higher propensity to contribute (as the good example by others crowds in contributions) or whether this tends to invite more free-riding (as the perceived necessity to contribute oneself too may be lower if others already engage in forest conservation activities). Compared to that of the first condition, the scenario in the third condition leaves omits the emphasis of the role of logging by farmers as an important cause of

forest loss which, in turn, causes desertification in the region. Comparing the outcomes of the first and the third condition allows me to infer whether an increased emphasis on one's own personal role in the environmental degradation process tends to result in higher contributions, or not. I find that emphasizing the role of others does not affect contributions, while explicitly pointing out the (negative) role the respondents play actually increases contributions.

Asking respondents to make actual contributions in a public good setting induces respondents to think more carefully about the problem they are confronted with, but it does so at the expense of underestimating the farmers' true valuation of the forest. After all, the costs of contributing are private while the benefits accrue to all, and hence true willingness to pay (for example elicited in a binding referendum format) will likely be higher than observed willingness to pay. However, under the plausible assumption that the extent to which hypothetical versus factual payments affect farmers' WTP levels is the same in all three conditions, my study provides a careful test of the "responsibility effect" on willingness to pay – the fact that I find differences in farmers' contributions between the various treatment arms indicates that also the farmers' true valuation will vary between the three treatment arms.

The setup of my paper is as follows. In section 2, I discuss the issue of hypothetical bias in stated preference valuation techniques, and how I dealt with this issue in this study. In section 3, I present the study's hypotheses and experimental design. Section 4 includes the results of the experiment and further analysis using different tools. Finally, section 5 presents the conclusions.

2.2. Valuation of Environmental Goods and Services

Environmental valuation is an important issue as the quantity and quality of nature and the environment directly affect people's welfare – and especially the welfare of those people who are directly dependent on these natural resources (Dean & Hoeller, 1991). Nevertheless, environmental valuation is not straightforward owing to the typical nature of the public goods. Pure public goods have two characteristics. First, they are non-excludable – people cannot be excluded from the benefits these goods provide, not even if they themselves did not contribute to their provision. And second, their consumption is non-rival – one person's consumption of the public good does not affect the extent to which others can benefit from it. Markets can provide information on people's valuation of private goods, but the characteristics of non-excludability

and non-rivalry in consumption implies that there are no naturally occurring markets for public goods (Carson, Flores, & Meade, 2001). Artificial markets for public goods, however, can be developed by using different valuation methods.

One such valuation method is the contingent valuation method (CVM), which was developed by Ciriacy-Wantrup (1947). CVM is, in essence, a survey method in which the respondent is provided with a description of a hypothetical public good provision program, like a bird protection project or an oil spill prevention program. The respondents are given detailed information on the benefits that the program will provide – the type of birds targeted, how they look like, their importance for maintaining the integrity of the ecosystem, etc. The scenario also specifies the increase in population size (or prevention of their decline) the program is expected to realize. After having provided this information, the respondent's valuation of the project is elicited – either by simply asking what the maximum amount of money is that she is willing to pay for the project to be implemented (so-called "open-ended bid elicitation"), or by asking the respondent whether she would be willing to pay a specific amount of money for the project's implementation yes or no. The second type of question is often framed as a referendum ("if the project would require the imposition of a tax of $x, would you vote in favor of the project, yes or no?") and is typically referred to as the dichotomous choice valuation approach (Adamowicz, Boxall, Williams, & Louviere, 1998; Mitchell & Carson, 1989). The demand function for the public good is then obtained by varying the amount to be paid – the higher the amount stated the lower the share of respondents who indicate that they would be willing to pay that amount.

Since its first application (Robert, 1963), the dichotomous choice valuation technique has become increasingly more popular (compared to the open-ended valuation approach); see (Freeman III et al., 2014; Haab et al., 2013; Oerlemans et al., 2016). The reason is that theoretically, the dichotomous choice approach is incentive compatible, in the sense that there is little reason to strategically misrepresent one's preferences (by saying "yes" to a price offer that is above one's true value or "no" to an offer that is below one's true value). This is not necessarily the case in the open-ended versions, where respondents may strategically (grossly) under- or overstate their willingness to pay depending on whether they think that they will actually be forced to contribute the stated amount, or not implementation (Cummings, Elliott, Harrison, & Murphy, 1997; Taylor, 1998).

However, despite the mentioned advantage of dichotomous choice mechanism, experimental studies have found some unresolved issues with the mechanism. One issue is a disparity in valuation results between the hypothetical referendum and the real referendum (Cummings & Taylor, 1999; J. A. Hausman, 2012). It is often the case that in CV, the WTP elicited tend to be higher than in situations where the yes/no question has real consequences (with all respondents being forced to pay and the project being implemented if the majority votes in favor). Two of the main causes for this upward bias is that the hypothetical nature of the method invites socially desirable answers, while respondents may also fail to pay enough attention to the budget consequences of their answer (if the project had not been hypothetical).

To address this issue, I decided to financially incentivize farmers' decision to contribute to the public good – a reforestation project in their local forest. Whereas this decreases the farmers' propensity to provide socially desirable answers, it does so at the cost of underestimating their true willingness to pay. This can be seen as follows.

Let q_i denote community member's contribution to the reforestation project – the number of trees she decides to have planted on her behalf. If there are *n* community members, the number of trees planted is $Q = \sum_{i=1}^{n} q_i$. From the community's perspective there are local benefits to having more trees. Let us denote the local benefits accruing to community member *i* (improved soil protection, improved retaining of groundwater, etc.) with $B_i\ (Q)$. Denoting community member *i*'s budget for tree planting with E_i and the (constant) costs of financing planting a tree with c_i, community member *i*'s welfare associated with planting *Q* trees is equal to

$$w_i = E_i - c_i q_i + B_i\ (\sum_{i=1}^{n} q_i), \tag{1}$$

and the social welfare consequence of the community planting $Q(= \sum_{i=1}^{n} q_i)$ trees is

$$W = \sum_{i=1}^{n} w_i = \sum_{i=1}^{n} (E_i - c_i q_i) + \sum_{i=1}^{n} B_i\ \ (\sum_{i=1}^{n} q_i). \tag{2}$$

Maximizing (2), the socially optimal number of trees planted by each community member is implicitly defined by

$$\frac{\partial W}{\partial q_i} = -c_i + \sum_{i=1}^{n} B_i' = 0. \tag{3}$$

But if a community member does not attach any value to the benefits of planting trees accruing to his/her fellow community members, he/she maximizes (1), and hence his/her privately optimal number of trees planted is

$$\frac{\partial w_i}{\partial q_i} = -c_i + B_i' = 0. \quad (4)$$

Comparing (3) and (4) and noting that $\sum_{i=1}^{n} B_i' = B_i' + \sum_{j \neq i} B_i' > B_i'$, it is clear that the privately optimal number of trees planted is smaller than the socially optimal number.

For this study, we financially incentivize community members to choose how many trees should be planted on their behalf. Unless all community members are pure altruists, our estimates of the marginal social value of trees are anywhere between B_i' and $\sum_{i=1}^{n} B_i'$, and hence may be a gross /underestimate of the (true) social value. With this approach, we trade off the benefits of a well thought-through financially incentivized decision at the cost of underestimating the true value. However, as we are interested in the treatment differences rather than in the levels themselves, we choose to financially incentivize private decision-making. Therefore, we do not estimate the true social value of trees. But we argue that if the framing affects the private decisions in a specific way, the social values are likely to vary similarly.

2.3. Context and Hypotheses

I hypothesize that the information about the efforts taken by others to promote forest conservation and reduce desertification will not affect respondents' valuation of conservation activities (and also not their contributions), but that reminding them of their own role in the deforestation process will result in increased contributions. Previous studies have found that including the human-caused environmental damage information will increase WTP (Bulte et al., 2005) and (Carson et al., 2003). More specifically, Bulte et al. (2005) studied how different causes of environmental degradation (human vs. natural) affect WTP values. They found that people state a higher WTP when the cause of environmental damage is human activity. They attributed this difference to an "outrage effect" –people contribute more if they think the damage to the environment is caused by human activity because this makes them feel upset. Alternatively, the results may be due to a "responsibility effect." People are willing to pay more

if they think they themselves are (partly) responsible for the observed degradation (Brown, Peterson, Marc Brodersen, Ford, & Bell, 2005; Walker, Morera, Vining, & Orland, 1999).

In this chapter, I test whether such a "responsibility effect" also exists among farmers in Ethiopia's Bale Eco-region (see below for more information). I do so using a financially-incentivized experiment that elicited WTP for a public good, afforestation. Decisions thus have real financial contributions. Respondents receive an endowment of 50 ETB (which is only slightly less than a full day's wage for unskilled labor). Respondents can pocket the money, but they can also spend it purchasing trees. Any tree purchased will be planted on their behalf. Having a tree planted on one's behalf costs10 ETB. The contribution decision is about the number of trees planted on one's behalf – any integer number between 0 and 5 trees. WTP thus takes six discrete values (0, 10, 20, 30, 40, and 50). I use a between-subjects design, and hence participants make a decision of how much to contribute in one of the three scenarios.

All scenarios (treatments) present the same information on the background about the importance of environmental good provision (deforestation and desertification), the good provided (afforestation), the mode of payment and budget constraint, and finally, the question of WTP elicitation. Hence, all the surveys have the same background, good, and payment mechanism but differ in the information used as treatments in the experiment. The three treatments are the following. The baseline treatment offers a scenario that, in addition to the information above, describes the efforts that neighboring countries are undertaking to prevent the problem, and it also explicitly draws attention to the role logging plays in the process of desertification and forest loss. Our respondents are small-scale farmers who engage in logging activities, and hence this scenario reminds them of their own responsibility in the process as well as of the activities of others to mitigate the problem. I call this the *combined* treatment. Compared to this first scenario, the second treatment condition omits the information about the respondents' own responsibility, and hence only provides information on the conservation efforts by others. I call this treatment the *effort elsewhere only* treatment. Compared to the scenario in the first treatment, the third condition omits the effort elsewhere content and hence provides only the information on the role of logging in the deforestation and desertification process. I call this scenario the *human-caused* treatment. Comparing contributions in the first and the second treatment provides insights about whether information about efforts of other countries crowds in

or crowds out contributions by our respondents. And comparing contributions in the first and the third treatment isolates the "responsibility effect".

The scripts are as follows. The general information on the deforestation was as follows:

"Desertification is the advance of deserts because the tree and plant cover that bind the soil is removed. It occurs when trees and bushes are stripped away for fuelwood and timber, or to clear land for cultivation. Desertification is a global issue, with serious implications worldwide for nature, wildlife, and agriculture. Some 50 million people in Ethiopia may be displaced within the next 10 years as a result of desertification."

And the willingness to pay question was framed as follows:

"One effective mitigating measure is planting trees to change the non-forest land to forest and prevent deserts from expanding.

Consider the benefits of planting trees in this region. Of the 50Birr you just received, how much do you wish to contribute to planting trees? For every 10Birr, we can plant 1 tree.

I am willing to contribute _____ birr."

The script regarding the role of small-scale logging activities was as follows:

"There are different factors that are increasing deforestation. One important factor is illegal logging by different parties. The uncontrolled cutting of trees will eventually change the forest land to non-forest land. This will aid the gradual changing of the land to desert and to an unfavorable climate. The trees that are cut down by illegal loggers are used as fuel for cooking as well as being sold in the market to be used for furniture and construction purposes."

This script was included in the *human-caused only* and in the *combined* treatments, but not in the *effort elsewhere only* treatment.

The script regarding the efforts of other countries to prevent deforestation and desertification was as follows:

> *"Tanzania, Kenya, and Uganda have united their efforts to combat illegal timber trade in East Africa to decrease deforestation. These countries recognize that illegal logging must be mitigated and forests managed sustainably, in order to reduce emissions from forest loss. As such, a key goal of the initiative is to curb illegal logging and trade in East Africa as a way to address deforestation and subsequently reduce emissions from forests.*
>
> *Even though there are many international initiatives to curb deforestation, recent reports show that global efforts to curb deforestation are insufficient, as forests are cleared faster than ever for agribusiness, timber, and other land development schemes. However, there was an important call made for a change in policy to deal with the problem."*

This script was included in the *effort-elsewhere* and in the *combined* treatments, but not in the *human-caused only* treatment.

I now explicitly state the hypotheses that will be tested in this study:

Hypothesis 1: Drawing respondents' attention to the role of illegal logging in the process of deforestation and desertification increases their contributions to the reforestation project offered. Average contributions are higher in the *combined* treatment than in the *effort elsewhere only* treatment.

Hypothesis 2: Informing respondents that other countries recognize the role of illegal logging and actively try to discourage it induces respondents to raise their contributions to the reforestation project. Average contributions are higher in the *combined* treatment than in the *human-caused only* treatment.

2.4. Field, Randomization, Recruitment, and Experimental Procedure

The context of the study is the Bale Mountains Eco-region in Ethiopia. The Bale Mountains Eco-region is the second largest standing moist tropical forest in Ethiopia (Defries et al., 2002). The Afro-alpine region provides habitat for numerous endemic species, marking the region as one of the 34 globally recognized biodiversity hotspots (Williams et al., 2005). More than 12 million people depend on the water that originates from the mountains. The dry lowlands of the east and southeast of Ethiopia (including neighboring Somalia and parts of Northern Kenya) get their perennial water only from water that springs from the mountains in the Eco-region. This region was selected as a study area for three reasons. First, it is of considerable economic importance for Ethiopia –its direct consumptive use value alone was estimated to be in billions of dollars per year (Watson, 2007). Second, it is a priority forest area selected for conservation, in light of its importance for neighboring countries and the surrounding communities. Finally, it covers the largest area of Afro-alpine forests in the African continent (100,000ha) and is registered as a world heritage area by UNESCO.

The sample in this study is taken from Dodola "Woreda" (the lower administration level next to regional administration), out of which three villages were selected: Bura-Adelle, Kechema, and Geneta (see Figure 1). These villages were selected because they were among the first to implement forest management in the Bale Eco-region, and they are more accessible in terms of infrastructure (see also Chapter 4).

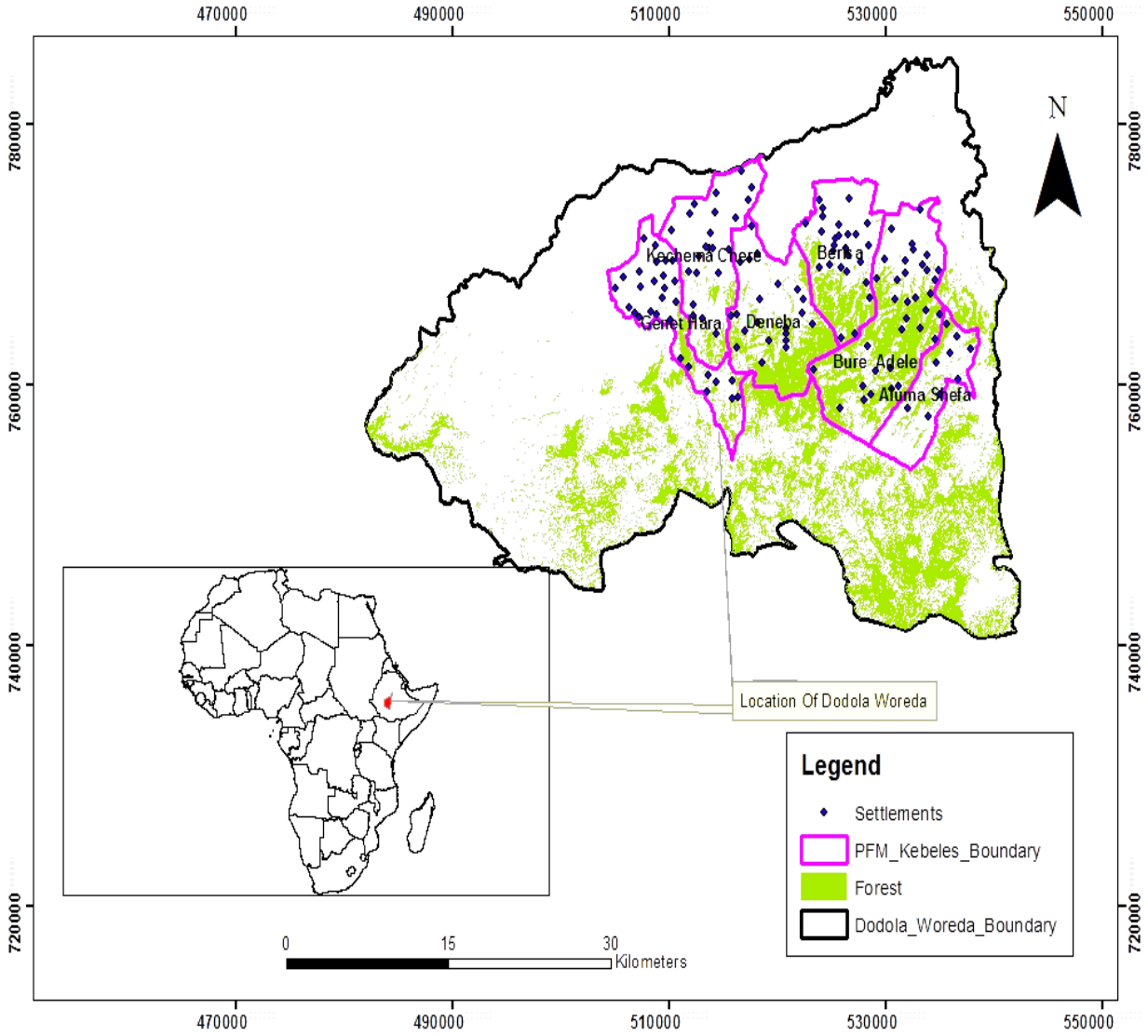


Figure 2.1: Map of the study area.

The experiment was implemented in January 2016 in the three villages. Every treatment was implemented in each of the three villages, and respondents participated in just one treatment. In total 96 individuals participated in this study. The subjects were invited via the village-level administration agents to come to the meeting places. Inviting subjects to the meeting via village administration agents is not unusual in the study area. Village-level meetings are a common occurrence in which various issues are discussed on a regular basis. Hence, it is unlikely that the village administration's assigned person would disseminate systematically biased information to

recruit certain types of individuals only.[1] Hence, the subjects in this study are likely to be a fairly representative sample of the people living in the three villages.

Upon arrival of the subjects to the meeting place, the experimenters gave a brief explanation of the research project and the researcher's background. Next, subjects undertook two tasks in the field. The first task was filling out a general survey which was administered to collect the background information on the subjects. Experimenters collected this information individually from the subjects in the form of an interview.

The second task was the implementation of the WTP elicitation experiment. Subjects were assigned to one of three treatment groups. Note that the within-village treatment allocation helps mitigate concerns of unobserved heterogeneity affecting treatment outcomes. Next, research assistants read out the script aloud to each treatment group. Reading of scripts to each treatment group was done such that participants in the one group were not able to overhear what was being said in another group. Furthermore, the subjects made the decision individually after being approached by the experimenters in the form of an interview. Finally, based on their decision the money was immediately collected, and the trees were planted seven months later, i.e. July 2016).

2.5. Results

2.5.1. Sample and Descriptive Statistics

Table 2.1 shows the summary statistics of responder characteristics in the three treatments, as well as the outcomes of the relevant balance tests. The subject pools are found to differ in some respects. In the combined treatment the share of male participants is lower than in the other two treatments. However, it should be noted that differences in female participants across treatments are small in magnitude. For example, the third treatment group contains only 3 women more compared to the other two treatments. Similarly, the membership in local collaborative forest management (CFM) groups differ somewhat across the treatments. The subjects of this experiment are also people who care about the environment and take the

[1] This is also because, given that the government already initiated a "5 households in one group" culture of working together, information disseminates very fast. Thus, it is less probable that certain households would be systematically sent to attend a meeting and others excluded, as this might damage the future relationships in the villages.

seriousness of environmental degradation into consideration. This opinion does not differ across treatments, as can be seen from the variable Opinion on climate change.

Table 2.1. Participants' characteristics by treatment group

	Human-caused only	Effort elsewhere only	Combination	p-value
Income	3506.284	3504.650	3807.793	0.899
	(423.767)	(529.497)	(547.556)	
Land Size	2.268	2.581	2.544	0.674
	(0.293)	(0.252)	(0.273)	
Age (>25)	1.000	0.947	1.000	0.215
	(0.000)	(0.037)	(0.000)	
Education (1-5)	0.677	0.447	0.519	0.159
	(0.085)	(0.082)	(0.098)	
Family size (>5)	0.742	0.658	0.667	0.734
	(0.080)	(0.078)	(0.092)	
Male	0.935	0.974	0.778	0.022
	(0.045)	(0.026)	(0.082)	
CFM member	0.774	0.526	0.667	0.099
	(0.076)	(0.082)	(0.092)	
Opinion on climate change	2.871	2.895	2.889	0.971
	(0.077)	(0.063)	(0.082)	
N	31	38	27	

Standard errors in parentheses. Orthogonality outcomes are based on F-tests on the variable distributions across the three treatment arms.

2.5.2. Experimental Results

Table 2.2 presents the mean contribution (or WTP) as well as results of pairwise comparison tests of the mean contributions across the three treatment groups. Subjects in the *human-caused only* treatment have the highest mean WTP value (19.03ETB), while those in *effort-elsewhere* treatment have the lowest mean WTP value of 10.78ETB. The mean WTP value of *combination* treatment is 18.14ETB.

Table 2.2. WTP by treatment group

Summary of WTP by treatment		Difference test	
Treatments	Mean WTP	Comparison between treatments	p-values
Combined	18.14 (11.38)		
Human-caused only	19.03 (14.78)	Human-caused only vs. Combined	0.457
Effort-elsewhere	10.78 (6.27)	Effort-elsewhere vs. Combination	0.0041

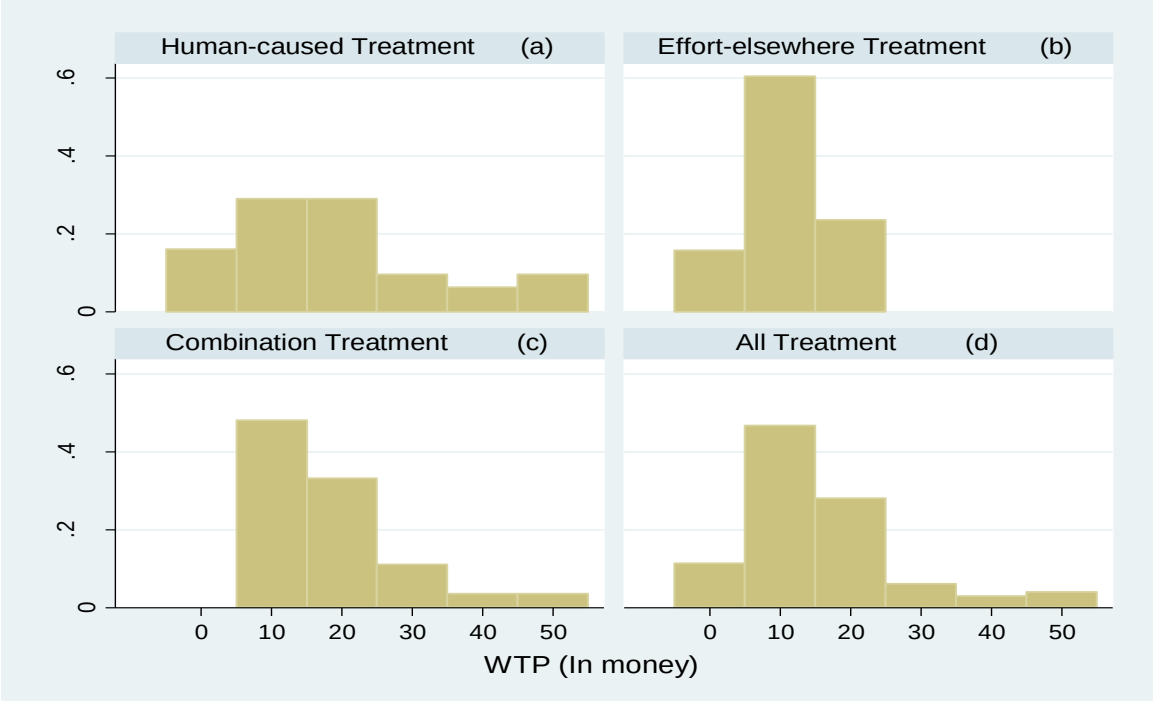
Standard deviation in parentheses.

The Kruskal-Wallis overall difference test indicates a statistically significant difference between the three treatment groups ($p = 0.0097$). The difference in means across treatments is tested using Mann-Whitney U tests. I find that mean contributions are significantly lower in the *effort elsewhere only* treatment than in the *combined* treatment ($p = 0.0041$). I thus find support for Hypothesis 1: compared to just being informed of efforts undertaken in other countries, additional information on the role of illegal logging in the region results in a significant and substantial increase in contributions. Next, contributions are not significantly affected by information on the efforts of neighboring countries to combat the problem. Average contributions are slightly higher in the *human-caused only* than in the *combined* treatment (albeit

not significantly so, as the p-value is 0.457). This suggests that, if anything, information on efforts elsewhere tend to crowd out (rather than crowd in) contributions, which is not in line with Hypothesis 2.

The distribution of WTP across the treatment groups can be seen from Figure 2.2, which presents the histograms of WTP by treatment and for all treatments together (Figure 2.2d). As noted before, the WTP is a discrete variable taking values within the range of 0 to 50 ETB, with step size 10. The overall distribution of subjects' WTP shows a right-skewed distribution (Figure 1d) similar to other WTP studies (Green, Jacowitz, Kahneman, & McFadden, 1998; Gunatilake & Tachiri, 2014; Kanninen, 2007; Martín-Fernández et al., 2014). Some participants have WTPs below 10 ETB (including zero). The majority of subjects' WTP values lie within the interval between 10 and 20 ETB. The distribution of WTP values, however, differs across the treatment groups.

Figure 2.2. Histograms of WTPs by Treatment.



In *human-caused only* treatment (panel a), the WTP shows more variation across the discrete values. The shape of WTP distribution for this treatment group is also rather less

skewed. On the other hand, the distribution of WTP values in the *effort-elsewhere only* treatment is less varied. The shape of the WTP distribution in this treatment is quite unimodal. Hence, the WTP of most participants in this treatment is very close to the others (about 60 percent have a WTP value of 10ETB). Finally, the distribution in the *combination* treatment indicates some variation. About 50 percent of subjects have WTP within the range of 10 to 20 ETB, with a positive distribution. In general, the histograms suggest that the WTP values vary more in the *human-caused only* treatment and the *combination* treatment compared to the treatment of *effort-elsewhere only*.

2.5.3. Econometric Analysis

The observed treatment differences are also explored using regression analysis, which allows for conditioning on covariates, in order to control for concerns about the impact of possible differences in the subject pools. Utilizing regression will furthermore help us test the construct validity of our CV surveys. To take into consideration the discrete nature of the dependent variable, the model is estimated using interval as well as ordered probit regression techniques. The ordered probit regression in this study serves as a robustness check given the weak normality of the dependent variable (revealed by the Shapiro-Wilk test), which is assumed by interval regression. The regression equation is specified by equation (5):

$$WTP_{ij} = \beta_0 + \beta_1 \, TrHumanCauseOnly_{ij} + \beta_2 \, EffortElsewhereOnly_{ij} + \beta_3 \, X_{ij} + \varepsilon_{ij.} \qquad (5)$$

WTP values are regressed on treatment variables to extract treatment effects on individual *i* in village *j*, which are (β_1, β_2). β_0 captures the average contribution in the *combination* treatment – the omitted category. The baseline treatment in our case is the *effort-elsewhere only* treatment. Finally, β_3 captures the subject-specific characteristics, such as the subjects' age, educational status, gender, income, land size, family size, and membership of environmental conservation groups.

Table 2.3 shows the factors that influence contributions to the reforestation project. All specifications include village fixed effects. The omitted category of the treatment indicators is the *combined* treatment; the coefficients on *human-caused only and effort elsewhere only* treatment dummies thus indicate the impact of omitting "effort elsewhere" and "own

responsibility" information, respectively. Columns 1 and 2 of the table indicate the treatment effects without including other explanatory variables using OLS and interval regression, respectively. Consistent with the non-parametric tests presented in Table 2.2, I find that omitting information on effort elsewhere does not affect outcomes (as the coefficient on *human-caused only* is not significantly different from zero), but that the responsibility effect is substantial (as the coefficient on the *effort elsewhere only* treatment dummy is negative and significantly different from zero).

Controlling for participants' characteristics (columns 3 and 4), under both interval and ordered probit regressions, does not really affect the above estimated coefficients. However, the explanatory variables can be utilized as a test of construct validity. Construct validity is typically tested to examine whether or not the CVM captures preferences of people in the valuation (by looking at whether the correlation of economic variables such as cost and income with WTP value is as expected in standard economic principles). In Table 2.3, the economic variable, income of participants, seems to predict WTP values consistent with the standard expectation – that is, the higher the income, the higher the WTP values, and this relationship is significantly different from zero.

The regressions also indicate other participant-specific predictors of the WTP values. For instance, the older participants are more likely to have lower WTP values compared to the younger ones (less than 25 years of age), and the same holds for below-average educated participants (although not significantly so).

Another interesting point is that being engaged in local collaborative forest conservation shows a positive correlation with WTP. In the study area, it is possible to engage in forest conservation with a group called a collaborative forest management group (CFM). This is a local conservation group that looks after the surrounding forest.

Hence, the positive correlation sign of CFM membership and WTP in the regression is expected since the members are contributors to the public good. I also ran regressions testing whether the treatment effects differ between CFM members and non-CFM members. None of these interaction effects are found to be different from zero, and hence they are not included in this table.

Table 2.3: Factors affecting contributions to the reforestation project.

	(1) WTP (OLS)	(2) WTP (Interval regression)	(3) WTP (Interval regression)	(4) WTP (Ordered probit)
Human-cause only treatment	1.028	0.829	0.454	-0.107
	(2.712)	(2.729)	(2.511)	(0.310)
Effort-elsewhere only treatment	-7.044***	-7.646***	-7.081***	-1.086***
	(1.932)	(2.053)	(2.071)	(0.287)
Gender			-0.886	-0.355
			(2.260)	(0.348)
Education (1-5 yrs)			0.977	0.294
			(1.935)	(0.256)
Age (>25 yrs)			-10.92***	-2.374***
			(3.067)	(0.713)
Income			0.000847**	0.000114**
			(0.000373)	(0.0000509)
Land Size			-0.495	-0.0793
			(0.904)	(0.123)
CFM member			4.418**	0.591**
			(2.081)	(0.287)
Village FE	YES	YES	YES	YES
Constant	9.921***	4.315**	10.75**	
	(1.599)	(1.677)	(4.331)	
Lnsigma		2.166***	2.090***	
Constant		(0.112)	(0.116)	

Observations	96	96	96	96
Adjusted/Pseudo R^2 /Log pseudolikelihood	0.379	-120.523	-114.417	0.3090

Robust standard errors in parentheses * *p < 0.10,* ** *p < 0.05,* *** *p < 0.01.*

Note: Column 1 presents OLS with the dependent variable WTP. Columns 2 and column 3present the interval regressions. The additional variables were included in column 4, which presents ordered probit regression of WTP on treatments with the additional explanatory variables.

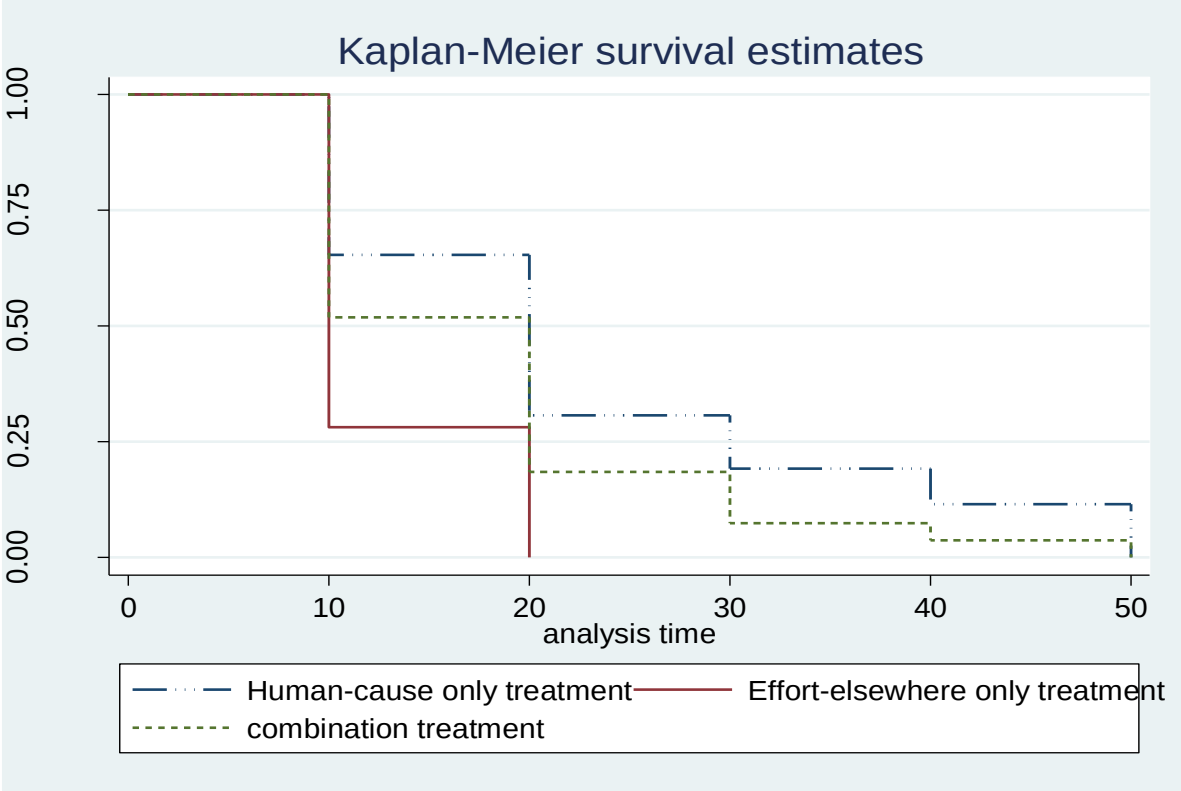
2.6. Aggregate WTPs and Robustness Checks

Reforestation provides a public good, and hence total willingness to pay is the sum of individual willingness to pays. I estimate the implications of the treatments for total willingness to pay using survival functions. Setting WTP responses as a survival function means that, instead of the original notion of "time," survival is defined by all the possible amounts (payments) that the respondents can contribute to the project. A respondent with positive willingness to pay "survives" that amount and a respondent with no willingness to pay "fails" that amount. Here, the log likelihood function is calculated by the difference in WTP densities evaluated at contributions of 0, 10, 20, 30, 40 and 50 ETB. The likelihood function can then be maximized based on the selected parametric distribution (shape) such as in a standard Kaplan-Meier and Weibull estimator. Setting data into a survival function format mitigate the discrete nature of the WTP values (predicts the probability that *true values* are within the discrete values). A further advantage of utilizing this function is that the survival analysis is in line with the assumption of the key economic theory that the cost for the fraction of participants with positive WTP decreases monotonically (2003).

Hence, in this study, the Kaplan-Meier survival curve is used to present the summary measure of people's WTP under the three treatments (Figure 2.3). Given the right-skewed nature of WTP – that is, there seem to be individuals who are not willing to contribute – mean summary for welfare analysis might not be the correct representation. Hence, the demand for the environmental good under the three treatments is compared with the 50^{th} quintile of the graph. As can be seen from Figure 2.3, the human-caused only treatment appears to have a higher survival rate than the other two treatments. Looking at the median survival time, which is the

probability of survival at 0.5, the effort elsewhere only treatment appears to give 10 ETB versus about 20 ETB in the other two treatments.

Figure 2.3 Survival function estimate of WTP across the treatments



2.6.1. Robustness Check: Estimators of WTP

I assess the role of covariates in the survival analysis using Weibull regression. In this model, the hazard measures risks faced by respondents in terms of failure (not paying). Accordingly, a higher hazard rate was associated with lower WTP values. In Weibull regression, since the reported coefficients of covariates are in the form of exp (β_i), interpretation of the hazard rate requires transforming the coefficient to exp (β_i) -1. Weibull regression results are reported in Table 2.4, which shows hazard increasing over the cost values (a positive sign of Weibull parameter ρ=2.90). That is, an increase in value by 10 ETB increases the likelihood of not leaving the lower WTP interval. As the values of WTP increase, the participants are less likely to pay more.

Table 2.4. Weibull regression

	WTP (Hazard ratio reported)
Education (1-5yrs)	0.790
	(0.194)
Age (>25yrs)	4.806**
	(3.744)
Income	1.000**
	(0.0000441)
Human-cause only treatment	0.627
	(0.191)
Efforts-elsewhere only treatment	3.094***
	(0.946)
CFM member	0.639*
	(0.169)
Village FE	YES

N	85
Weibull parameter (ρ)	2.90
	(0.235)

Exponentiated coefficients; Standard errors in parentheses * *p < 0.10,* ** *p < 0.05,* *** *p < 0.01*

Table 2.4 shows and confirms that several explanatory variables are significant predictors of WTP decisions. For instance, being an older participant increases the hazard rate by more than 4 times over being a younger participant. That is, older participants are 4 times more likely not to leave the lower interval of WTP values. Being a CFM member decreases hazard by 36% (0.627-1). Thus, a member is 36% less likely to stay in the lower interval, which indicates that CFM members have higher WTP than non-CFM members. Table 2.4 also shows that one ETB increase in income results in a zero hazard rate (1-1). This is to say, for one ETB increase, the hazard rate will stay constant. Hence, the economic variable seems to predict the WTP decision.

Furthermore, the role of treatments as shown by the Weibull regression similar to the results in the main finding mentioned before. In Table 2.4, the treatment coefficients show that being in the treatment group of *effort-elsewhere only* increases the hazard rate by more than 3 times compared to the baseline treatment (i.e., *combination*). That is, being offered the *effort-elsewhere only* scenario decreases WTP. In general, despite the assumption of a specific shape parameter in the Weibull regression, the results in this regression are consistent with the main findings (Table 2.3).

2.7. Conclusions

The contingent valuation method was designed to elicit preferences for environmental goods for which conventional markets are unavailable (Carson et al., 2003; Hanemann, 1994). By allowing the attachment of monetary values to environmental goods, CV creates conventional market-like decision-making. In this study, we see evidence for this claim – that is, the creation of market-like behavior by CV – from the preferences of the subjects. Specifically, the relationship between the predictors and WTP values shows evidence of the subjects' preference for environmental goods. First, the economic variables seem to be in line with the standard economic theory that the higher the income the higher the WTP of participants. Second, other

predictors such as being a cooperative member of a local forest conservation seem to be correlated with higher WTP values.

This study tests the hypothesis of whether drawing attention to owns involvement of human-caused environmental damages increase the WTP estimates. I do so by offering respondents in Ethiopia's Bale Eco-region the opportunity to contribute to a reforestation project, using three different scenarios. All scenarios describe the issue of deforestation and desertification that is affecting the region. In one scenario additional information is provided that illegal logging plays a major role in this process, in another additional information is provided about the efforts other countries are undertaking to mitigate this problem of illegal logging; the third scenario offers both these types of information. Next I analyze how the contributions to the reforestation projects differ between the three different scenarios. This approach is akin to the contingent valuation method, which was designed to elicit preferences for environmental goods (Carson et al., 2003; Hanemann, 1994). My approach differs from this method by asking respondents to make real financial contributions. Asking for real contributions makes decisions consequential and makes sure that respondents will think hard about how much they are willing to provide, and hence mitigates the effect of providing socially desired answers. Indeed, economic variables seem to predict contributions in a way that is in line with the standard economic theory; the higher the income the higher contributions made by the participants. Also, other predictors such as being a cooperative member of a local forest conservation seem to be correlated with higher contributions.

Extant literature notes that including information on human-caused environmental damage in contingent valuation surveys increases the WTP values. This was, however, attributed to outrage effect – that is, because respondents are upset, they contribute more to environmental goods. In a somewhat different setting this study finds evidence that contributions to a reforestation project by respondents who are implicated in the process of environmental degradation are not affected by information on efforts of others to mitigate the problem, but also that their contributions increase significantly and substantially if attention is drawn to their own responsibility in the deforestation and desertification process.

The responsibility effect increases contributions for two reasons. First, the sample pool

of this study consists of participants who potentially engage in human-caused damage. In this case, the responsibility effect can be reinforced by including information on human-caused damage. Second, the majority of participants have a strong belief that the current environmental damage is a serious problem in terms of the near future consequence (as measured in the survey question).

Chapter 3:

Can Uniform Price Auctions inform the design of Payments for Ecosystem Services schemes?

Evidence from the lab and field[2]

3.1. Introduction

Conservation payments have been advocated as both an effective and efficient means of protecting the planet's most valuable natural resources – especially if the payments are made conditional on the actual delivery of environmental outcomes above and beyond what would otherwise have materialized (Wunder, 2007; Wunder, Engel, & Pagiola, 2008). These conditional payments, typically referred to as Payments for Ecosystem Services (PES), have been implemented in developed and developing countries alike; prominent examples include the Conservation Reserve Program in the United States (Wu & Babcock, 1995) and the *Pago por Servicios Ambientales* Program in Costa Rica (Pattanayak, Wunder, & Ferraro, 2010b). The rationale behind PES is that without compensation, the resource owners incur the costs of conserving natural resources while they typically reap only a small share of the conservation

[2] This chapter is based on joint work with Daan van Soest, Ty Turley, Paul Christian, Eline van der Heijden.

benefits. That means that while the societal benefits of conservation typically exceed the costs, economic decision making is biased against conservation and towards resource degradation. Offering financial compensation, conditional on environmental service delivery, is thus a means of changing the resource owner's cost-benefit evaluation outcome in favor of conservation (Pattanayak, Wunder, & Ferraro, 2010a).

Typically, conservation contracts are offered take-it-or-leave-it style, with the contracts specifying what services the resource owner needs to deliver as well as the amount of money she will receive in compensation if the contractual requirements are met. One of the key challenges in the design of Payments for Ecosystem Services schemes is to find the optimal compensation price. Offering a higher price results in a larger share of the resource owners agreeing to participate in the PES scheme, but at the expense of the amount of rents earned by the inframarginal resource owners (Engel, Pagiola, & Wunder, 2008). That means that the amount of environmental services obtained with a fixed budget are a hump-shaped function of the price offered, and hence finding the optimal price is a key challenge for any PES scheme.

If the slope and location of the aggregate conservation cost schedule are known, the conservation agency (the government, or an NGO) can determine the optimal price to be offered. For example, if a large share of the resource owners can provide the conservation services at quite low cost while conservation is very expensive for the remaining ones, setting the price equal to the cost level of the most expensive among the low-cost farmers can result in high take-up rates while limiting the excess compensation received by the infra-marginal resource owners. To implement this, three steps need to be taken: information should be collected on the location and slope of the aggregate conservation cost schedule, the optimal price should be determined given the available budget, and then conservation contracts can then be offered, take-it-or-leave-it style, to the resource owners.

This chapter focuses only on the first step of the process – identification of the location and slope of the conservation cost schedule. Resource owners differ in the (opportunity) costs they incur when supplying environmental protection, and typically these owners have better information about their private conservation costs than the potential buyer. Two potential solutions have been proposed to overcome this information asymmetry problem in the literature. The first is to offer different take-it-or-leave-it compensation prices to different groups of randomly selected resource owners (see for example (Kaczan & Swallow, 2013), (Swallow et al.,

2009)). Different price levels result in different sign-up rates, and hence this approach allows the conservation buyer to trace the conservation supply schedule so that she can identify the price that minimizes average conservation costs. This approach is incentive compatible, but it is both expensive and cumbersome because the sample size needs to be fairly large for each price offered to have a sufficiently precise estimate of the take-up rates associated with that price, and possibly quite many prices need to be tested to approximate the aggregate conservation supply schedule.

A second approach has been suggested by Jack, Leimona, and Ferraro (2009) and (Ajayi, Jack, & Leimona, 2012), and that is to use incentive-compatible valuation elicitation methods to uncover the levels and distribution of the conservation costs of a group of randomly selected resource owners. One candidate incentive-compatible valuation elicitation method is the (reverse) sealed-bid Uniform Price Auction (UPA), where potential service sellers are asked to indicate the minimum amount of money they need to receive to be willing to participate in the PES scheme. The potential sellers are informed that if their "ask" turns out to be below a predetermined (but undisclosed) "strike price" they are accepted into the program and will subsequently be paid the *predetermined strike price* if they meet the environmental requirements. If the amount they ask is above the predetermined price, they will not be offered the contract.

Submitting one's true opportunity costs is a dominant strategy in this set up (Krishna, 2009; Vickrey, 1961). Overasking does not yield any benefits if asking more than one's true opportunity costs still results in one being admitted to the program. The bidder is paid the predetermined price for her efforts – but this would also have been the case if she would have submitted an ask equal to her true opportunity costs. The bidder will regret having overasked if (i) participating in the program is profitable for her at the predetermined price and (ii) her ask turns out to be so high that she is not admitted to the program. So by overasking the bidder cannot win but may actually jeopardize the chance to earn money. And a similar reasoning implies that submitting an ask below one's true opportunity costs is never a profitable strategy either, because doing so may result in the agent being signed into the program at too low a price for the program to be profitable for her.[3]

[3] The Uniform Price Auction is thus very similar to the Becker-DeGroot-Marschak (BDM) mechanism, where bids are compared to a price that is randomly drawn for each individual

Submitting an ask equal to one's true opportunity costs is thus a dominant strategy in Uniform Price Auctions, and hence, in theory, the UPA approach allows the researcher to obtain precise values of the opportunity costs of resource owners. If resource owners know their conservation costs, the share of resource owners accepting a specific take-it-or-leave-it (TILI) price should be the same as the share of resource owners with asks equal to or below that price level in the UPA. After all, accepting a price offer that is higher than one's opportunity costs is a dominant strategy in take-it-or-leave-it contracts too, and hence this suggests that the UPA approach would be both a more precise and a more efficient way of uncovering the location and slope of the conservation costs schedule than the first approach in which the opportunity cost curve is traced out by offering different prices to many groups of randomly selected resource owners.[4]

Despite the fact that theoretically TILI and UPA should result in the same take-up rate if the same strike price is use, there is some evidence that outcomes can be substantially different. Jack (2013) invited landowners in Malawi to participate in a tree planting project and finds that for a specific take-it-or-leave-it price offer take-up rates are more than twice as high than predicted by the UPA (99% actual uptake versus a predicated uptake of 37.5%). This suggests that UPA outcomes are a poor predictor of actual uptake when the contracts are offered take-it-or-leave-it style. But interestingly she also finds that the actual survival rate is 15% *higher* among the PES participants who had been randomized into the UPA treatment. The fact that UPA underestimates take-up under TILI sheds doubt on the usefulness of UPA in motivating PES design. But the fact that survival rates are higher in UPA than in TILI suggests that *suboptimally many* resource owners decide to participate – not just the ones who have a good chance of meeting the contract specifications to receive payments. This may be wasteful because

(Becker, DeGroot, & Marschak, 1964). With BDM subjects are thus paid different prices for the same service, which was deemed problematic.

[4] A third approach to overcome the asymmetric information problem is to induce competition between potential conservation service providers via selling contracts using get-paid-what-you-ask auctions (Latacz-Lohmann & van der Hamsvoort, 1998). These so-called discriminatory price procurement auctions result in resource owners receiving different amounts of compensation for the same type of conservation services supplied. This ex-post inequality reduces the mechanism's practical acceptability, and indeed discriminatory price procurement auctions are rarely used in PES programs (Engel et al., 2008)

the financial payment is just part of the costs of a PES project – providing the seedlings and the materials for tree maintenance are costly too.

In this paper, we replicate Jack (2013) result that with TILI price offers the take-up rates are significantly higher than when using UPA. We do so in a field-experimental setting very similar to that of her study, as we test for the difference in sign-up rates between TILI and UPA for a tree planting program in Northern Ghana. Above and beyond replicating her result, we also try to identify the mechanism causing this difference.

Perhaps surprisingly, the poor predictive power of UPA has not received much attention. Jack (2013) documents the lack of predictive power but was not able to identify the cause. To the best of our knowledge there is only one other paper that tests the predictive power of incentive-compatible valuation techniques. Berry et al. (2015) explicitly aimed to test whether incentive-compatible valuation mechanisms can indeed predict outcomes of TILI price offers, and constructed a series of experiments to identify the potential cause of any observed difference. Using the standard Becker-DeGroot-Marschak (1964) mechanism they find that households in Northern Ghana are, on average, about 15% more likely to purchase a water filter via TILI prices than via the BDM mechanism. They rule out that the difference in take-up rates is due to either anchoring or strategic considerations.[5]

We complement the work by Berry et al. (2015) in that we try to uncover the mechanism causing the difference in take-up rates. We take a more cognitive (and behaviorally motivated) approach and argue that even if participants in TILI are instructed to carefully think through the consequences of saying yes or no to the price that will be offered to them in a moment – explicitly telling them to carefully think about the minimum amount of money they would need to receive to be willing to participate in the tree planting program – decision making is more careful and deliberate in UPA than in TILI. This mechanism would explain both the higher take-up as well as the lower survival rates in TILI, as documented by Jack (2013). We hypothesize

[5] Both Jack (2013) and Berry et al. (2015) not only tested whether UPA results in different take-up rates than TILI, but they also tested whether there are any consequences for behavior too. Jack (2013) finds that landowners who selected themselves into the tree planting program by means of UPA keep significantly more planted trees alive than those who signed up via TILI, and Berry et al. (2015) find that those households who bought the water filter via UPA use the water filter slightly more intensively than those who bought it via TILI (with the difference being significant at the 10% level only; they fail to detect any significant differences at 10% or better for other use indicators).

that if decision-making is more deliberate, the explanatory power of economic variables (like land area and ease of access to water needed to water the tree saplings) and preferences (like risk attitudes and time preferences) is higher under UPA than under TILI. Our results provide suggestive evidence that indeed decision making is more careful and deliberate under UPA than under TILI. Key variables that should increase a farmer's propensity to participate in the program (such as sizeable land area, easy access to water, and, albeit to a lesser extent, farmer characteristics like risk preferences) are predictive of uptake in the UPA treatment, but not in TILI.

With these field-experimental results in hand, we turn to further test the hypothesis by implementing similar tests in a laboratory experiment. Laboratory experiments have the advantage over field experiments that they allow for less noisy hypothesis testing as the decisions to be made are less complex and because preferences can be elicited with more precision. We endow student subjects with a chocolate bar and subsequently offer them the possibility to sell back their bar using either the TILI or the UPA approach. We not only document that the sell-back rates are not invariant to the approach taken, but also that characteristics that are expected to affect the decision to keep the bar have predictive power in the UPA decisions but not in the TILI outcomes – as was the case in the field experiment.

The set-up of this paper is straightforward. Section 2 presents the design and outcomes of the field experiment offering farmers in the arid Northern part of Ghana the opportunity to participate in a tree planting project, and section 3 does the same for the laboratory experiment implemented using student subjects from Tilburg University, the Netherlands. Section 4 concludes.

3.2. Field experiment

Our field experiment is implemented as a subproject of Ghana's Sustainable Land and Water Management Project (SLWMP). This project aims to improve soil and water conservation in Ghana's arid North. This four-year project, co-financed by the World Bank, is implemented by the Government of Ghana, and consists of different types of interventions including agricultural soil erosion prevention methods such as intercropping of cash crops and leguminous food crops and the construction of bunds on field perimeters. It also includes a tree planting projects to improve soil and water retention, reduce siltation and sedimentation and enhance

biological diversity (especially by providing habitat) and carbon sequestration. Trees thus provide global as well as local benefits, but in our case, they also provide private benefits – the species available for plantation in this project include mango, cashew, and mohagony. Despite the presence of private benefits, voluntary uptake is quite limited – because there are opportunity costs associated with having trees. Saplings need to be planted, but more importantly, to keep them alive they need to be watered in the first two years during the dry season, and from the third year onward the yields of other crops start to decline as the trees start to block sunlight (by providing shade).

Because the number of farmers willing to engage in the SLWMP's voluntary tree planting was too low, the Government of Ghana agreed to experiment with a Payments for Ecosystem Services (PES) scheme where farmers receive financial compensation conditional on the number of trees surviving. The program is scheduled to run for four years (2016-2020), and the payments participants will receive are a declining function of the share of trees surviving. More specifically, farmers would be paid the full price if 75% or more of their trees are still alive at the end of the first year, half of the full price if at least 50% of them survive, and a quarter of the full price if 25% or more are still alive. If less than 25% of the trees are still alive, the farmers are paid nothing.

3.2.1 Design of the field experiment

In our randomized controlled trial (RCT), we implemented the two preference elicitation methods for PES programs, take-it-or-leave-it price offers (TILI) and Uniform Price Auctions (UPA), in six communities. These communities were randomly selected from a set of 80 eligible communities in the country's Northern, Upper East and Upper West regions. The RCT took place in May 2016.[6] The RCT was implemented as follows.

Two days before visiting a community the local extension worker announced our visit to the local chief, and requested that all households having the right to plant trees on their land would be invited to send one household representative above the age of 18 – preferably the

[6] In addition, pilots were run in four communities, which resulted in a substantial shortening of the surveys as well as adding an additional valuation question in the TILI treatment. See below for details.

household head – to the scheduled meeting.[7] Upon arrival at the session, only those household representatives were admitted to the session who were at least 18 years old, and whose households had the power to plant trees on their land. Because of time constraints, the maximum number of participants in each community was set at 48; if more than 48 representatives of individual households were present, a lottery determined who would be admitted to the session. To control for any (observable and non-observable) community characteristics, we randomly assigned half of the community's household representatives to the TILI treatment, and the other half to the UPA treatment, *i.e.* we use a between-subjects design where treatments were randomized within communities.

After having sent away all non-participant community members, we started by registering the names of all participants. We also implemented a short survey to elicit information on the respondent's personal characteristics (including gender and age), on his or her household's land size and tree ownership, on their perceived benefits of having trees on one's land, on some of their key preferences (including time preferences and risk preferences, elicited using non-financially incentivized multiple price lists), and on a series of possible decision biases (including self-determination, (lack of) self-control, and optimism). Interviews were conducted by extension workers (sometimes with the help of translators) who had received a full day's instruction on the do's and do nots of survey implementation as well as on the TILI and UPA procedure.

After all participants had been interviewed, the actual PES session started. Participants were informed that they would be given the opportunity to participate in a tree planting project. They were informed that if they participated in the project, they would be given 40 saplings of a (mixture of) tree species they preferred: mango, cashew, teak, acacia, etc. They were also informed that they would be provided with materials to protect the saplings from being eaten by livestock or wild grazers (especially chicken wire) and that their community would be provided with a donkey and cart to collect the water needed to keep the saplings alive. They were stimulated to think hard about the (public and private) benefits of having trees on their land (e.g., mango is expected to start bearing fruit after two years), but also about the (private) costs of doing so – the time and effort required to water the saplings and to protect the saplings from grazing, the fact that trees take up land that can otherwise be used for agriculture, etc. We

[7] A household was defined as the group of individuals sharing the same pot at night.

explained that in the first two years the main costs would be their time and effort to water the saplings (which needs to be done every two or three days in the dry season) and to protect them from being damaged by animals. After two years watering is no longer necessary because the trees' root system would by then have developed sufficiently to be able to extract water from the soils. We also stated that the most important costs from year 3 onwards would be reduced crop productivity because the trees would by then be sufficiently large to compete for both sunlight and water resources. If there were any questions on this, the farmers could ask their questions privately, upon which the session leader would repeat the question in neutral terms for the group to hear before answering it aloud.[8]

Next, the participants were separated into two groups; the ones who would receive the TILI treatment, and the ones who would be offered to participate in the tree planting project via UPA. The groups convened in different locations in the community (usually in two areas close to the community's central area, at least 50 meters away from each other), and they were informed of the mechanism via which it would be decided whether they would participate in the tree planting program (and for what price), or not. Whereas the TILI procedure is quite straightforward, that of UPA is more difficult to understand. Therefore we also announced that each of the two groups would be carefully informed of the mechanism via which participation would be determined and that, as an illustration, we would do a practice round so that every participant would perfectly understand the procedures we would follow. Rather than risking anchoring participants on specific prices by doing a hypothetical example of the tree planting project, we chose to offer them the opportunity to sell us one of their shirts using the relevant procedure (TILI, or UPA). To ensure that the farmers would be paying close attention, the practice round was financially incentivized – if their decisions indicated that they would be willing to sell their shirt for the predetermined price that we were willing to pay, they would have to hand in their shirt, and they would be paid that predetermined price. We also told them that they were not allowed to

[8] This is to control the exchange of information among participants. A farmer's question would be rephrased for the rest of the group in neutral terms, and the answer given would also be factual – explaining parts of the procedure that were unclear or explaining why asking one's minimum price is in their best interest, but never any statements about, for example, what specific amount they should ask (in UPA) or whether we recommend them to accept the price (in TILI).

communicate during the session (neither verbally nor non-verbally), and that they would be excluded if they violated this rule.

For both TILI and UPA it is essential that the prices the farmers in a community would be offered are (i) predetermined, (ii) the same for all participants in a community (to avoid conflict), and (iii) unknown not just to them but also to the experimenters and their translators. We implemented this as follows. For every community, we chose four different prices from the range of prices that we were willing to pay to buy their shirts (between 2 and 5 Ghana Cedis, that is between \$0.50-\$1.25), and we did the same for the range of prices the government of Ghana was willing to pay to each participant keeping 40 trees alive during the year (between 180 and 420 Cedis – or between \$45 and \$100).[9] Each price was written on two cards, each card was put in a small envelope, and the two small envelopes with the same price card were placed in a large envelope.

At the session, we thus had two piles of four large envelopes (one pile of envelopes containing shirt prices, and the other one containing prices for the tree planting project), and a trusted member of the community was invited to come forward and choose one envelope from each of the two piles. The two selected large envelopes were opened, and each of the teams of extension workers implementing the TILI and UPA treatments received one small envelope containing the price we were going to pay for the shirts, and also another small envelope containing the tree planting compensation price. The small envelopes were not opened (and hence the price was kept secret) until either all participants had made their bid (in UPA) or until the moment just before subjects were asked to make their yes/no decision (in TILI).

The mechanism used in the UPA treatment was explained as follows. Participants were reminded that the price at which we were going to buy their shirts is predetermined and hidden in the small envelope. They were asked to think hard about how much the shirt is worth to them – do they like their shirt, is it old or new, how costly would it be to go to the market to replace it? We told them that they would be asked to submit a bid for which they are willing to sell their shirt. If the price they asked for was smaller than or equal to the predetermined price in the envelope, they would receive the predetermined price and we would take home their shirt. If the

[9] Half of the communities are in Ghana's Sahel savanna (in the North-Eastern part of the country), and the half are in the Guinea savanna (in the North-West). Because the former is much drier than the latter, prices from the upper end of the range were offered to the communities in the North-East, and prices from the lower end were offered to those in the North-West.

price they asked for turned out to be higher than the predetermined price, their shirt would be too expensive for us, and hence we would not buy it. They would keep their shirt, and they would not be paid the predetermined price. We then also told them explicitly what price they should ask for: the minimum amount of money that they would need to receive to be willing to sell their shirt to us. Asking a higher price than their minimum does not yield any benefits (if the price they ask is still lower than the predetermined price, they sell their shirt at the predetermined price, but this would also have been the outcome if they had asked their true minimum price), while it may turn out to be disadvantageous (if the predetermined price is in between the price they ask and the minimum price they need to receive, they would not sell their shirt, and hence they would fail to make a profitable trade). And similarly, we explained that asking for a price lower than their minimum price would never be advantageous either, but that it could be disadvantageous too. Participants were then given the opportunity to ask questions in private, which would then be answered in public.

Next, the participants in the UPA treatment were called forward one by one. They were asked in private what minimum price they needed to receive to be willing to sell their shirt. After they stated their amount, the consequences of their asking price were explained once more[10], and they were also asked whether they would want to revise the price they asked.[11] After all participants in the UPA treatment submitted their asking price, the small envelope was opened and the price we were willing to pay for their shirt was revealed. Again, all UPA participants were called forward one by one to implement (or not implement) the trade. After this was completed we checked whether everyone understood the mechanism, whether (now that they learned the price) anybody regretted having asked the price they had asked and why, and whether everyone understood that it was in their best interest to ask the minimum price they needed to receive – not more, and not less. Again, all participants were allowed to ask their questions in private, which would subsequently be answered in public. We then continued with the tree planting bids, again explaining the benefits and costs, and emphasizing that they should think

[10] Defining Y as the price a participant had submitted, the relevant script was as follows. "The amount you ask is Y Cedis. So if the price in the envelope is Y Cedis or higher, you would sell your shirt and be paid the higher, predetermined price. If the price in the envelope is lower than Y Cedis, you would not sell your shirt at the predetermined price. Do you understand?"

[11] The script read as follows. "Would you regret having asked Y Cedis if the price in the envelope is less than Y Cedis? If so, would you like to revise your bid?"

hard about the minimum amount of money they would need to receive to be willing to provide the service of keeping saplings alive for the 2016-2017 agricultural season.[12]

The procedures and script used in TILI only differed from that of UPA in the description of the mechanism. Participants were encouraged to think hard about the benefits and costs of selling their shirts, and they were also encouraged to determine the minimum amount of money they needed to receive to sell their shirt or participate in the program. After all, having considered what the minimum is they need to receive, they should be happy to accept if the predetermined price is higher; if the predetermined price is lower, they should (be happy to) reject the price we offer. We re-emphasized that the prices for both the shirt and the tree planting project had been predetermined, and that they would be informed of the price in private, but that everyone ultimately is offered the same, still unknown, price. We checked whether everyone understood the procedure; any questions could be asked in private and were answered in public. Participants were then invited forward one by one. They were informed in private of the price in the envelope and were then asked whether they were willing to sell their shirt at that price, yes or no. Their answer was recorded, but trades were only implemented after all participants had made their decision.[13] After completion of the trades we again checked their understanding, and if necessary participants could ask clarifications in private (with the answer being given in public). An analogous approach was implemented for the TILI tree planting project.

Choosing what price to offer TILI (which is also to be used as strike price in UPA, to make sure that all participants in the program receive equal compensation) is complicated because the available information on opportunity costs was scarce and very imprecise. We proceeded as follows. First, in May 2016 we consulted close to 50 extension workers for their best guess of an "appropriate price". Based on these consultations, we set the price between 180 and 290 Cedis for the communities in the Guinea savanna regions in the North and North-West of Ghana, and

[12] The program is renewed every year, with the compensation level remaining the same in at least the first two years; the price may change after the third year because the nature of the net opportunity costs would change (from time and effort in the first two years to foregone crop revenues from year three onward, with the trees being expected to start producing fruits and nuts from the fourth year onward). We were careful in emphasizing the long-run nature of the tree planting program, but also stressed that the decision they needed to make was how much money they needed to receive for the program's initial period.

[13] Because the trade does not take place at the same time as the decision is made, participants cannot observe what decisions their fellow participants made, and hence they can also not be influenced by the decisions of others.

between 380 and 480 Cedis for communities in the more arid Sahel savanna in the country's North-East. Second, we implemented these prices in four pilot communities in early May 2016. We noticed that signup rates were close to 100% in both TILI and UPA, and proposed to the government of Ghana's project team to reduce the actual payments to save budget. The government team decided, however, that lowering the prices offered to the remaining communities would be a potential source of unrest, and hence they decided that the prices offered should remain high. While good news for the farmers, such high prices potentially jeopardize the research because they would result in very limited variation in outcomes – nearly all farmers were expected be willing to participate in the program, independent of whether they had been randomized into the UPA or TILI treatments.

As the government team was vehement that the prices should be kept very high, we decided to elicit take-up rates for much lower prices by asking TILI participants the following question: "if the conditional compensation offered is X Ghana Cedis, would you be willing to participate in the tree planting project?", where X was either 10, 30, 60 or 100 Cedis. This question was asked *before* the actual predetermined price (which was 180 Cedis, or higher) was retrieved from the sealed envelope. The question was non-binding, but we are confident that the respondents perceived the question as a legitimate inquiry into their willingness to accept.[14] Also note that the UPA protocol remained unchanged because by comparing the farmer's asking price to any of the prices we can determine whether the farmer would have been accepted into the program at that price, yes or no.

The RCT was implemented in six communities, and in total 260 household representatives participated in this experiment. We decided to drop households from the analysis with unrealistically large reported land areas (more than 100 acres) and/or with unrealistically high reported tree densities (more than 75 trees per acre). Doing so resulted in a sample of in total 226 households. Unfortunately, information on some key variables is missing for another 28, implying that our data set ultimately consists of 198 households, 97 of whom participated in the

[14] Participants were very much aware that their answers could not influence the price they would be offered – as the price was in the envelope. Hence, there is no reason for strategic misrepresentation. Also, comparing participation rates in the four pilot communities (without the extra question) to those in the last six communities (with the extra question), we do not find any evidence that this additional question affects the TILI participants' propensity to accept the actual price offer; these rates remain very close (if not equal) to 100%.

TILI treatment and 105 in UPA. Table 3.1 presents the characteristics of the participants in the two treatments.[15]

Table 3.1: Balance test for the field experiment

	TILI	UPA	Difference	p-value
Female	0.330	0.257	0.073	0.258
	(0.048)	(0.043)	(0.064)	
Age	42.742	41.657	1.085	0.621
	(1.621)	(1.484)	(2.193)	
Land area	14.624	11.619	3.005	0.123
(acres)	(1.681)	(1.032)	(1.940)	
Access to water point	0.165	0.162	0.003	0.954
(dummy variable)	(0.038)	(0.036)	(0.052)	
Owns trees	0.969	0.943	0.026	0.369
	(0.018)	(0.023)	(0.029)	
Perceived benefits of	2.361	2.433	-0.072	0.578
trees (numbers)	(0.092)	(0.090)	(0.129)	

[15] Not dropping the 34 households with unrealistically large reported land areas and tree densities results in the two treatments failing to be balanced on these variables. Because households were randomized into TILI and UPA within each community, this imbalance suggests that the reported numbers are indeed unrealistically high. All results reported in the rest of the paper are, however, robust (qualitatively, but also quantitatively) to not dropping these observations

Volatile agricultural revenues (perceived)	3.567 (0.051)	3.538 (0.058)	0.029 (0.077)	0.712
RiskAversion[a]	27.158 (2.034)	27.048 (1.820)	0.110 (2.721)	0.968
Impatience[b]	90.372 (11.205)	82.000 (9.985)	8.372 (14.959)	0.576
N	97	105	202	

[a] The survey question measures the minimum amount a subject needs (in Cedis) to receive with certainty to prefer that to participating in a lottery with a 50-50 chance of receiving 50 Cedis. A metric of risk aversion is obtained by subtracting the minimum amount to be received from the maximum payoff in the lottery (50 Cedis). The higher the number thus obtained, the more risk averse is the respondent.
[b] Minimum amount needed (in Cedis) to receive in 31 days to prefer that to receiving 20 Cedis tomorrow.

As shown in Table 3.1 the treatments are balanced on all variables. This is not very surprising because of our fairly large sample size, and also because of the fact that randomization occurred at the within-village level was instrumental in obtaining balance. We also have balance on key observable respondent characteristics like gender and age (the latter was verified using voting card IDs), and also on most of the variables measuring individual preference and attitudes.

3.2.2 Results of the field experiment

We first present the results of participants' willingness to sell their shirt in the take-it-or-leave-it (TILI) and the uniform price auction (UPA) approach. Table 3.2 shows the take up rates under the two approaches. We determine the willingness to sell of those participants participating in the UPA treatment by simply comparing the ask they submitted to the price we want to evaluate their decision at (2, 3, 4 or 5 Cedis). The procedure is somewhat more involved for the TILI participants. Participants were offered different prices for their shirts, and hence we observe their propensity to sell from their actual decisions for the price they were offered. But if we

observe that someone is willing to sell for 2 Cedis, we also know that she would have been willing to sell for 3, 4, or 5 Cedis, and hence we code her decision accordingly for these prices too. Similarly, if we observe that someone is not willing to sell for 5 Cedis, we also know that she would not have been willing to sell if the price offered had been 4, 3 or 2 Cedis. Using these coding procedures, Table 3.2 shows the shares of subjects willing to sell their shirt at the price levels offered.

Table 3.2: Shares of subjects willing to sell their shirts in the TILI and UPA treatments, for different price levels

Price (in Cedis)	TILI	Auction	Difference	p-value [a]
2	0.204	0.209	-0.004	0.946
3	0.645	0.283	0.362	0.000
4	0.930	0.353	0.578	0.000
5	0.968	0.458	0.510	0.000

[a] p-values obtained using two-sided two-proportions tests.

Table 3.2 shows that participants in the TILI treatment are more willing to sell their shirt for 3 out of the 4 prices we offered; the difference in participants' propensity to sell between the two treatments only fails to be significantly different at the lowest price we offered.

Table 3.2 provides a glimpse that the two mechanisms may indeed turn out to be different in practice, and we now proceed testing whether this is indeed also true for our participants' decision to accept participating in the tree planting program. Participation rates under UPA and TILI for a selection of prices are presented in Table 3.3. For each respondent in the UPA treatment we looked up whether her ask was above or below each of the specific prices, and subsequently calculated, for each of these prices, the share of participants who had submitted an ask less than or equal to that specific price. As the mechanism is incentive compatible, this is our best predictor of take-up rate in the group of respondents who were offered the contract with a specific price, take-it-or-leave-it style. For example, we observe that 48% of the participants in the UPA treatment submitted an ask less than or equal to 10 Cedis, and hence we expect 48% of the participants in the TILI group to be willing to participate when offered the contract with 10 Cedi compensation. The second column of Table 3.3 shows actual take-up rates as observed

among the group of respondents who were offered the opportunity to participate take-it-or-leave-it style.

Table 3.3: Shares of subjects accepting actual (180 and higher) and virtual price (100 and lower) offers in the TILI and UPA treatments.

	Price offered	Sh. Accepted UPA	Sh. Accepted TILI	p-value
Hypothetical prices	10	0.476	0.655	0.0962
	30	0.581	0.927	0.0000
	60	0.742	0.975	0.0000
	100	0.800	1.000	0.0036
Actual prices	180	0.725	0.933	0.008
	380	0.975	1.000	0.103
	>380	0.971	1.000	0.081

[a] p-values obtained using two-sided two-proportions tests.

Not surprisingly Table 3.3 shows that the share of respondents deciding to participate is a (weakly) increasing function of the conservation price offered, independently of whether the respondents participated in the UPA or TILI treatment. More importantly, we find that when offered a specific price take-it-or-leave-it style, respondents are (much) more likely to agree to participate in the PES program than those who are requested to submit their ask in the UPA treatment. We thus find that there is a systematic and significant difference between the farmers' propensity to participate in the PES program in the UPA treatment and in the TILI treatment. UPA systematically and significantly underestimates farmers' propensity to participate in the PES program when contracts are offered take-it-or-leave-it style. This difference is substantial, and even remains significantly different for the real prices offered (180 Cedis and higher). We thus replicate Jack (2013) result that the UPA mechanism results, and substantially so, in lower take-up rates than the TILI mechanism.

So what causes this difference between predicted and actual uptake? Jack (2013) documented the difference too, but was unable to identify the mechanism. Berry et al. (2015) also test whether an incentive-compatible valuation mechanism – in their case Becker-de Groot-Marshack (BDM) – is a good predictor of agents' behavior respondents' propensity to purchase. They offer participants, poor farmers in northern Ghana, the opportunity to buy a new water filter. They thus look at the buyer's side of the market (as opposed to our focus on the seller's side) and find that their incentive-compatible value elicitation mechanism underestimates their respondents' propensity to purchase the water filter.

Comparing the outcomes of Berry et al. (2015), Jack (2013) and ours, it is interesting to note that all three find that the incentive-compatible mechanisms underpredict market transactions that occur with take-it-or-leave-it offers, independent of whether the respondents are on the buyer's or the seller's side of the market. Berry et al. hypothesize that in their experiment the difference in take-up rates is due to strategic considerations playing (a more important) role in their BDM treatment than in the TILI treatment. They experimentally vary disclosure of the water filter's sales price at the markets in the regional capital (which may affect BDM bids via anchoring) and they also experimentally vary the provision of information that the study's outcomes will be used to determine the filter's future sales prices when their supply is scaled up in the villages. None of these manipulation significantly affects outcomes, suggesting the difference between predicted and observed purchase rates is due to neither strategic bidding nor to anchoring.

In this study we test the relevance of an alternative mechanism, based on the hypothesis that the UPA invites more deliberate and hence also more careful decision making than take-it-or-leave-it price offers. Even though we explicitly instruct our TILI participants to carefully think through the consequences of saying yes or no to the price that will be offered to them in a moment – explicitly telling them to carefully think about the minimum amount of money they would need to receive to be willing to participate in the tree planting program – the actual incentive to think through the consequences of one's decisions is stronger if one is asked to submit than if one knows the contract price will be revealed before one has to make a decision oneself. We hypothesize that if decision-making is more deliberate, the explanatory power of economic variables (like available land area and prior experience with the benefits of having trees on one's land) and preferences (like risk attitudes and time preferences, but also more

psychological characteristics like optimism and impulsiveness) is higher under UPA than under TILI.

To test this hypothesis, we focus our attention on analyzing differences in outcomes between TILI and UPA for the (Shanahan, 2012 100)1 price set (as we do not have any variation in the TILI decisions for the actual prices). In the case of TILI we simply regress the yes/no decision on the price offered on a series of economic variables influencing the costs and benefits of participating in the program (like land area, and whether one has experience with the benefits of having trees on one's land), and also on a series of personal characteristics like gender, age, and "behavioral" parameters like risk aversion, impatience, optimism.

We implement the same analysis for UPA, but the procedure is slightly more complex because the outcomes of the analysis may be dependent on whether an individual's participation decision is determined using a strike price of 10 Cedis, or of 100 Cedis. For every participant in the UPA treatment in a community, we randomly select one of the prices (10, 30, 60, 100) that had been offered to the TILI participants in her community. We compare the participant's asking price in the UPA to the randomly selected price, and the participant is coded as accepting to participate in the tree planting project if the former is less than or equal to the latter. Outcomes may depend on the assignment of prices to the participants, and hence we implement a Monte Carlo approach using 1000 replications for each model specification we use.

Table 3.4 presents the probit regression results explaining the participants' decisions to be in the tree planting program for the UPA and TILI households using two different specifications. The price offered is included in specification (i), and so are key farm characteristics that are very likely to affect the farmer's propensity to participate in the tree planting program – land area, access to water, and the presence of trees. The propensity to participate is expected to be higher the higher is the price offered, the larger the farm's land area, and if there is easy access to water to be able to water the saplings in the dry season. The fourth variable included, the presence of trees on the farmer's land, is also likely to affect participation decisions, although the sign of the coefficient is ex-ante ambiguous. It may be positive if having trees on their land implies that farmers are more acutely aware of the (private) benefits of having them, but of course, it may also be negative if there are diminishing marginal returns to having trees on one's land. Specification (ii) includes, in addition to the ones included in (i), characteristics of the farmer him/herself, including gender, risk posture, whether he/she perceives farm revenues to be very

volatile, (im)patience, impulsiveness, whether he/she is optimistic about his/ her future prospects, and also a set of interaction terms.[16]

Focusing first on specification (i) in Table 3.4, the propensity of farmers to participate is increasing in the price offered in both the UPA and TILI treatments. However, while all three key farm characteristics are highly significant in the UPA treatment, only one of them – the presence of trees on one's land – shows up significantly in the TILI treatment, whereas both the available land area and easy access to water are not found to affect the farmer's participation decision. Adding additional controls (see specification (ii) for both UPA and TILI) does not affect these results, and while the farmers' personal characteristics do not seem to be very important drivers of decision making, there are still marked differences between UPA and TILI. First and foremost, optimism about the future is the only farmer characteristic that shows up significantly in the TILI regressions; all other variables are far from significant. In UPA there is also just one variable showing up significantly at the 10% level or better. By itself, farmers' perceiving their farm revenues as very volatile does not affect their decisions, and the same holds for their risk postures. However, the above-average risk averse farmers are more likely to sign up for the tree planting program if the farm revenues are more volatile (p = 0.093) – as the tree planting project can be seen as a means to diversify risk. This interaction term is the only significant variable in UPA, but it is interesting to see that there are three other ones that are close to being significant (with p-values below 0.15). Interacting time preferences and impulsiveness, we find that the p-value on patience is 0.13, which suggests that more patient farmers are more likely to sign up – unless they also tend to be more impulsive (p = 0.15). And also optimism only just fails to be significant in the UPA regressions (p = 0.11)

Table 3.4: Probit regression results explaining participants' decision to (not) accept to be in the tree planting program.

	UPA		TILI	
	(i)	(ii)	(i)	(ii)
Price offered	0.018***	0.020***	0.024***	0.027***
	(0.005)	(0.006)	(0.004)	(0.005)

[16] Risk attitude (being risk averse), patience, optimism, and impulsiveness are all dummy variables based on a median split, and take a value 1 if the subject belongs to the above-median category.

Land area	0.0434***	0.047***	-0.001	-0.011
	(0.010)	(0.014)	(0.006)	(0.014)
Access to water	0.994**	1.115**	-0.325	-0.540
	(0.393)	(0.384)	(0.511)	(0.463)
Owns trees	0.998***	1.196***	1.649***	1.102*
	(0.285)	(0.299)	(0.506)	(0.576)
Female		-0.296		-1.004
		(0.336)		(0.622)
Age		0.550		0.071
		(0.353)		(0.442)
Optimistic		0.636+		1.284***
		(0.339)		(0.435)
Patient		0.621+		-0.377
		(0.311)		(0.858)
Impulsive		0.155		-0.520
		(0.549)		(0.796)
Patient x Impulsive		-1.215+		0.153
		(0.714)		(1.345)
Risk averse		-0.036		0.385
		(0.312)		(0.785)
Volatile revenues		-0.597		0.822
		(0.449)		(0.584)
Risk averse x Volatile Revenues		0.866*		-0.854
		(0.348)		(0.725)
N	105	105	99	97
Pseudo R2	0.17	0.30	0.19	0.36

Robust standard errors in parentheses, clustered at the community level. + $p < 0.15$, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Our findings thus suggest that key variables pertaining to the perceived costs and benefits of participating in the program (access to water and land area) have explanatory power in the UPA treatment but not in the TILI treatment, and there are also traces of evidence that also more behavioral parameters tend to affect decisions in the former whereas they are nowhere near significant in the latter. One possible explanation for this result may be that decision making is

more deliberate in UPA than in TILI.[17] Deciding whether or not to participate in a program for a given price requires less mental effort than deciding what the minimum amount of money is that one needs to receive to be willing to participate in the program. If this is the case in the field, we should be able to find similar results in a laboratory environment, where the decision situation can be simplified and behavioral parameters can be measured with more precision.[18]

3.3. The laboratory experiment

To further test whether decision making is more deliberate in UPA than in TILI, we decided to implement a laboratory experiment in which we would endow our subjects with an item that we would subsequently buy back using either UPA or TILI. For this test, the choice of the item to be sold by the participants is crucial. For the laboratory experiment to be able to reflect the outcomes of the field, we should endow participants with an item where (i) observable personal characteristics are (expected to be) predictive of individual subject's valuation of the item under consideration (similar to the role of observables like land area and access to a water in the field experiment), and where (ii) behavioral parameters (like patience and risk preferences in the field experiment) are expected to be predictive too. Identifying such an item is not trivial. After all, subjects in the lab are predominantly WEIRD[19], and hence it is not straightforward to find specific observables that are predictive of the decision to sell (or not to sell) a specific item.

Gender is an obvious individual characteristic of any randomly selected sample of WEIRD subjects, and hence we searched the literature to find an item that tends to be evaluated differently by different genders. Rozin, Levine, and Stoess (1991), analyzed gender differences in the appreciation of various food items and identified chocolate as one of the items where the gender difference in appreciation is largest – with female students appreciating chocolate better

[17] Of course, more considerate decisions are not necessarily better decisions if "behavioral biases" are dominant. We will come back to this issue later on in this paper.

[18] Of course, the true test of whether decision making is indeed more deliberate (and hence also better) is whether the survival rates are higher under UPA than under TILI. At the time of writing this chapter that information is not available yet. Also it is doubtful whether our experiment would be able to detect such an effect, as the payments offered by the government are so high that the incentives to keep the trees alive are very strong – even for those who accepted participating in the program without thinking through all the consequences. But this is left for future research, when the survival data become available.

[19] White, Educated, Industrialized, Rich and Democratic; (Henrich, Heine, & Norenzayan, 2010).

than male students. Knetsch (1989), documented that the propensity to sell chocolate bars in the laboratory is affected by the degree of loss aversion, and hence we decided to endow student subjects with chocolate bars, and buy them back using either UPA or TILI.

The stakes in and complexity of selling a chocolate bar in the laboratory are negligible compared to the considerations involved in the decision whether or not to participate in a multi-year tree planting program in the field. This means that asking WEIRD subjects to sell their chocolate bars under either UPA or TILI in the laboratory is a strong test of the hypothesis that decision making is more considerate in the UPA treatment than in TILI.

3.3.1 Design of the laboratory experiment

The chocolate bar we selected is of a brand that was introduced in the Netherlands just a few weeks before the implementation of the experiment. That means that our subjects did not have prior information on sales values they might anchor their bids on.[20] Also, at the time of the experiment the taste was new for the Dutch market (salty fudge and brownie). Upon entering the lab the subjects received their chocolate bar (wrapped and sealed), and were offered the opportunity to sample the chocolate by taking a piece from a plate which was passed around. Unbeknown to the subjects we recorded whether a subject did or did not decide to taste the chocolate.

The actual implementation of the UPA and TILI treatments mimicked the field procedures as closely as possible. In the general introduction subjects were informed that the chocolate bar they received was theirs, but that they would be offered the possibility to sell it back during the session. We emphasized that they should carefully consider how they appreciated the bar; do they like the taste, etc. We also announced that the price at which we were willing to buy back, had already been determined. The price was set at market price estimate in the first session of the chocolate sell and we varied the prices for some of the sessions. We then proceeded with the treatment-specific instructions, informing the subjects of the way (UPA, or TILI) in which they would be offered the choice to sell back their bar to us. We implemented the same scripts of informing subjects (about the incentive-compatibility) of the two mechanisms as we used in the

[20] Coincidentally, the chocolate was produced in cooperation (‘fair trade’) with Ghanaian farmers.

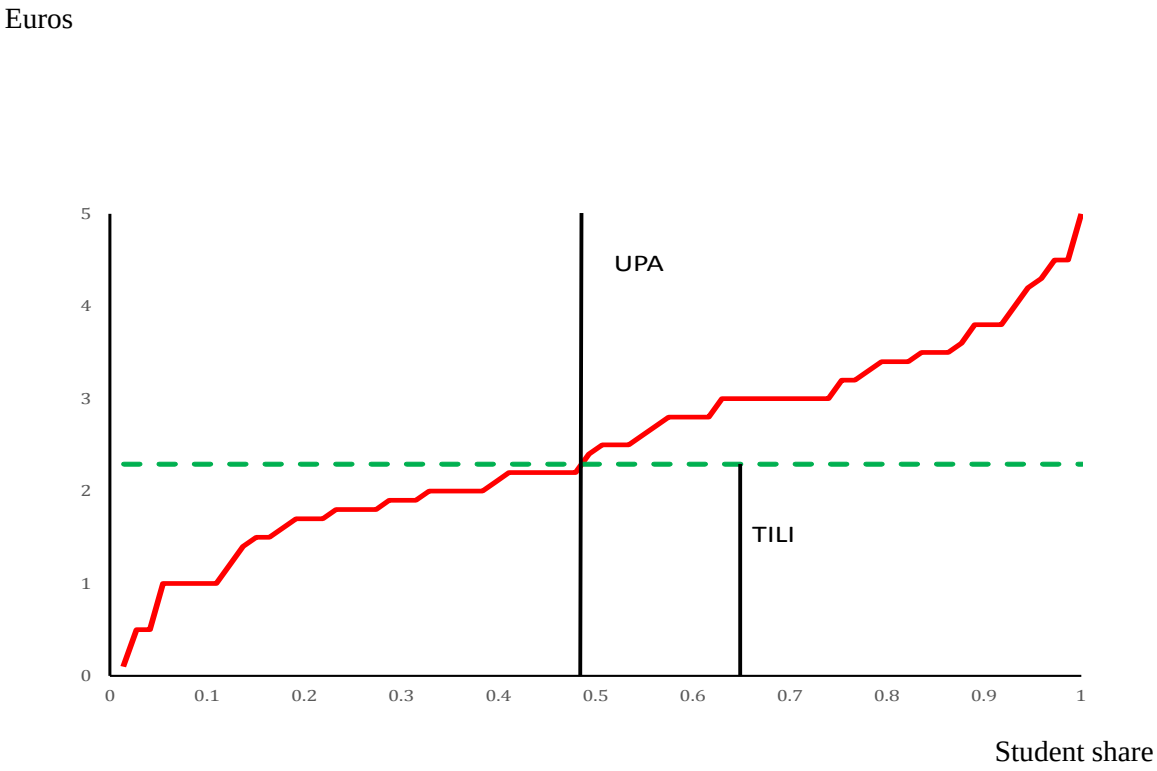
field. We elicited risk and loss aversion preferences using a financially incentivized multiple-price list approach (Holt & Laury, 2002).

In total, 132 subjects participated in 7 sessions; 73 in the UPA treatment and 59 in the TILI treatment. We recorded their gender, country of origin, what study program they were in, and whether they were willing to sample the chocolate. We hypothesize that decision making is more deliberate in UPA than in TILI, and this hypothesis can be tested in two ways. First, deliberate decision-making takes time, and hence we test whether the amount of time needed to take a decision differs between the two treatments. The second approach is via regressions similar to the ones presented in the field – if UPA induces more deliberate decision making than TILI we expect key explanatory variables like gender and loss aversion (measured using a financially incentivized multiple price list) to have more explanatory power in UPA than in TILI.

3.3.2 Results of the laboratory experiment

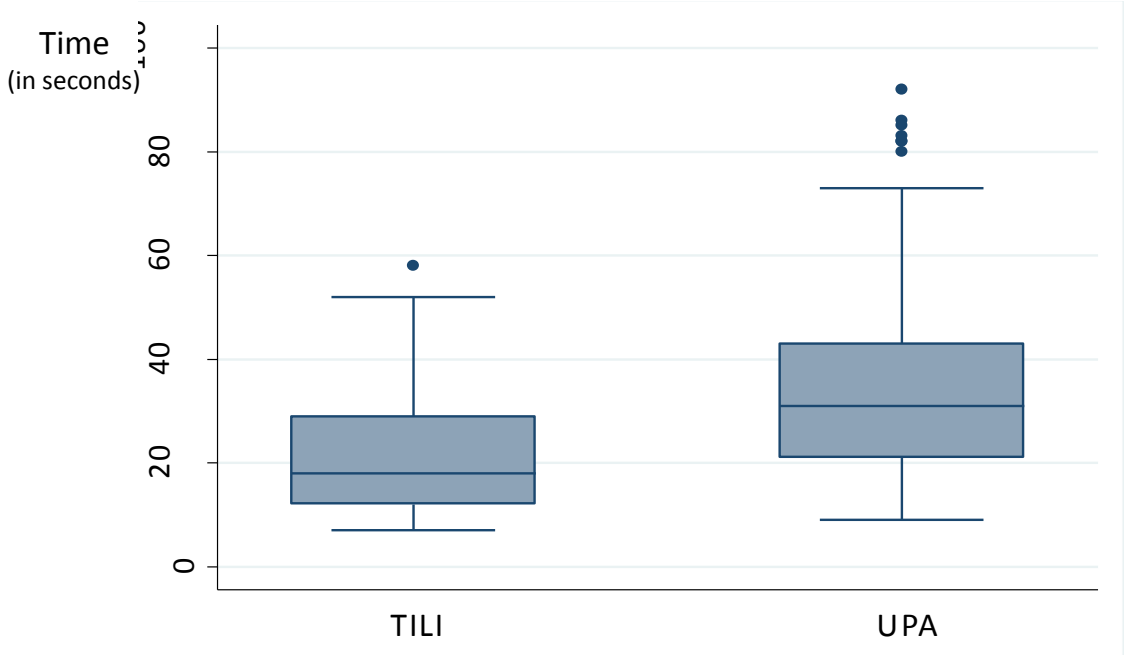
We first test whether indeed subjects in TILI are more prone to selling their bar than those in UPA. Figure 3.1 presents the opportunity cost schedule of the asks in UPA (the upwardsloping, non-interrupted line), the predetermined strike price (the dashed, horizontal line), as well as the share of subjects willing to sell their bar at that predetermined price. As was the case in the field, the share of subjects willing to sell is higher in TILI than in UPA. The respective shares are 0.64 and 0.48, and this difference is significant at $p = 0.059$ according to a standard two-sided Chi-squared test. So, we find a difference in take-up rates of about 16 percentage points (or 33% higher in TILI than in UPA).

Figure 3.1: The chocolate bar opportunity cost schedule, the predetermined strike price and the shares of subjects in UPA and TILI who are willing to sell their bar at the predetermined price in the laboratory experiment.



Having established a difference between selling rates in UPA and in TILI, we test whether decision making is indeed more deliberate in the former than in the latter. Figure 3.2 shows the means of times taken to make the decisions (whether to accept selling one's bar for the predetermined price in TILI, and one's asking price to be willing to part with one's chocolate bar in the UPA treatment). The results found are in line with our expectations. The mean time taken in UPA is 40.52 seconds, as compared to 23.93 seconds in TILI. This difference is significant at $p < 0.0001$ according to a two-sided Mann-Whitney U test.

Figure 3.2: Box plots of decision times in UPA and TILI (in seconds).



Note: The plots show the intervals containing the middle 50% (in the boxes) and 90% (the barred lines) of the observations, as well as the outliers in terms of time taken. The horizontal lines in the boxes give the median times.

Next, we test whether key characteristics like gender and loss aversion can explain at least some of the variation in sales outcomes in UPA, and not in TILI. To do this, we ran several specifications of a probit regression model (analogous to those presented in Table 3.4 for the field experiment); results are presented in Table 3.5.[21]

Table 3.5: Probit regression results explaining student participants' decision to (not) accept to sell their chocolate bar.

	(1) UPA	(2) TILI	(3) UPA	(4) TILI
Unwilling to Taste	0.389 (0.722)	0.0554 (0.752)	0.379 (0.750)	0.0639 (0.834)

[21] Two subjects switched columns more than once in their loss aversion multiple price list, and hence their choices are inconsistent. We excluded these two observations from the regression analysis. Also, we decided to not include risk aversion as a control variable in the regression analysis. No fewer than 107 of our 131 participants were found to be risk neutral, and hence the variance in this variable is too small to be included in the analysis.

Western	-0.192	0.294	-0.149	0.295
European	(0.326)	(0.206)	(0.293)	(0.205)
Male	0.268	0.720*	0.696**	0.692
	(0.419)	(0.399)	(0.353)	(0.582)
Loss Averse	-0.0111	0.0251	0.500	-0.0129
	(0.369)	(0.171)	(0.378)	(0.240)
Loss Averse x Male			-1.082***	0.0843
			(0.360)	(0.910)
Constant	-0.0901	-0.125	-0.371***	-0.111
	(0.182)	(0.321)	(0.135)	(0.369)
N	71	56	71	56
adj. R^2	0.015	0.069	0.045	0.069

Robust standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Columns (1) and (2) of Table 3.5 present the results, for UPA and TILI respectively, when controlling for, among others, gender and loss aversion. Gender shows up significantly for TILI, at p = 0.071. Columns (3) and (4) show the results upon adding the interaction term between gender and loss aversion. The substantial increase in the adjusted R^2 suggests that the results of column (3) are preferred to those of column (1). Comparing (3) and (4), the results are quite striking. None of the explanatory variables we collected has any predictive power in TILI, while we find both the observable variable (gender) as well as the behavioral parameter (loss aversion, for men) significantly affect a subject's propensity to sell in UPA. Consistent with Rozin et al. (1991) we find that male students have a lower appreciation of chocolate (and hence are more willing to sell) than female students. And we also find that male students with above-median levels of loss aversion are less likely to sell than below-median loss averse men. Female students and loss averse male students are thus less eager to sell their bars than "loss neutral" male students. The fact that behavioral and observable (or economic) characteristics are predictive of decisions in UPA but not in TILI parallels the outcomes of the field experiment, and thus adds credibility to the claim that decision making is more considerate in the former than in the latter.

3.4. Conclusions

Payments for Ecosystem Services (PES) schemes have rapidly gained importance as an instrument to stimulate environmental conservation on private land. Typically, the purchaser of nature conservation services (the government, or an NGO) offers landowners a contract, take-it-or-leave-it (TILI) style, specifying the types of activities a landowner should undertake and the amount of compensation she will receive in return.

One of the key challenges for PES design is to find the appropriate compensation level. Offering too low a price does not induce conservation, a price that is too high quickly exhausts the PES budget. Economic theory has developed several "truth-telling mechanisms", so-called incentive compatible value elicitation methods, in which revealing one's actual (opportunity) costs of service provision is the optimal strategy. Theoretically, behavior under both mechanisms should be the same, and hence for a given price take-up rates should be the same. In this paper, we replicate Jack's (2013) result that for a given compensation price, the share of landowners agreeing to participate in a PES program is substantially higher when offering them a contract take-it-or-leave-it style than predicted using another incentive-compatible elicitation method, the Uniform Price Auction.

We hypothesize that the difference is caused by the fact that indicating one's true willingness to accept is much more cognitively demanding than making the decision whether one is willing to participate in the program for a specific price. We find some evidence for this claim in the field experiment on reforestation we implemented in Northern Ghana, as we find that variables that plausibly predict participation (like land area) show up significantly in the regressions explaining participation using the incentive-compatible elicitation method (the Uniform Price Auction), but that the coefficients on these variables are not significant in the regressions explaining participation in the take-it-or-leave-it treatment. We proceed by running a laboratory experiment with (i) a much easier decision problem (whether subjects are willing to sell back the bar of chocolate that they received before, (ii) where we have priors on observable characteristics (gender) and behavioral parameters (loss aversion) that plausibly affect outcome of the decision variable (sell, or not sell), and (iii) where we can easily observe how much time it takes to make the decision – which is arguably correlated with how difficult the decision task is considered to be. Consistent with our expectations, we find that both gender and attitudes to losses predict the outcome in the Uniform Price Auction treatment but not in the take-it-or-leave-

it treatment, and we also find that the time needed to make the decision in nearly twice as long in the former than in the latter treatment.

We thus find that decision making is more deliberate in UPA than in TILI. Despite the fact that implementing UPA is more complex than implementing TILI, the government may prefer to op for the former – especially if the PES compensation price is a relatively small share of the total conservation costs involved (because of all the other costs, such as the costs of providing the saplings, fencing materials and the like).

Chapter 4:

Trust and Trustworthiness between cooperators and non-cooperators in Public Good Provision:

Evidence from an Artefactual Field Experiment in Ethiopia

4.1. Introduction

Trust and trustworthiness are important elements in the dynamics of cooperation and development (Bouma et al., 2008; Fukuyama, 1995; Knack, 2001). Trust and other pro-social behaviors are also crucial factors in overcoming collective action problems (Ostrom, 1991; Ostrom, 2014). Standard economic theory predicts that collective action problems arise because selfish agents have no incentive to contribute to public goods (Olson, 2009). However, numerous empirical and experimental studies show that substantial shares of mankind contribute to public goods (Andreoni & Samuelson, 2006; Chaudhuri, 2011; Fehr & Schmidt, 1999), implying the existence of the conditional cooperators in addition to selfish agents. Furthermore, cooperators tend to initiate these contribution actions, trust more and are more trustworthy (Algan & Cahuc, 2010; Bouma et al., 2008; Knack, 2001; Ostrom, 2000).

However, whether the observed cooperation can be sustained, with multiple types of agents in the population, is an empirical question. Insights can be derived from predictions of existing theories to understand trust and trustworthiness between cooperators and non-cooperators. Social identity theory predicts that people have more affinity towards their fellow in-group member than non-group member (Tajfel, 1974). If conditional cooperators and selfish agents can engage in potentially mutually beneficial yet strategically risky exchange opportunities, the prediction of social identity theory could be the strategy taken by the agents. There are extensive experimental studies in discrimination literature which examine in-group and out-group biases (see meta analysis by Lane, 2016). Findings of these studies suggest that by and large, out-group biases are the most prevalent phenomena in line with the prediction of social

identity theory. However, most of these studies utilized what is called the minimal group paradigm, i.e., laboratory assigned identity which is distinct from real-world identity (Goette, Huffman, & Meier, 2012; Lane, 2016). Goette et al. (2012), examined to what extent the test of in group-out group biases observed in a minimal group setting yields different result if real identity is to be considered. Their findings pointed out that lab-induced identity and real-world social ties yield different results. And there is a need to conduct more field-based experiments to understand in-group and out-group biases and its role on the outcome such as cooperation.

Another way to understand in-group-out-group biases, in the context of whether to trust or not, is to look at if these biases have also their beginning originated in indirect evolution theory. The indirect evolutionary theory states that biases might arise not only because of the player's identity or social ties but whether the player's given identity is also considered as reciprocal type. This is because the indirect evolutionary theory assumes that perhaps most of the cooperation observed is sustained with the desire of being reciprocated (Güth & Yaari, 1992). Accordingly, the less reciprocal types are selected against, which makes reciprocity an evolutionarily stable strategy (Güth, 1995).

As an extension of this theory, Ostrom (2014) proposed a revised theory of collective action to explain how trust and other prosocial behavior evolve to enable cooperation. Ostrom's revised theory of collective action predicts that players receive an objective payoff but the decision is based on the transformation of the objective payoff into intrinsic value. For instance, conditional cooperative players value trust, fairness and other pro-social behaviors that in turn, adds a subjective change parameter to actions that are consistent with their norms. Hence, these conditional cooperators are modeled to be a trustworthy type, while selfish agents are forward-looking and self-interested. When information about the types of players (conditional cooperators vs. selfish agents) is provided and if players can engage in potentially mutually beneficial yet strategically risky exchange opportunities, the conditional cooperators will consistently receive a higher payoff. Thus, using evolutionary reasoning only the conditional cooperators will survive if complete information is available (Ostrom, 2000).

The similarity of the two theories is that both theories predict the decision of subjects correlates with the identity of whom they are matched with conditional on own identity. Since to trust or not to trust has economic consequences, then the payoffs from the decision depend on i)

own identity and ii) identity of another player. This can be demonstrated with a standard trust game (Berg et al., 1995), where full information of the group and non-group member type is known. The difference between the two theories is described in the following illustration. Suppose in the trust game, there are two types of players; group members and non-group members. Suppose that group members are considered more trustworthy, while non-members are considered to be more selfish. Also assume that there is full information on the types of the players. Then social identity theory predicts that group members trust other group members more than that they trust non-group members, and the reverse will hold for non-group members. Yet indirect evolutionary theory predicts that both group members and non-group members will put more trust in group members,

To test the pertinence of these theories, I designed a setting in which participants who are members of Collaborative Forest Management group (CFM), along with some non-members (non-CFM), play a trust game. The trust game is a standard experimental procedure to measure the level of trust and trustworthiness (Berg et al., 1995). The game is played by two players, one of whom is the first mover and the other is the second mover. In the game, the first mover is endowed with a certain amount of money and has to decide if and how much money to send to the second mover. The amount sent by the first mover is usually tripled by the experimenter before it is sent to the second mover. The second mover has to decide how much of the money received to send back to the first mover. Standard economic theory predicts that (selfish) second movers will not return any money, and therefore (selfish) first movers will not send any money in the first place. However, a first mover may give money to the second mover in the hope that her trust will be reciprocated. Trust is then measured by the amount sent, and trustworthiness is measured by the amount returned.

In the current setting, social identity theory predicts that members of a group send higher amounts to other group members than to non-group members whereas non-group members send higher amounts to fellow non-group members. Ostrom (2000) theory of collective action, on the other hand, assumes that there are (at least) two types of agents, selfish types, and conditional cooperators. And, even though conditional cooperators are more likely to display pro-social behavior than selfish agents at first, in the case of total information provision (about the types of

players), all types will favor the cooperator type. As both theories offer different alternative hypotheses we can use the experimental data to test which one is most appropriate.

In this study, the group members are specifically the members of a Collaborative Forest Management (CFM) group. CFM was deployed initially as a rational response to a crisis in forest management in the 21st century that had clearly signaled sustainable forest management to be unfeasible (Lawlor, Madeira, Blockhus, & Ganz, 2013; Pinyopusarerk, Tran, & Tran, 2014). CFM is used as a conservation tool in developing countries, the Americas, Australia and also some parts of Europe (Dyke, Cash, Brody, & Thornton, 2005; Kibria, Makoto, & M., 2014; Matthews, 2009; Papacostas, 2014). In light of the lower productivity of the top-down approach in considering the community's involvement in environmental management, attention was given to designing a management tool that considers the active involvement of communities in the conservation of natural resources (Frasera, Dougilla, Mabeeb, Reeda, & McAlpinec, 2006; Kibria et al., 2014; Lawlor et al., 2013).

CFM membership in the study area, the Bale Eco-region of Ethiopia, is in principle voluntary and everyone can become CFM member, but the procedure follows certain rules of eligibility such as geographic proximity to the forest area and length of time having lived in the village (Dubé & Schmithüsen, 2014). CFM members are a group of farmers (usually up to 30 individuals in a group) who jointly manage the forest. These CFM groups are assigned a specific forest area to look after by the local government forest conservation office. A village creates as many of these groups as it can form depending on the size and carrying capacity of the forest area in their region. Finally, these groups are administered by one cooperative "body" at the village level. The cooperative is the body responsible for distributing the benefits from the forest to the CFM members.

The extant literature on trust is mostly concentrated around the discussion of cooperation at the individual level, such as if the trusting individuals are also cooperators (Bouma et al., 2008; Johnson & Mislin, 2011). Meanwhile, the in- and out-group trust – that is, if individuals put equal trust in cooperators and in non-cooperators in a field experiment setting – have received scarce attention in the literature to date (Chaudhuri, Paichayontvijit, & Shen, 2013; Kugler, Bornstein, Kocher, & Sutter, 2007; Kugler, Kausel, & Kocher, 2012; Lane, 2016). To address this broader aspect, in terms of understanding interactions among the key players (with

their identity known), studies have focused predominantly on the role of trust in decision-making as a group and as an individual in laboratory experiments. These groups are typically not natural groups but are constructed by the experimenter (the so-called minimal group design; (Bornstein & Yaniv, 1998; Garza, Becker, & Kugler, 2011; Goette et al., 2012; Luhan, Kocher, & Sutter, 2009; Wolf, Insko, Kirchner, & Wildschut, 2008). To our knowledge, there has been no four-dimensional exploration of trust and trustworthiness, in and out of a group, that allows for the systematic interaction of all the players, where the players' identities are provided in the field setting. As will be explained below, I am able to do so because I use an extended version of the strategy method.

Hence, this study differs from previous studies, specifically Chaudhuri et al. (2013); Kugler et al. (2007); Kugler et al. (2012), (Goette et al., 2012) in terms of the design, subject pool and due to the presence of different types in the sample. The design of this study does not allow for making decisions as a group, but rather as an individual. The experiment uses a combination of a between-subjects design and a within-subjects design in which the participants play a trust game both with a CFM member and a non-CFM member. Hence, the participants play the trust game twice, once with a CFM member and once with a non-CFM member. In addition, they make decisions both as a first mover (sender) and as a second mover (returner). As the identity of the participant (being a CFM member or not) is known, this design enables an investigation of the role of identity, i.e. CFM membership, on the decisions made in the game, in particular on the amounts sent as well as on the amounts returned.

In light of this, the current study aims to examine the role of identity, specifically of being a cooperator, in relation to trust and trustworthiness. The working hypothesis is that given the high level of trust towards the participants in a collective action, the CFM members will receive higher payoffs. The expected channel is that the CFM members will send higher amounts but also that higher amounts will be returned to CFM members.

This study finds the following results. First, both CFM members and non-CFM members send more money to CFM members (typically perceived as more cooperative types) than to non-CFM members, but the difference is not statistically significant. Second, and interestingly, the non-CFM members, in general, send more than the CFM members. However, the difference in the decision made is driven mainly by the behavior of CFM members towards the non-CFM

members. Thus, it seems that not only to whom the money is sent matters but also it is important to look at the identity of the sender, i.e. whether or not the sender belongs to the CFM group. Third, regarding returner decisions, we find that the CFM members and non-CFM members behaved differently with respect to the amount returned. The average share returned by CFM members is lower than the share returned by non-CFM members which is caused mainly by the behavior of CFM towards non-CFM members.

These results support the hypothesis that higher trust is placed in individuals who contribute to real-world public good provision – CFM members. The CFM members receive more money than non-members, but they send and return less, specifically to non-members. CFM members thus benefit more from participating in the trust game than non-members. Note that the CFM members in the area are not considered more powerful than non-members since the benefits that a member derives from cooperation are more of intrinsic nature than objective benefits. For example, one can easily access fire wood and timber products from the market without sacrificing time and effort looking after the forest blocks, and instead, choose to spend more time on other (agricultural) activities. Moreover, as will be discussed in more detail later in the paper, we see that both the CFM and non-CFM members are equally engaged in other social interaction activities. Also note that in order to rule out the possibility of misunderstanding the game by the CFM members as an opportunity they can use to be rewarded for their everyday cooperative behavior, the experimenters have thoroughly explained that the final payoff of participants only depends on the decisions they make during the trust game.

Hence, at first, it seems that there is some kind of paradox that although CFM members may trust non-CFM members less (and thus send less), the non-CFM members are in fact more trustworthy (return more). However, the findings suggest that the possible channel in which the cooperating types consistently derive higher payoffs is probably in line with Ostrom's revised theory of collective action. Nevertheless, we do not have a repeated trust game setting to fully claim these results the stable strategy in the long run. This can be addressed with future research.

The policy relevance of this study relates to understanding pro-social behavior with relation to engagement in collective good management and maximizing the advantages that arise with it (see Bouma et al., 2008; Narloch et al., 2012; Papacostas, 2014). Recently, collective environmental management has been proposed as a tool to further address global climate change

(Ostrom, 2010) through the implementation of REDD+ (Reduction of emission from deforestation and forest degradation) (Newton, Oldekop, Brodnig, Karna, & Agrawal, 2016; Pelletier, Gélinas, & Skutsch, 2016). Hence, understanding the functioning of trust and other pro-social behavior among the "local conservation groups" and other non-member individuals is crucial (i) to deepen our understanding of the role of pro-social behavior between cooperators and non-cooperators; (ii) to examine the prudence of collective level environmental management. By conducting a field experiment in a relevant study area, we attempt to learn and understand the behavior of people who are individually involved.

4.2. Experimental Design, Procedure, and Hypotheses

4.2.1 The Field

The context of the study is the Bale Mountains Eco-region in Ethiopia (see Chapter 2 for more information).

The sample in this study is taken from Dodola "Woreda" (the lower administration level next to regional administration), out of which three villages were selected: Bura-Adelle, Kechema, and Geneta (see Figure 4.1). These villages were selected because they were among the first to implement forest management in the Bale Eco-region, and they are more accessible in terms of infrastructure. The three villages in this study (Bura-Adelle, Kechema, and Geneta) were selected because they are more accessible in terms of infrastructure, and more important for the current study, they were among the first to implement forest management in the Bale Eco-region.

CFM is a joint action by a group of farmers, in which one group contains up to 30 individuals, to conserve the forest block assigned to them in their respective villages. All the groups in different villages are organized under one cooperative that is responsible for sharing the benefits accrued from forest products and from the hunting permitted in the forest. Members are in return rewarded from these benefits. In this study, although all the participants are forest dependent, not all of them are members of collaborative forest management (see Appendix 3 for details).

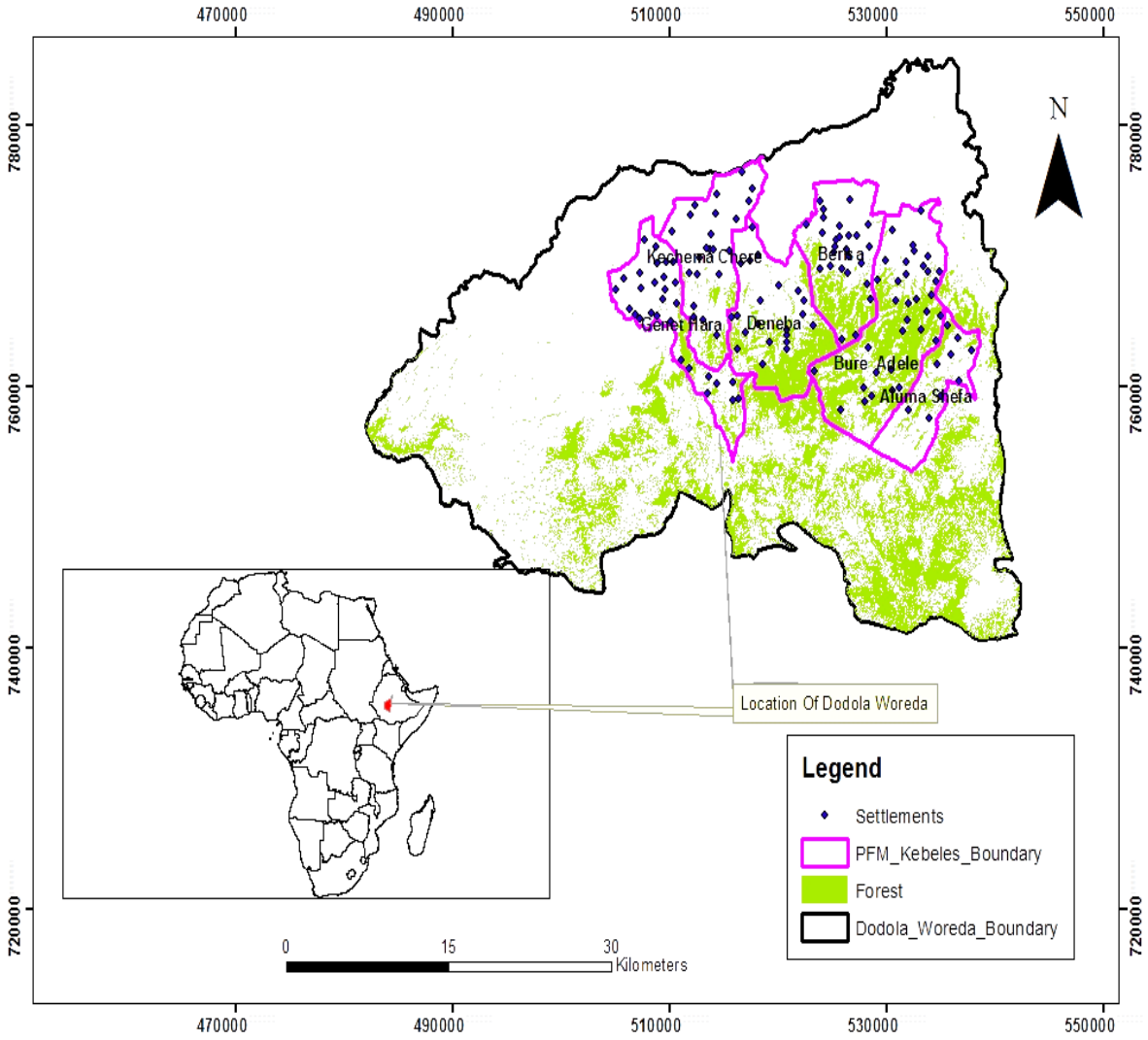


Figure 4.1: Map of the study area.

4.2.2 Experimental Design

The experiment employed in this study is based on a trust game. In a standard two-player trust game, developed by Berg et al. (1995), both players are endowed with a certain amount of money. One player (Player 1, the trustor) has to decide how much of his endowment to send to another player (Player 2, the trustee). In its standard format, the amount sent by Player 1 is tripled by the experimenter and given to Player 2. Then, Player 2 has to decide how much of the amount received she wants to send back to Player 1. The payoffs for Player 1 are the amount kept as Player 1 plus the amount returned by Player 2, whereas the payoffs for Player 2 are equal

to the amount she received minus the amount she sent back to Player 1.[22] Using backward induction, standard economic theory predicts that Player 2 will not return any money, and thus Player 1 will also not send any money. Numerous studies have found, however, that both the amounts sent and amounts returned are positive, but also that decisions are affected by experimental parameters ((such as endowment, multiplication factor, etcetera, see Johnson & Mislin, 2011).

In the experiment, I implement two changes compared to this standard trust game. First of all, in a standard trust game, subjects play only one role; that is, they are either the trustor (sender) or the trustee (returner/receiver). In the current experiment, each subject plays both roles. Second, rather than hot decision making, I apply the strategy method (Selten, 1967), in which subjects have to make decisions for every situation that may arise – in this case, whether they are matched with a CFM member, or with a non-CFM member. In each role, players thus have to make two choices. In particular, in their role as Player 1, each subject has to make two decisions: (i) how much he wants to send to the other subject if the other subject is a Collaborative Forest Management (CFM) member, and (ii) how much he wants to send to the other subject if the other subject is not a Collaborative Forest Management member (non-CFM). The same approach is used for return decisions. That is, in their role as Player 2, each subject has to indicate how much he wants to send back to the other subject (i) if the other subject is a CFM member; (ii) if the other subject is a non-CFM member. Note that the use of the strategy method or the fact that subjects play both roles and make multiple decisions does not affect the game-theoretic predictions based on standard, selfish and money-maximizing preferences. Experimental findings and insights from behavioral economics suggest, however, that both variations could have an impact on behavior.[23] This should not matter for our experiment and results, as this study's aim is not in absolute levels but in (treatment) differences in behavior

[22] There are several variants of the standard trust game. One of the variations is to give Player 2 no endowment or a positive endowment. We have chosen to not endow Player 2.

[23] For example, Burks et al. (2003) find that playing both roles reduces both trust (the amount sent) and reciprocity (the amount returned). Casari and Cason (2009) report that the strategy method gives similar rates of trust but significantly lower rates of trustworthiness. Note, however, that in their study trustors could only send nothing or the full amount. Differences are often attributed to differences in emotions and intentions associated with the environments.

towards CFM members and non-CFM members, and between CFM members and non-CFM members.

The details of the experimental design are as follows (see Appendix 1 for the instructions). First, all subjects have to make two decisions as Player 1. Player 1 is given ETB50 (which is half a day's wage) at the start of the game (Player 2 receives 0), and each Player 1 has to decide how much of the ETB50 he wants to send to Player 2. There were six possible choices: ETB0, ETB10, ETB20, ETB30, ETB40 and ETB50. As explained above, Player 1 has to make two decisions: how much she wants to send to Player 2 if Player 2 is a CFM member, and how much she wants to send to Player 2 if Player 2 is a non-CFM member. Then all subjects have to make decisions as Player 2. As Player 2, each subject has to decide for each of the possible amounts received, how much she wants to send back to Player 1. For example, if ETB10 is chosen by Player 1, Player 2 receives ETB30, and she can decide whether to send back ETB0, ETB10, ETB20 or ETB30. Also as Player 2, subjects have to make two series of six decisions: (i) how much money they want to send back to Player 1 if Player 1 is a CFM member conditional on that CFM member sending her 0, 10, 20, 30, 40 or 50 ETB, and (ii) how much they want to send back to Player 1 if Player 1 is a non-CFM member, for each of the 6 possible amounts they can receive.

In order to make all choices' potential payoff relevant, the payoffs to each subject were based on random matching and random role assignment, which was done after all decisions had been made. First, after all, decisions had been made, pairs of two subjects were formed, and one subject in each pair was randomly assigned the role of Player 1 (trustor) and the other of Player 2 (the trustee). Then for both subjects, it was checked if they were a CFM member or a non-CFM member, and this determined which of the decisions was implemented. So for example, suppose a CFM member was assigned the role of Player 1, who was matched with a non-CFM member playing the role of Player 2. Payoffs for both participants were then determined by (i) verifying how much the CFM member would be willing to send if she was matched with a non-CFM member (say 20 ETB), and (ii) looking up how much of the 60 ETB the non-CFM member would be willing to return if she was matched with a CFM member who would send him ETB 20. All details of the experiment including the matching and the payment procedure were explained to the subjects.

4.2.3 Experimental Procedure

The experiment took place from January 8-10, 2016, in one of the lower administrations in the Bale Eco-region of Ethiopia. The participants in the experiment are forest-dependent individuals from three villages (Bura-Adelle, Kechema and Geneta). The invitation to subjects to participate in the experiment was made with the help of development agents from Oromia Forest and Wildfire Enterprise who work closely with forest-dependent communities in general and with CFM members in particular. The development agents asked the leaders within these villages to send 30 to 35 participants per village to the meeting/experiment; the sample had to include both CFM and non-CFM members.

Upon the participants' arrival to the session, an introductory meeting was held in the open field (where they usually hold meetings), and the local development agent introduced the research team to the participants. Next, the research team informed the participants of the general objective of the research, followed by the experiment's instructions. Further explanation was given when necessary to make sure that the participants understood the procedure. Details of the decision-making were provided when players were interviewed individually, specifically concerning the possible choices they had when making decisions as a first mover and a second mover. Hence, after the joint instruction, the subjects were individually approached for a survey and to make decisions in the experiment.

Subjects came forward to make their decisions one by one, and were invited to wait for the procedure to continue in an area that was physically separated from the area where the yet-to-be-interviewed participants were waiting. Interviewing one participant took 15 minutes on average. There were four data collectors and interviewers including the researcher, without including the development agent who was the facilitator at the experiment site. Per village, the trust game experiment took on average two and half hours including time spent on explaining and illustrating the game as well as answering questions from the subjects.

At the end of the experiment, after all the subjects were interviewed and made their decisions, the random assignment of a role was implemented. It was done so by using separate matching cards on which we filled in information such as card number, participant's membership status in a Collaborative Forest Management (CFM), amount to be sent to CFM and non-CFM

member, and finally the amount to be returned for all possible amounts sent (0, 10, 20, 30, 40, 50) to CFM and non-CFM members. After filling out all the information on the matching card, the cards were placed in a box and mixed together to enable the random matching. Matching was done in such a way that by definition the first card picked was assigned the role of Player 1, while the second card picked was assigned the role of Player 2. Then, the information on the cards was used to see whether players were matched with a CFM or non-CFM member and implemented the appropriate decisions of both players to calculate the payoffs. The matching was done anonymously but in front of the participants. Finally, the participants were called forward to collect their money based on the decision they made. Note that the final payoffs depended on actual CFM membership status of the person a subject was paired with, such that subjects had an incentive to use their true preferences and beliefs (which may be different when they are matched with CFM or non-CFM members). Decision-makers did not get a chance to know the person with whom they were matched, and their identity was kept anonymous.

4.2.4 Hypotheses

The main question that is addressed in this study is whether trust and trustworthiness depend on the identity of the person (being a CFM member or not) and/or on the identity of the person she is matched with. Based on the discussion in the introduction, several testable null hypotheses can be formulated.

H1: CFM members send the same amount as non-CFM members.

This hypothesis can be divided into two sub-hypotheses in two ways, thus resulting in four testable hypotheses:

H1a: CFM members send the same amount to CFM members and to non-CFM members;

H1b: Non-CFM members send the same amount to CFM members and to non-CFM members.

H1c: CFM members and non-CFM members send the same amount to CFM members;

H1d: CFM members and non-CFM members send the same amount to non-CFM members.

As argued in the introduction, there may be (at least) two reasons why one may expect behavior to be different. The first reason follows from social identity theory (Tajfel, 1974), which suggests that people have more affinity with in-group members than with out-group members, and as a result they will treat in-group members more favorably. In the current setting, this theory predicts that CFM members will send higher amounts if they are matched with CFM members than if they are matched with non-CFM members, whereas non-CFM members will send higher amounts to non-CFM members. Ostrom's theory of collective action, on the other hand, assumes that there are (at least) two types of agents, selfish types and (conditional) cooperators where cooperators are more likely to display pro-social behavior than selfish agents.[24] Assuming that CFM members are the more cooperative type, this theory would predict that CFM members at first send more to CFM members and to non-CFM members.

Combined with the assumption that CFM members are the more cooperative type, thus more trusted, this theory would predict that CFM members receive higher amounts than non-CFM members, irrespective of the identity of the sender. As both theories offer some different alternative hypotheses, the experimental data can be used to test which one is most appropriate. For example, both theories suggest that hypothesis H1b will be rejected, but Ostrom's theory predicts that non-CFM members send more to CFM members whereas social identity theory does not. Similarly, while Ostrom's theory suggests that H1c will not be rejected (as amount sent to CFM members is the same), social identity theory predicts that it will be rejected (as CFM members send more to CFM members).

In a similar vein, null hypotheses for the amount returned can be derived as the following;

H2: CFM members return the same share as non-CFM members.

Again this hypothesis can be divided into four testable sub-hypotheses:

[24] According to Ostrom's theory, cooperators are not only more likely to display pro-social behavior than selfish agents at first but in the longer run also end up receiving higher payoffs because they are more trusted. Since the time frame in the experiment is very short, we do not focus on this.

H2a: CFM members return the same share to CFM members and to non-CFM members;

H2b: non-CFM members return the same share to CFM members and to non-CFM members.

H2c: CFM members and non-CFM members return the same share to CFM members;

H2d: CFM members and non-CFM members return the same share to non-CFM members.

Here again, social identity theory and Ostrom's theory may offer some conflicting hypotheses. For example, the first would predict that non-CFM members would return more to non CFM members than to CFM members, whereas the latter would predict the opposite.

4.3. Results

4.3.1. The Sample

Table 4.1 presents the descriptive statistics of the participants of each village in the experiment and balance tests of covariates using orthogonality tests. There were 96 participants from the three villages. Note that these are the same individuals as those who participated in the Valuation paper (Kitessa, 2017). The three villages have household compositions that are quite similar in terms of various characteristics, alongside the geographical one. Table 1 shows that in all the villages the participants in the experiments are predominantly male. This is probably because of the cultural setting in which, as the household head, the male is more likely to attend meetings. In a similar way, except for one village (Village 1), more than half of the sample population have a family size of 10 members or more in one household. In all of the villages, only 20% of the households have families with fewer than four members. This shows that the study area is characterized by large families and the average number of children is much higher than the country's average of 4.1 children per household.

In the sample, the reported average income of the households ranges from 2,500 ETB to slightly above 4,500 ETB per month, and the reported land size in hectares is 2.47 on average (within the range of 2.16 to 2.78 hectares).

Table 4.1: Descriptive Statistics

Variable	Village 1	Village 2	Village 3	(1) vs. (2)	(1) vs. (3)	(2) vs. (3)	p-value of overall test
Male (%)	0.853	0.875	1.000	-0.022	-0.147**	-0.125**	0.102
	(0.062)	(0.059)	(0.000)	(0.086)	(0.066)	(0.061)	
Family size of 4 or less (%)	0.176	0.094	0.033	0.083	0.143*	0.060	0.173
	(0.066)	(0.052)	(0.033)	(0.033)	(0.077)	(0.063)	
Family size within the range of 5 to 7 (%)	0.206	0.188	0.233	0.018	-0.027	-0.046	0.908
	(0.070)	0.070)	(0.079)	(0.099)	(0.105)	(0.105)	
Family size within the range of 8 to 10 (%)	0.206	0.094	0.167	0.112	0.039	-0.073	0.456
	(0.070)	(0.052)	(0.069)	(0.089)	(0.099)	(0.086)	
Family size of 11 or more (%)	0.412	0.625	0.567	-0.213*	-0.155	0.058	0.204
	(0.086)	(0.087)	(0.092)	(0.122)	(0.126)	(0.127)	
Mean income (ETB)	2552.073	3480.500	4884.514	-928.426	2332.440***	-1404.014*	0.004
	(353.689)	(543.309)	(539.248)	(640.770)	(630.928)	(766.487)	
Mean hectares of land owned	2.483	2.164	2.780	0.319	-0.297	-0.616*	0.289
	(0.286)	(0.257)	(0.263)	(0.386)	(0.392)	(0.367)	
Participants	0.029	0.000	0.033	0.029	-0.004	-0.033	0.605

below 25 years of age (%)	(0.029)	(0.000)	(0.033)	(0.030)	(0.044)	(0.032)	
Participants within age group of 25 to 35 (%)	0.265 (0.077)	0.313 (0.083)	0.200 (0.074)	-0.048 (0.113)	0.065 (0.107)	0.112 (0.112)	0.608
Participants within age group of 36 to 55 (%)	0.441 (0.086)	0.313 (0.083)	0.667 (0.088)	0.129 (0.120)	-0.225* (0.123)	-0.354*** (0.121)	0.018
Participants above 56 years of age (%)	0.265 (0.077)	0.375 (0.087)	0.100 (0.056)	-0.110 (0.116)	0.165* (0.097)	0.275** (0.105)	0.042
Participants with no formal education (%)	0.235 (0.074)	0.219 (0.074)	0.067 (0.046)	0.017 (0.105)	0.169* (0.090)	0.152* (0.089)	0.162
Participants with up to 5 years of schooling (%)	0.559 (0.086)	0.625 (0.087)	0.433 (0.092)	-0.066 (0.123)	0.125 (0.126)	0.192 (0.127)	0.315
Participants with 6-8 years of schooling (%)	0.206 (0.070)	0.156 (0.065)	0.467 (0.093)	0.050 (0.096)	-0.261** (0.115)	-0.310*** (0.112)	0.012
Participants with 9 or more years of schooling (%)	0.000 (0.000)	0.000 (0.000)	0.033 (0.033)	0.000 (0.000)	-0.033 (0.031)	-0.033 (0.032)	0.336
Mean non-	0.088	0.563	0.400	-0.474***	-0.312***	0.162	0.000

members of Dabo (%)	(0.049)	(0.089)	(0.091)	(0.100)	(0.100)	(0.127)	
Call Dabo two times per year (%)	0.676	0.250	0.500	0.426***	0.176	-0.250**	0.002
	(0.081)	(0.078)	(0.093)	(0.113)	(0.123)	(0.121)	
Call Dabo more than two times per year (%)	0.235	0.188	0.100	0.048	0.135	0.087	0.369
	(0.074)	(0.070)	(0.056)	(0.102)	(0.094)	(0.090)	
Mean members of Idir (%)	0.147	0.250	0.267	-0.103	-0.120	-0.017	0.456
	(0.062)	(0.078)	(0.082)	(0.099)	(0.101)	(0.113)	
CFM members (%)	0.765	0.719	0.433	0.046	0.331***	0.285**	0.012
	(0.074)	(0.081)	(0.092)	(0.109)	(0.117)	(0.122)	
No. of participants	34	32	30	66	64	62	

Standard deviation in parentheses * *p < 0.10,* ** *p < 0.05,* *** *p < 0.01*

It is also important to note some between-village differences. In village 3, both income and the hectares of land owned are on average much higher than in the other two villages. It is sensible that average income is higher where the size of land owned is relatively higher, given that almost all the participants are farmers.

Less than 3% of participants in the experiment are within the age bracket of 25 years and below. In fact, the majority are adults of 35 years of age and above. The younger age group (25-35 years) accounts for about 26% of the participants.

With regard to participants' literacy status, about half of the participants have up to 5 years of schooling, while village 3 seems to be relatively more educated than the other two

villages. Thus, the sample of this study predominantly consists of farmers with low literacy status who are also forest dependents. Other variables will be discussed in later sections.

In general, although some differences exist between the villages, the sample is rather balanced in terms of observable characteristics. Importantly, even though we control for village differences later in the analysis, we do not believe that the differences between the villages matter, as we are interested in the effect of identity (treatment differences).

4.3.2. Experimental Results

4.3.2.1 Descriptive statistics and non-parametric test results

Table 4.2 presents the summary statistics of the trust game. This table reports the average amount sent by Player 1 in case she is matched with a CFM member and also for the case she is matched with a non-CFM member. Similarly, the average amount returned by Player 2, for all (possible) discrete values that were initially sent by Player1, are shown in the table. These decisions (on how much to send as Player 1 and to return as Player 2) were made twice, with respect to a CFM member and a non-CFM member. Column 1 presents the average amount sent (returned) to CFM members, while column 2 reports the average amount sent (returned) to non-CFM members. The last column shows the p-values of Wilcoxon matched-pairs signed-ranks test comparing the two values.

The first and second column of Table 4.2 show that on average, the mean amount sent by Player 1 is 15.70, which is about 31.40% of the initial endowment. This measures the trust level. To get some idea about the trust level in this study, it can be compared with the trust levels observed in various other studies. It turns out that compared to all regions of the world, this average is rather low, even though some variation across studies and regions can be observed (see Table 4.3). This relatively small amount sent seems to be in line with the previous finding that trust is lower in African countries compared to other regions of the world (see the meta-study by Johnson and Mislin, 2011). The same two columns in Table 4.2 also show that the average amount sent to CFM members is almost 1.5 as high as the average amount sent to non-CFM members (18.64 versus 12.76). A Wilcoxon matched-pairs signed-ranks test shows that this difference is statistically significant at the 1% level (see the last column).

Table 4.2: Summary statistics of trust game

	Matched with CFM member	Matched with non-CFM member	p-values (Wilcoxon signed-ranks test)
Mean amount sent by Player 1	18.64 (10.22)	12.76 (8.14)	0.000
Mean amount returned by Player 2			
if trustor sends 10	12.55 (6.53)	11.09 (5.77)	0.043
if trustor sends 20	22.92 (11.96)	20.00 (11.69)	0.008
if trustor sends 30	35.89 (18.97)	31.88 (17.09)	0.034
if trustor sends 40	48.54 (24.41)	41.46 (26.03)	0.004
if trustor sends 50	57.29 (30.80)	51.61 (30.04)	0.023
Share amount returned by Player 2			
if trustor sends 10	0.418 (0.22)	0.369 (0.19)	0.049
if trustor sends 20	0.382 (0.20)	0.333 (0.19)	0.007
if trustor sends 30	0.399 (0.21)	0.354 (0.19)	0.035
if trustor sends 40	0.405 (0.21)	0.345 (0.22)	0.003
if trustor sends 50	0.382 (0.21)	0.344 (0.02)	0.022
Average share returned	0.397	0.349	0.000

Note: Standard deviations in parentheses; last column indicates the p-values of statistical tests of difference in values reported in the second and third columns (Wilcoxon matched-pairs signed-ranks tests).

The amounts and shares returned by Player 2 to CFM and non-CFM members are also reported in Table 4.2. It should be noted that for all possible values that Player 2 received, they decided to return on average at least 30 percent. Taking averages of shares returned at all levels, the overall average share is about 0.35, which is slightly greater than the shares returned in some parts of the world (see Table 4.3). Interestingly, comparing the shares returned in columns 1 and 2 of Table 4.2, we observe that for all possible amounts sent, a higher share is returned to a CFM member (column 1) than a non-CFM member (column 2), and all differences are significant at

the 5% level, at least. This shows that the CFM members received consistently and significantly higher returns on their initial investments.

Table 4.3: Fraction sent and returned in trust games by region

Variable name	Obs.	Sum N	Mean	Std. dev.	Min	Max
Panel A: Fraction sent (trust)						
All regions	161	23,900	0.502	0.124	0.224	0.885
North America	46	4579	0.517	0.158	0.259	0.885
Europe	64	9030	0.537	0.121	0.224	0.783
Asia	23	3043	0.482	0.102	0.285	0.710
South America	13	4733	0.458	0.074	0.336	0.857
Africa	15	2515	0.456	0.133	0.300	0.750
This study	To CFM	96	0.373	0.204	0.000	1.000
	To non-CFM	96	0.254	0.163	0.000	1.000
Panel B: Proportion returned (trustworthiness)						
All regions	137	21,529	0.372	0.114	0.108	0.812
North America	41	4324	0.340	0.089	0.119	0.496
Europe	53	7596	0.382	0.094	0.108	0.542
Asia	15	2361	0.460	0.114	0.215	0.597
South America	13	4733	0.369	0.147	0.184	0.812
Africa	15	2515	0.319	0.106	0.180	0.514
This study	To CFM	96	0.397	0.178	0.000	1.000
	To non-CFM	96	0.349	0.168	0.000	1.000

Source: (Johnson & Mislin, 2011). The number of observations (obs.) is the number of studies reported in the meta-study.

Next, I consider the role of identity by breaking down the cooperators' and non-cooperators' behavior. We do so by examining the amounts sent by subjects who are CFM members and those who are non-CFM members. Table 4.4 shows that both the CFM and non-CFM members alike send more to CFM members than to non-CFM members. For both groups,

these differences are highly significant ($p < 0.01$, Wilcoxon matched-pairs signed-ranks test). Furthermore, non-CFM members send on average about 2.5 ETB more than the CFM members. However, the results from a Mann-Whitney U test indicate that the difference is not statistically significant when the receiver is a CFM member, whereas it is highly significant when the receiver is a non-CFM member ($p = 0.002$, Mann-Whitney U tests).

Table 4.4: Amount sent to CFM and non-CFM by CFM membership status (Identity)

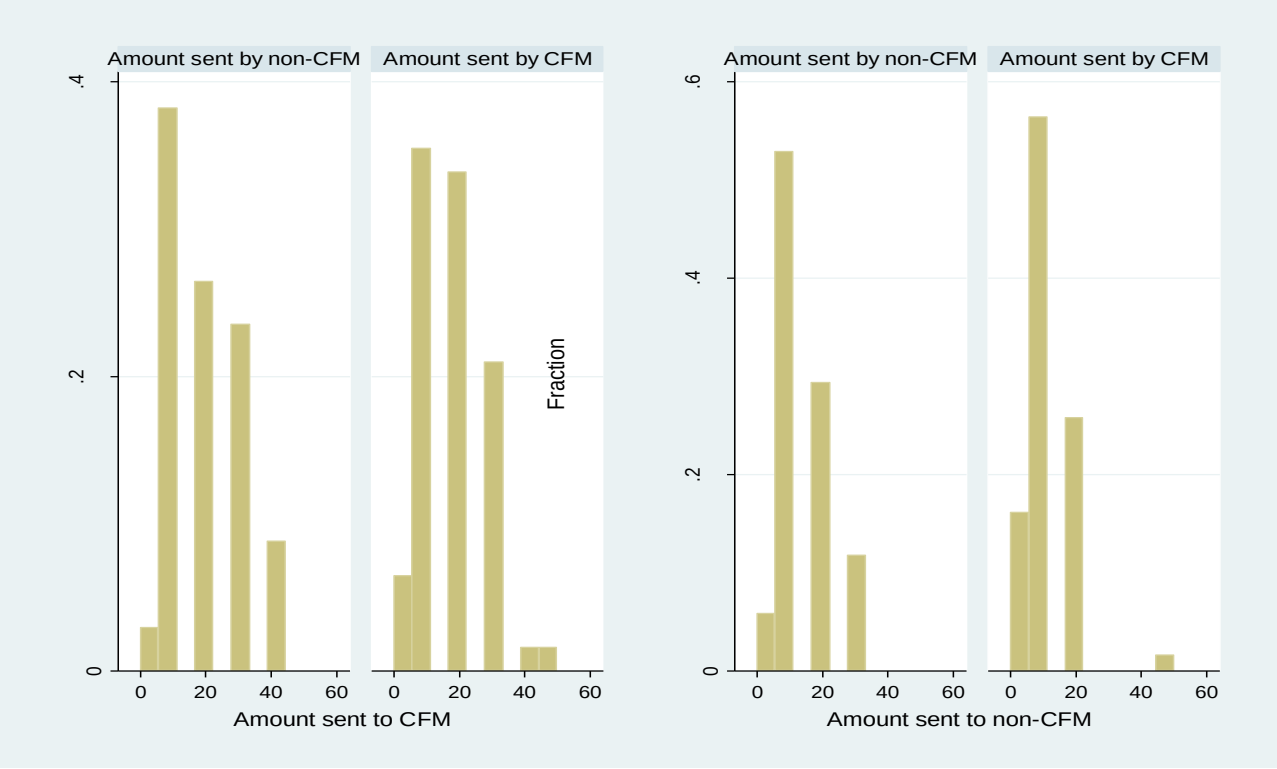
	Amount sent to CFM	Amount sent to non-CFM	p-values (Wilcoxon signed-ranks test)
Mean sent by CFM	18.06 (10.06)	11.61 (8.137)	0.000
Mean sent by non-CFM	19.71 (10.58)	14.71 (7.87)	0.012
p-values (Mann-Whitney U test)	0.51	0.002	
Observations	62	34	

Taken together, these results support our hypothesis that CFM members are viewed as more trustworthy than non-CFM members, not only by fellow CFM members but also by non-CFM members. At the same time, however, non-CFM members send more than CFM members, regardless of the membership status of Player 2, which suggests that they trust more.

A graphical summary of the decision-making allows us to document the divergence in behavior between the two groups in more detail. Figure 4.2 presents the histograms of the amounts sent in the trust game by the CFM and non-CFM members, in the right panel and left panel, respectively. According to Figure 4.2, the distributions of the amounts sent to CFM members by both CFM and non-CFM members differ slightly from the distributions of the amounts sent to non-CFM members. For instance, the distribution of amounts sent to non-CFM (by both CFM and non-CFM) is more right skewed and shows a central tendency on a lower value relative to the other group. In contrast, the amounts sent to the CFM members show a

rather normal distribution. Interestingly, both CFM and non-CFM behaved in a relatively similar way towards CFM members. Thus, the distributions of amounts sent in this experiment differ in shapes based on with whom the participants were matched (CFM vs. non-CFM). This claim is also supported by the formal test of difference in distributions. The Kolmogorov-Smirnov test shows that there is no difference in distributions between the amounts sent to CFM by CFM and non-CFM- the first panel. And Kolmogorov-Smirnov test shows no statistical difference in distributions between the amounts sent to non-CFM by CFM and non-CFM- the second panel. Thus, though between-subject decision distributions, i.e. the amounts sent to CFM and to non-CFM differ, there is no difference in within-subject decision distributions, i.e., amounts sent to CFM by CFM and non-CFM vs amounts sent to non-CFM by CFM and non-CFM.

Figure 4.2: Histograms of the amount sent to CFM members and non-CFM members, by membership



So far I have determined that the differences in amounts sent are related both to the identity of Player 1 (CFM member or not) as well as to the characteristics of Player 2 (CFM member or not), where the latter plays a bigger role. CFM membership thus seems to be

important, but we would like to know if the observed differences are really related to CFM membership or could also be due to people's involvement in other types of cooperative behaviors. To find out whether measures of other cooperative action could also play a role, we asked subjects about their participation in different forms of real-world social interaction.

The hypothesis here is that the rate of one's involvement in related social and economic cooperation differs between CFM and non-CFM members. If that is the case, the observed differences in the amounts sent and returned in the trust game may be due to those differences rather than to the CFM membership. To test this hypothesis we examine how the participants' involvement relating to social cooperation within the community varies across CFM and non-CFM members. We consider three measures of social interaction. The first is the number of times per year that a participant is involved in labor sharing (exchange) in relation to farm activities. Involvement of individuals in this labor sharing activity is locally known as "Dabo," and it is one of the three traditional types of social interaction in the study area (Ruben & Heras, 2012). The second type of cooperation is called "Ikub," which is a traditional saving association in which members make regular contributions to a common fund. And the third type of cooperation is "Idir," which is a group (or social) support system in which people voluntary become members to help each other, specifically in times of deaths and funerals.

Table 4.5 shows the subjects' involvement in different social interaction activities including Dabo by their CFM membership status. See also Table 4.2 for the summary statistics of these cooperation measures, namely Dabo, Ikub and Idir, across the villages in our sample. The variable Dabo takes the values 0, 1 and 2. On average, CFM members do just slightly less than one Dabo activity per year, whereas non-CFM members do on average 2/3 activity. Nevertheless, we cannot reject the null hypothesis of no difference between CFM and non-CFM members (Fisher's exact test, $p = 0.14$).

The middle column in Table 4.5 shows that the percentage of subjects' participation in Ikub is rather low in general and the rate does not differ for CFM-members and non-CFM members (Fisher's exact test gives $p = 0.29$). To some extent, the same applies to the last type of cooperation, "Idir." The difference in Idir participation between CFM and non-CFM members is small and not statistically significant (Fisher's exact test $p = 0.45$).

Table 4.5: Participants' involvement in cooperative activities by CFM membership status

Actual CFM membership	Dabo per year (Average # times)	Ikub (% participation)	Idir (% participation)
non-CFM member	0.65	0.26	0.62
CFM member	0.93	0.19	0.58
p-values (Fisher's Exact)	0.144	0.289	0.448

Hence, since as there is no evidence that these cooperative activities differ between CFM and non-CFM members, the conclusion is that the observed differences in amounts sent are mainly due to CFM membership and not due to other types of community involvements. This conclusion is supported if we consider the amounts sent and returned in more detail. Table 4.6 confirms that the decisions on the amount sent are by and large unaffected by any of the three other social cooperation status, while amounts sent to CFM members are systematically higher than amounts sent to non-CFM members, and this behavior is irrespective of the cooperation type.

Table 4.6: Amount sent (all) by other cooperation measures

Cooperation type		**Amount sent to CFM**	**Amount sent to non-CFM**	**No. observations**
Idir	Member	19.12	12.63	57
	Non-member	17.95	12.82	39
p-values (Mann-Whitney U test)		**0.482**	**0.616**	
Dabo	Cooperator	18.09	12.86	63
	Non-cooperator	19.69	12.42	33
p-values (Mann-Whitney U test)		**0.440**	**0.827**	
Ikub	Member	19.05	14.76	21
	Non-Member	18.53	12.13	75

p-values (Mann-Whitney U test)	**0.626**	**0.092**

Being engaged in these other types of social interactions seems to have no effect on the amount sent to CFM members and non-CFM members. Thus, this supports our claim that the identity of the participant (being a CFM member) and the identity of the receiver are the predominant predictors of trust in this model.

Next, let us have a closer look at returner behavior. As can be seen from Table 4.2, the more Player1 sends, the more is returned by Player 2 on average. Moreover, on average, for every amount sent, at least the initial absolute amount sent was returned on average, and as a result, the mean amount returned (19.89) is higher than the mean amount sent (16.25). A second measure to look at is the share returned, i.e. the amount returned divided by amount received. The mean share returned to both CFM and non-CFM together is 0.37, and shares do not seem to vary much with the amount received. This fraction is similar to the proportions Johnson and Mislin (2011) found in their meta-analysis for most regions (see panel B in Table 4.3), but slightly higher than average trustworthiness in Africa. Hence, amount returned is a decreasing percentage of amounts tripled.

Furthermore, the mean percentage returned drops from 39.75% when returned to CFM to 34.86% in the case of non-CFM. Wilcoxon matched-pairs signed-ranks tests show that for each of the different amounts sent by Player 2 – that is, 10, 20, 30, 40 and 50 – we can reject the hypothesis that the shares returned to CFM and non-CFM are the same (all p-values are below 0.05). Using the amounts returned instead of the shares gives (of course) similar results. Hence we can conclude that there is a significant statistical difference between the shares (or amounts) returned to CFM members and to non-CFM members.

Finally, I examine whether the membership status of Player 2 (being a CFM member or not) affects trustworthiness. To that end, Table 4.7 shows the average shares returned by CFM membership status. CFM members return on average less (30.635) than non-CFM members (38.205). The results of Mann-Whitney U tests show that CFM members and non-CFM members return similar amounts to CFM members, with the exception of the case in which 50 is sent (see Appendix 4). This stands in sharp contrast to behavior towards non-CFM members. For all

possible amounts, the results of Wilcoxon matched-pairs signed-ranks tests show that there is a statistically significant difference in amounts returned to non-CFM members and to CFM members ($p < 0.02$ in all instances, Table 4.2). So the difference in amounts returned by CFM and non-CFM members is entirely driven by differences in trustworthiness towards non-CFM members.

Related to this is the finding that CFM members return significantly different amounts to CFM members than to non-CFM members for all possible amounts sent (such that also the average shares differ significantly, see Table 4.7), whereas, for non-CFM members, there is no statistically significant difference between the amounts returned to both groups.

Table 4.7: Amount returned to CFM and non-CFM member

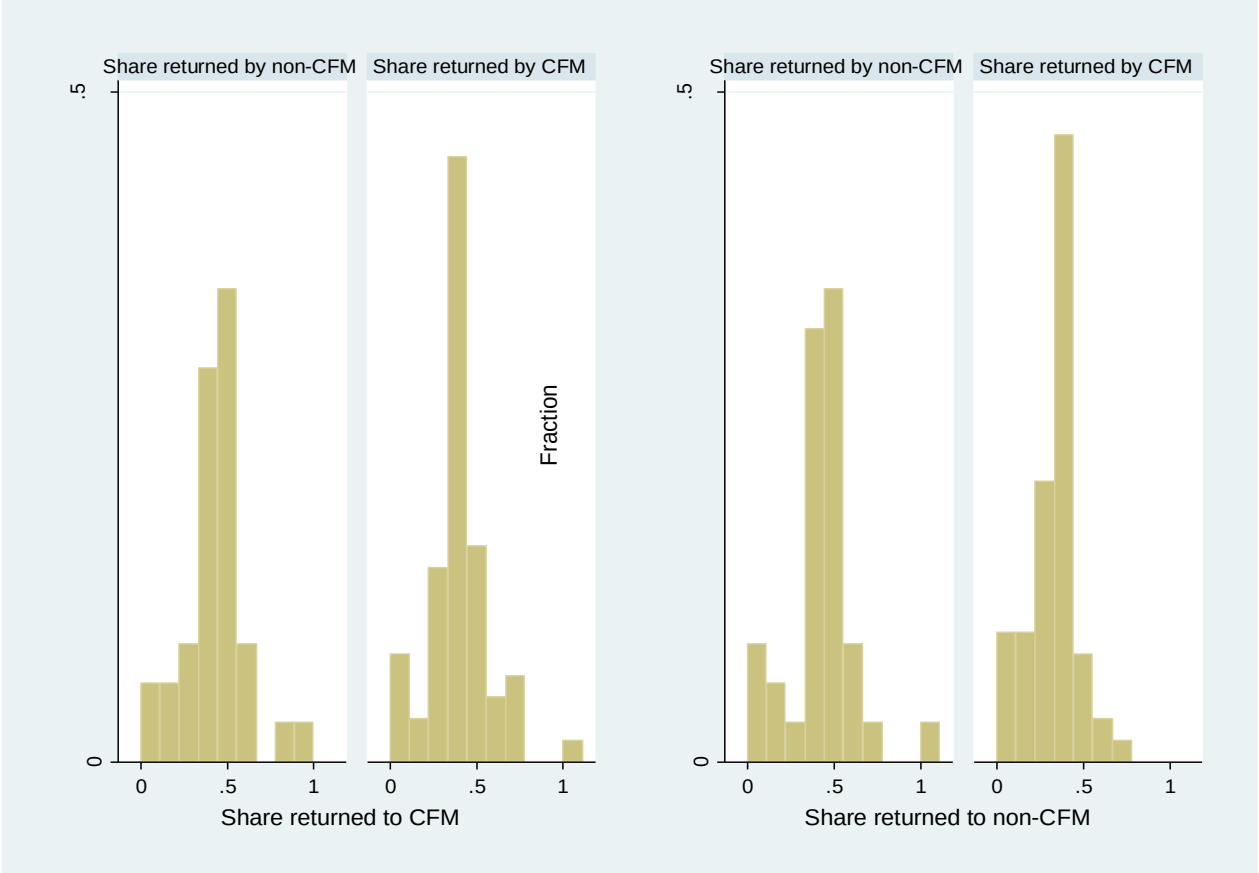
	Average share returned to CFM	Average share returned to non-CFM	P-values (Wilcoxon signed-ranks test)
Average share returned by CFM	0.379 (0.174)	0.311 (0.142)	0.000
Average share returned by non-CFM	0.430 (0.183)	0.420 (0.191)	0.722
P-values (Mann-Whitney U test)	0.147	0.001	
Observations	62	34	

Note: We also have looked at a further classification of the second mover behavior in the following manner; consider amounts returned for all possible amount sent (0, 10, 20, 30, 50, 50) and define the following categories of behavior: Selfish: return always 0, Altruist: return always amount received (i.e. three times amount sent), Equal split: return always half of the amount received, Reciprocal: return at least amount sent, Weakly increasing: return non-decreasing amounts: $R(X) \geq R(X-10)$ where X is amount sent increasing, Other: any other behavior (see Appendix 5 for details). The huge majority behave in a reciprocal manner, or can considered to be conditional cooperators: for any amount sent they return at least that amount. Moreover, the analysis shows that both CFM and non-CFM members reciprocate more when matched with a CFM member than when matched with a non-CFM member.

Graphically and more detailed information on the reciprocation decisions taking into account the role of identity can be seen in Figure 4.3. Figure 4.3 presents the histograms of the

average share returned in the trust game to the CFM and non-CFM members (right panel and left panel, respectively), by non-CFM and CFM members (left figure and right figure, respectively). The distribution of average share returned to non-CFM members (by both CFM and non-CFM members) seems to differ only slightly from the distribution of the amounts returned to CFM. The difference is more observable when the amounts returned to CFM and non-CFM members are examined by the identity of participants (CFM vs. non-CFM). Clearly, CFM members behaved differently towards both parties. Interestingly, the non-CFM also behaved slightly in a different fashion in making the decision of average share returned to CFM and non-CFM members. As a result, the distributions of average share returned to CFM members by CFM and non-CFM members seem to vary. However, that difference is not statistically significant (Kolmogorov-Smirnov test), while the difference in the distributions of average share returned to non-CFM members by both CFM and non-CFM members is statistically significant according to Kolmogorov-Smirnov test.

Figure 4.3: Histogram of average share returned to CFM members and non-CFM members by identity[25]



4.3.3 Discussion

The experimental results above indicate that CFM members send and return more to fellow CFM members than to non-CFM members. Non-CFM members send more to CFM members relative to the amounts sent to non-CFM members too. As a result, people who are considered the cooperative type are also more likely to derive higher payoffs, which is in line with the Ostrom's theory of collective action.

[25] The reported share returned here is the average share returned over all possible amounts sent by the Player 1 (30, 60, 90, 120, 150). Since the strategy method is utilized, the further classification in Figure 3 is the average amount returned to members and non-members by same membership status.

A further analysis of regressions (Table 4.8) seems to confirm that these results are not changed when we control for individual specific characteristics. The results are presented using standard OLS regression, where the standard errors are clustered at the village level.

Table 4.8: Trust regression

	Amount sent to CFM (OLS)	Amount sent to non-CFM (OLS)
CFM member=1	3.635	-2.658*
	(3.127)	(0.795)
Age (>35)	1.161	1.669
	(0.943)	(1.236)
Education (#Years>6)	5.902	3.445
	(3.754)	(4.088)
House type	2.225	-5.841
	(1.586)	(2.525)
Gender	0.713	-3.110
	(1.079)	(1.188)
Income	-0.000338**	0.0000729
	(0.0000483)	(0.000475)
Land size (hectares)	-0.563	0.870
	(1.695)	(0.870)
Village FE	YES	YES
Constant	11.54***	13.76**
	(1.005)	(2.504)
Observations	96	96
Adjusted R^2	0.192	0.096

*Standard errors in parentheses * p < 0.10, ** p < 0.05, *** p < 0.01*

Column 1 of Table 4.8 presents predictors of the amount sent to CFM members. Income seems to explain the amount sent to CFM members. Most importantly, as in the non-parametric tests, the regression (column 1) shows that the amount sent to a CFM member does not depend on membership status. In other words, amounts sent to the cooperators by players of both identities are statistically not different from each other.

Column 2 (Table 4.8) shows predictors of the amount sent to non-CFM members. CFM membership seems to explain the difference in the amounts sent to non-CFM members namely, CFM members sent significantly lower amounts than non-CFM. Hence, being a non-cooperator is correlated with a lower amount sent by the cooperator, thus reflecting low trust towards the non-cooperators.

However, it is not straightforward to make causal inference of CFM membership and the outcome variables. Causality requires experiment-like settings, i.e. taking CFM as a treatment variable with the similar characteristics of individuals across CFM membership balanced. In other words, causality requires CFM membership to be an exogenous variable. If the characteristics of individuals across CFM membership are not similar, which may be the case here (see Appendix 2), they should thus be balanced to claim causality.

In appendix 2, the endogenous regression model is used to test of endogeneity of a binary variable, CFM membership. It does so by allowing for a specific correlation structure between the unobservables that affect the binary variable and the unobservables that affect the potential outcomes. If an unobserved variable affects which group a person gets to be in and affects the outcome as well, we have an endogeneity problem. Table B1 in Appendix 2 presents endogenous regressions. Column 1 and 2 indicate the regressions of amount sent to CFM and non-CFM respectively. The bottom panels of the columns show the first stage regression in which probability of CFM is predicted. While the top panel of the regressions shows the role of being a CFM on the amounts sent. In both column 1 and 2 of the table, the likelihood-ratio tests in the last row indicate that we can reject the null hypothesis of no correlation between the original assignment of CFM variable errors and the amounts sent errors. The negative relationships show that unobservables that raise the amounts sent tend to occur with unobservables that lower CFM membership.

Thus, I weighted the observed characteristics across the CFM group to derive results not

confounded with other variables, obtaining the ATE (Average Treatment Effect). This was done by using inverse probability weight treatment estimators. Using these estimators, which balance covariates across CFM membership, Table 4.9 presents the ATE of being a CFM member on the outcome variables (amounts sent).

Table 4.9: ATE of CFM membership on trust

	Sent to CFM	Sent to non-CFM
ATE		
1vs 0. CFM	-0.479	-4.823**
	(2.337)	(1.930)
POmean		
0. CFM	18.42***	15.72***
	(1.973)	(1.745)
TME1		
Age (>35)	1.246**	1.246**
	(0.541)	(0.541)
House type	-1.478***	-1.478***
	(0.553)	(0.553)
Land size	0.441**	0.441**
	(0.185)	(0.185)
Constant	-0.529	-0.529
	(0.590)	(0.590)
Observations	96	96
Over identification test (Prob > chi2)	0.6805	0.6805

*Standard errors in parentheses * p < 0.10, ** p < 0.05, *** p < 0.01*

Table 4.9 shows the ATE after balancing covariates between the CFM and non-CFM group. Typically, ATE measures the role of a treatment if everyone in the sample is put under that treatment. Column 1 of Table 4.9 indicates that being a CFM member lowers the amount sent to a fellow CFM member by 0.50 cents from the average of 18.42 ETB sent by a non-CFM member. This difference, clearly, is not statistically significant. In the same way, column 2 of Table 4.9 shows that being a CFM member lowers the amount sent to a non-CFM member by 4.82ETB from the average of 15.72ETB sent by a non-CFM member. The difference in the amount sent to a non-CFM member by the two types is statistically significant.

As mentioned above, the ATE in this model is calculated by balancing covariates across the CFM group. We tested whether our new model has indeed balanced the covariates between

the CFM and non-CFM group. The over-identification test of covariates shows that we cannot reject the null hypothesis that states that covariates are balanced (Prob > Chi2 = 0.6805). Therefore, these results support the findings of the non-parametric test that the CFM members tend to send lower amounts in general and much lower amounts to non-CFM members in particular.

Examination of the subjects' behavior with respect to the amount returned (trustworthiness) using regressions is presented in Table 4.10. Table 4.10 also utilizes inverse probability estimators. The ATE after balancing covariates across the CFM group indicates the magnitude of the causal relationship between CFM membership and the share returned.

In column 1 (Table 4.10), where the output variable is the decision of the share returned to a CFM member, the average share returned by fellow CFM members is 0.043 cents lower than an average of 0.43 cents, the share returned by the non-CFM members. Clearly, this difference is not statistically significant, and the average shares returned by the members and non-members do not differ from each other, in line with the non-parametric test. On the other hand, as column 2 of the table shows, the share returned to non-CFM members is 0.11 cents lower than 0.42, the share returned by non-CFM members. Again, CFM members returned on average lower shares compared to the shares returned by non-CFM members. In this case, the difference is statistically significant.

Table 4.10: ATE of CFM membership on trustworthiness

	(1) Returned to CFM	(2) Returned to non-CFM
ATE		
r1vs0. CFM	-0.0428	-0.113***
	(0.0383)	(0.0382)
POmean		
r0. CFM	0.426***	0.424***
	(0.0328)	(0.0335)
Age (>35)	1.246**	1.246**
	(0.541)	(0.541)
House type	-1.478***	-1.478***
	(0.553)	(0.553)
Land size	0.441**	0.441**
	(0.185)	(0.185)

Constant	-0.529	-0.529
	(0.590)	(0.590)
Observations	96	96
Over identification test (Prob > chi2)	0.6805	0.6805

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The reliability of the results (share returned) under this model is examined by conducting the balance test of covariates by CFM membership. The test of over-identification shows that the covariates are balanced. Thus, we cannot reject the null hypothesis that the covariates are balanced (Prob > chi2 = 0.6805). ATE results are in line with the main findings, and confirm that CFM members tend to return lower amounts in general and much lower to non-CFM members. As a result, CFM members earn consistently higher payoffs than non-CFM members. Although the higher payoffs are in line with Ostrom's theory, some other findings on trustee behavior are not. In particular, the alternative hypothesis following from this theory, that cooperative subjects display more pro-social behavior (and thus return more) is not supported.

4.4. Conclusions

This study examines trust and trustworthiness between members of a certain group and non-members (CFM and non- CFM members). In settings like this, different theories can be considered to predict the outcomes of matching a certain group members (both with each other and non- members) for economic decision making.

Since the amount sent and returned are designed within the setting of between group member and non-group member decision making, the prediction of social identity theory (see Tajfel, 1974), could be the strategy taken by the subjects. Social identity theory predicts that people favor their fellow in-group member than non-group member if matched in decision making.

Another way to understand in group-out group biases is to look at if these biases have also their beginning originated in indirect evolution theory. Following numerous evidence of conditional cooperators in public good provision and the implication thereof, Ostrom (2014) recommended a revised theory of collective action. This theory uses the indirect evolutionary approach to examine the evolution of conditional cooperators and the role of pro-social behaviors such as trust in cooperation. In an indirect evolutionary approach, players transform

the objective payoff of a certain decision into an intrinsic value that is in line with their norms, on which they base their final decision.

In the same theory, there are at least two types of agents – cooperators and selfish agents. And where information about the types of agents is provided, the conditional cooperators will consistently receive a higher payoff. Meanwhile, the selfish agents will receive consistently lower payoff since they are not trusted. Because of the higher payoff, only the cooperators will survive with the complete information process (Ostrom, 2000).

This study tested trust and trustworthiness between CFM and non-CFM member by providing complete information on the types of players in the trust game. I find that trust and trustworthiness are indeed affected by both the characteristics of a player on whom these traits are bestowed and the identity of a bestower. Characteristics of the bestowed, such as being a member of a CFM and thus a cooperator, tend to increase trust towards her/him. However, the CFM members seem to not reciprocate as would conventionally be expected, thus exhibiting low trust towards the senders, specifically the non-CFM members.

In summary, I find that CFM members may trust non-CFM members less (and thus send less), but in fact, non-CFM are more trustworthy (return more). Thus, a higher amount is sent to CFM members, which shows that they are more trusted. Interestingly, it seems that the non-CFM members tend to signal the CFM members that they can be trusted by sending an even higher amount than what the fellow CFM member sent on average. However, their trust was not reciprocated. The findings in this study seem to be as predicted by Ostrom's collective action theory, that is, as time passes, the conditional cooperators are trusted more and will consistently receive a higher payoffs, while non-cooperators will receive lower payoffs (because they are less trusted). However, we do not have repeated trust game setting to fully claim whether this strategy is stable in the long run. This can be addressed in future research.

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APPENDICES

APPENDIX 1: Questionnaires and Experimental Design Guidelines

Name of Interviewer _____
Date _____
Code of Respondent _____
Interview Started at _____
Interview Ended at _____

Good morning/afternoon! Thank You in advance for making time for the interview. I am _____, an interviewer for this survey. The survey was designed by Rahel J. Kitessa, who is studying at Tilburg University in The Netherlands. Currently, she is collecting data for research purposes. You are kindly requested to give information that we will use to understand some issues of deforestation. The information you provide could help to

increase the understanding about the problems in utilization of the forest and other related natural resources. Whatever information you provide will be kept strictly confidential.

Part I

General Survey Questions

1. Please indicate your gender.

   a. Female b. Male

2. Please indicate your age group.

   a. 18-25
   b. 26-35
   c. 36-45
   d. 46-55
   e. 56 +

3. Are you the head of your household?

   a. Yes b. No

4. What is the highest level of formal education you received?

   a. No formal education c. Grade 7-8
   b. Grade 1-6 d. Grade 9-12 e. Above grade 12

5. How many members are there in your household?

   a. 1-4 b. 5-7 c. 8-9 d. 10 or more

6. Who owns the house that you are living in?

   a. It is our own house b. It is a rented house

7. Would you please tell me the range corresponding to your total monthly income?

   a. Below 500 Birr c. 500- 1000 Birr
   b. 1001-2000 Birr d. 20001-3000 Birr e. 3001-4000 Birr f. Above 4000 Birr

8. What is/are the source(s) of your income?

   a. Agriculture b. Forest and its products c. Police d. Daily laborer
   e. Business owner f. Other_____

9. Can you please tell me to which ethnicity you belong?

a. Oromo
b. Amhara
c. Other
d. Do not wish to report

10. Do you have land of your own?
    a. Yes
    b. No
11. What is the total land area that you can cultivate (land that you own plus land that you rent)? _____hectare(s)
12. Do you consider your land as quality land?
    a. Yes
    b. No
13. Are you a member of Collaborative Forest Management (CFM)?
    a. Yes
    b. No
14. If your answer for Q. 13 is "yes," why did you become a member of CFM?
    _____
15. If your answer for Q.13 is "no," why didn't you become a member of CFM?
    _____
16. How do you accomplish the farming activities during peak high season including sowing and harvesting time?
    a) Hire labor
    b) Call for Dabo
    c) Do it with members of my household
17. Do you go to Dabo often?
    a. Yes
    b. No
18. Are you a member of Ikub
    a. Yes
    b. No
19. Are you a member of Idir?
    a. Yes
    b. No
20. Were you born in Bale?
    a) Yes
    b) No
    c) Do not wish to report

21. How do you evaluate the seriousness of forest degradation and climate change in the country?
    a) Very serious
    b) Serious
    c) Not that serious
    d) Not serious at all
    e) I don't know
22. Do you think you have an influence on decision-making in managing your forest together with members?
    a) Yes
    b) No
23. Please indicate your religion.
    a. Muslim
    b. Christian
    c. Other
    d. Do not wish to report

Part II: The Experiment's Guidelines

1. **Instruction session:**

Welcome to all of you. Now we will give instructions about a game you will play in a moment. In the game you may earn some money. There are no winners and losers in the game, but how much you earn will depend on how you and other people play the game. The objective of the game is purely research and the money for the game is provided by Tilburg University in the Netherlands.

Now I would like to explain the game to you. In the game there are two groups. Players in one group are called PLAYER 1, players in the other group are called PLAYER 2. During the interview you will not know whether you will be assigned the role of PLAYER 1 or the role of PLAYER 2. Therefore, you will first be asked to make a decision as PLAYER 1. Then you will be asked to make a decision as PLAYER 2.

Once everybody has made their decisions, it will randomly be decided which role you will be assigned. If you are assigned the role of PLAYER 1 your decision as PLAYER 1 will be

implemented. You will be matched with a person who has been assigned the role of PLAYER 2. If you are assigned the role of PLAYER 2 your decision as PLAYER 2 will be implemented. In that case you will be matched with a person who has been assigned the role of PLAYER 1. You will not learn who the person is with whom you are matched, and the other person will not learn your identity either. Nobody will know with whom you play the game and which decisions you have made.

Now, let us look at the decisions you have to make in the game, depending on whether you will be assigned the role of PLAYER 1 or of PLAYER 2. First, we will look at the decision PLAYER 1 has to make. PLAYERS 1 are given ETB50 to start the game with. That means that PLAYER 1 starts the game and decides how much of the ETB50 he or she will send to PLAYER 2. All the money PLAYER 1 does NOT send to PLAYER 2 will be paid to PLAYER 1. All the money PLAYER 1 sends to PLAYER 2 will be tripled by the organizers. Next, PLAYER 2 has to decide how much of the money he or she received that he will give back to PLAYER 1, and how much of the money he will keep for him/herself.

- PLAYER 1 thus receives the amount of money he or she kept at the beginning of the game, plus the amount PLAYER 2 sends back to PLAYER 1.
- PLAYER 2 receives the amount of money he or she received from PLAYER 1 (which was tripled by the experimenter), minus the amount he or she sent back to PLAYER 1.

Consider the following examples.

- Suppose PLAYER 1 keeps ETB40. That means that PLAYER 1 sends ETB10 to PLAYER 2. That amount is multiplied by three, and hence PLAYER 2 receives ETB30. PLAYER 2 can then decide whether to send back ETB0, ETB10, ETB20 or ETB30. PLAYER 1 then receives the amount he or she kept, ETB40, PLUS the amount PLAYER 2 sent back to PLAYER 1. PLAYER 2 receives ETB30 MINUS the amount he or she sent back to PLAYER 1.
- Suppose PLAYER 1 keeps ETB20. That means that PLAYER 1 sends ETB30 to PLAYER 2. That amount is multiplied by three, and hence PLAYER 2 receives ETB90. PLAYER 2 can then decide whether to send back ETB0, ETB30, ETB45, ETB60 or ETB90. PLAYER 1 then receives the amount he or she kept, ETB20, PLUS the amount

PLAYER 2 sent back to PLAYER 1. PLAYER 2 receives ETB90 MINUS the amount he or she sent back to PLAYER 1.

As I said, you do not know which role you will play. Therefore, you will be asked to make decisions as PLAYER 1 and as PLAYER 2, but only one of the decisions will be carried out for real money. After everyone has made their decisions, it is randomly decided whether you will be PLAYER 1 or PLAYER 2. If you are assigned the role of PLAYER 1, you are matched with another person who has been assigned the role of PLAYER 2.

If you are assigned the role of PLAYER 1, then your total earnings in the game equal the amount of money you start out with (ETB50), minus the amount of money you decide to send to PLAYER 2, plus the amount of money PLAYER 2 decides to send back to you.

If you are assigned the role of PLAYER 2, you are matched with another person who has been assigned the role of PLAYER 1. If you are assigned the role of PLAYER 2, then your total earnings in the game equal the tripled amount of money you received from PLAYER 1, minus the amount of money you decided to send back to PLAYER 1.

To make sure you understand the game, we will now show with paper money how the game is played.
(The experimenter walks to the middle, takes money out of an envelope and shows them how the decision is made, and reminds them that it is going to be tripled when sent by PLAYER 1.)
Later we will test each of you individually to see whether you understand the game. Only if you understand the game will you be allowed to play. If you have any questions, please ask now.

2. Implementation of the game

We do not know who you will be matched with. The other person may be a CFM member, or he or she may not be a member of the CFM. Whom you are playing with may matter for your decisions, and hence we ask how much you want to send as PLAYER 1 in case you are matched with a CFM member, and also in case you are matched with a non-CFM member. In

a moment we will do the same when we ask you for your decisions in case you are assigned the role of PLAYER 2.

PLAYER 1:

1. Suppose you are PLAYER 1 (which means you are given 50 birr)
   (a). How much are you willing to send to PLAYER 2 if PLAYER 2 is a CFM member (0 10 20 30 40 50)?
   (b). How much are you willing to send to PLAYER 2 if PLAYER 2 is a non-CFM member (0 10 20 30 40 50)?

PLAYER 2:

2. Suppose you are PLAYER 2 and you are returning money from the amount sent to you by PLAYER 1.
   Suppose PLAYER 1 is a CFM member; how much are you willing to return if PLAYER 1 sends you

   I. 10 (then we will multiply it by 3) which means that you receive 30. In this case how much are you willing to send back (0 10 15 20 30)?
   II. 20 (then we will multiply it by 3) which means that you receive 60. In this case how much are you willing to send back (0 20 30 40 60)?
   III. 30 (then we will multiply it by 3) which means that you receive 90. In this case how much are you willing to send back (0 30 45 60 90)?
   IV. 40 (then we will multiply it by 3) which means that you receive 120. In this case how much are you willing to send back (0 40 60 80 120)?
   V. 50 (then we will multiply it by 3) which means that you receive 150. In this case how much are you willing to send back (0 50 75 100 150)?

3. Suppose you are PLAYER 2 and you are returning money from the amount sent to you by PLAYER 1.
   Suppose PLAYER 1 is a non-CFM member; how much are you willing to return if PLAYER 1 sends you

   I. 10 (then we will multiply it by 3) which means that you receive 30. In this case how much are you willing to send back (0 10 15 20 30)?
   II. 20 (then we will multiply it by 3) which means that you receive 60. In this case how much are you willing to send back (0 20 30 40 60)?
   III. 30 (then we will multiply it by 3) which means that you receive 90. In this case how much are you willing to send back (0 30 45 60 90)?
   IV. 40 (then we will multiply it by 3) which means that you receive 120. In this case how much are you willing to send back (0 40 60 80 120)?

V. 50 (then we will multiply it by 3) which means that you receive 150. In this case how much are you willing to send back (0 50 75 100 150)?

Card for Random Matching

Card no._____

Respondent is CFM member y/n _____

Respondent is matched with partner with Card no. _____

Matched with partner that is CFM member y/n _____

Assigned role of respondent (circle it): PLAYER 1 PLAYER 2

Amount to be sent to CFM _____

Amount to be sent to non-CFM_____

Amount returned for all possible amounts sent by PLAYER 1 **before** tripling

CFM	10	Non-CFM	10
	20		20
	30		30
	40		40
	50		50

Final earnings for respondent: ____

APPENDIX 2:

Table B1: Endogenous regression of CFM on the amounts sent

	(1) AmSent1	(2) AmSent2
Age (>25)	-0.943	0.605
	(2.637)	(2.085)
Education (#Years>6)	9.264***	5.421
	(3.486)	(3.311)
House type	3.615	-5.030**
	(2.592)	(2.164)
Income	-0.000227	0.000121
	(0.000499)	(0.000440)
Land size (hectares)	-1.493	0.198
	(1.327)	(1.055)
CFM member=1	12.71**	3.323
	(6.177)	(4.470)
Village FE	YES	YES
Constant	7.659**	8.212***
	(3.816)	(3.040)
CFM member=1		
Age (>25)	0.695**	0.695**
	(0.335)	(0.335)
Education (#Years>6)	-0.986***	-0.986***
	(0.318)	(0.318)
House type	-0.610	-0.610
	(0.392)	(0.392)
Income	-0.0000445	-0.0000445
	(0.0000777)	(0.0000777)
Land size (hectares)	0.383**	0.383*
	(0.182)	(0.182)
Village FE	YES	YES
Constant	-0.0713	-0.0814
	(0.381)	(0.381)
athrho		
Constant	-0.642	-0.642
	(0.404)	(0.404)
Observations	96	96

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

APPENDIX 3**:**

Participatory Forest Management Achievement in Adaba_Dodola											
S/ no	Woreda	Pas	CBO Names	Year of est.	No. kebeles	No. of block s	Area in ha	Participant s			
								Male	Femal e	Tota l	Dependents
1	Dodola	Berisa	Berisa	1992	1	6	2645.00	137	21	158	772
2	Dodola	Deneba	Danaba	1993	1	15	5479.00	238	65	303	2051
3	Dodola	Bura_Addele	Bura-Calle	1995	1	10	3419.00	424	132	556	2981
4	Dodola	Bura_Addele	Addelle	1995		12	6159.00	223	81	304	1364
5	Dodola	Kechema	Kachema	1998	1	10	2596.00	219	29	248	2010
6	Dodola	Har_Geneta	Geneta	1998	1	4	1066.00	91	25	116	544
7	Dodola	H.Daakiyie	Hara-Dakiyie	1998	0	7	2791.00	140	40	180	1327
8	Dodola	Keta_berenda	WAJIB	1999	1	9	3408.00	191	60	251	1964
9	Dodola	Alentu_dobedo	Dobado-kuse	2002	1	10	2812.00	204	87	291	1552
10	Dodola	Alentu_dobedo	Hara-Bubftu	2002		10	2776.00	203	85	288	2776
11	Dodola	Ashena_Robe	Ashena	2002	1	4	13976.00	94	25	119	708

12	Dodola	Aluma_shifa	WAJIB	2003	1	6	5049.00	138	42	180	1084
13	Dodola	A.H.kaanku	Oda Amba Kanku	2005	1	2	809.00	242	37	279	1953
14	Dodola	Gafarsa-kaarra	Gafarsa	2005	1	3	7`65.48	182	62	244	1708
15	Dodola	Morke	Silencho Morke	2005	1	7	667.02	137	41	178	1246
16	Dodola	Biqiqaa	Burkitu Bikika	2007	1	2	7288.02	156	33	189	1323
17	Dodola	Maqaaliitu	Mekalitu Ode	2007	0	1	5606.97	96	20	116	812
		Subtotal					***66547.01***	***3115***	***885***	***4000***	***24040***
1	Adaba	Ejersa_Chumulugo	**Walta'ii Guddinaa**	1996	1	7	1886.00	123	26	149	1043
2	Adaba	Bubisa	**Bubbisaa Nageellee**	1996	1	10	8771.00	247	52	299	2093
3	Adaba	Bucha	**Walta'ii Barii**	2002	1	3	1170.00	75	15	90	630
4	Adaba	K-witicho	**Wiixichoo**	1998	1	4	1480.00	93	11	104	728
5	Adaba	Wege	**Handhura Weegee Siree**	2005			8535.00	385	35	420	3780
6	Adaba	Cofra	**Coofira Amaallamaa**	2005			3625.00	206	17	223	2007
	Adaba	Bucha	**Busooftuu Handhuraa**	2003	1	4	1239.00	89	31	120	840
	Adaba	Bubisa	**Bubbisaa Nageellee**	2003	1	3	1980.00	66	24	90	630

7	Adaba	Lenca/washaa	**Anshuumoo Dhagabooraa**	2004	1	4	2889.00	529	16	545	5815
8	Adaba	Koma	**Qoomaa Abaasaa**	2004	1	3	1420.00	461	12	473	3311
	Adaba	Bucha3	**Roobee Raayyaa**	2005			3947.70	53	9	62	558
9	Adaba	Sole	**Lodeedoo Xaxayeenshoo**	2006			885.40	223	11	234	2106
10	Adaba	Ossee-xonsicho	**Ossee Xoonsichoo**	2007			8494.00				
11	Adaba	Gaama wallaallee	**Gaamaa Gamiroo**	2006			11307.00	226	21	247	2223
		Subtotal			***8***	***38***	***57629.10***	***2776***	***280***	***3056***	***25,764***
S/ no	Woreda	Pas		Year of est.	No. kebeles	No. of block s	Area in ha	Participant s			
								Male	Femal e	Tota l	Dependents
1	Kokosa	Bokore	Bokore	2003	1	7	1215.00	50	5	55	385
2	Kokosa	Dayu	Dayu	2003	1	3	72.00	47	4	52	357
3	Kokosa	Kaawo	Kaawo	2003	1	5	1784.00	45	6	51	357
4	Kokosa	Wayyo	Wayyo	2006	1		255.49				
5	Kokosa	Diki Hora	Diki Hora	2006	1		95.00				
6	Kokosa	Diki Gaxa	Diki Gaxa	2006	1		262.66				

7	Kokosa	Jidha	Jidha	2006	1		284.64				
8	Kokosa	Garba Hurufa	Garba Hurufa	2006	1		251.18				
					8	***15***	***4219.97***	***142***	***15***	***158***	***1099***
1	Nansabo	Huro_Baro	Huro_Baro	2002	1	6	1885.00	321	12	333	2331
2	Nansabo	Shambel_Kadir	Shambel_Kadir	2002	1	7	2880.00	402	25	427	2989
3	Nansabo	Tulu_Lenca	Tulu_Lenca	2002	1	5	1087.00	226	6	232	1624
4	Nansabo	Korema	Korema	2003	1	3	588.00	86	2	88	616
5	Nansabo	Mandoyu	Mandoyu	2003	1	4	5434.00	108	5	113	791
6	Nansabo	Bulga	Bulga	2003	1	2	353.00	98	4	102	714
7	Nansabo	Gerembamo	Gerembamo	2003	1	7	12145.00	97	21	118	826
8	Nansabo	Nansabo_chabi	Nansabo_chabi	2004	1	3	11259.30	313	2	315	2205
9	Nansabo	Refisa	Refisa	2004	1	3	7158.00	176	6	182	1274
10	Nansabo	Melka_Dembi	Melka_Dembi	2006	1	2	6748.20	207	7	214	1498
11	Nansabo	Bohera	Bohera	2007	1		5034.25	0	0	0	0
12	Nansabo	Habera	Habera	2006	1		14760.85	0	0	0	0
13	Nansabo	Gorxe	Gorxe	2006	1		20891.00	0	0	0	0
14	Nansabo	Bedesa	Bedesa	2007	1		9460.00	0	0	0	0
15	Nansabo	Gemechu	Gemechu	2007	1		3538.30	0	0	0	0

Summary	

16	Nansabo	faca'a	faca'a	2006	1		14877.13	0	0	0	0
17	Nansabo	Roricho	Roricho	2006	1		10127.00	0	0	0	0
18	Nansabo	Riripha	Riripha	2006	1		8138.84	0	0	0	0
19	Nansabo	Gata	Gata	2007	1		2818.30	0	0	0	0
		Subtotal	*56*				***139183.17***	*2034*	*90*	2124	*14868*
		Grand total					***267579.25***			***9338***	

Total hectares_234,633.3.8ha	
From 1992-1999 (with support of GIZ)_36,3882 ha	
From 2000-2001 (under woreda Agr. office) 0 ha	
From 2002-2005 (under OFWE support) 174,388.10 ha	
REDD+= 75984.9 ha (53131.9 ha humid forest + 22853 ha dry evergreen mountain forests)	

APPENDIX 4: Mann-Whitney U test of amount returned by CFM membership

	Returned by CFM member	Returned by non-CFM member	p-values (Mann-Whitney U tests)
Share amount returned			
if trustor sends 10	0.405 (0.206)	0.441 (0.238)	0.3205
if trustor sends 20	0.365 (0.197)	0.442 (0.202)	0.3936
if trustor sends 30	0.388 (0.210)	0.417 (0.214)	0.2367
if trustor sends 40	0.384 (0.208)	0.441(0.192)	0.2521
if trustor sends 50	0.351 (0.193)	0.437 (0.217)	0.0118
Number of Obs.	62	34	

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

APPENDIX 5:

Table A5: Second mover classification based on return decisions

	Second Mover behavior when matched with			
	CFM member	Fraction	Non-CFM member	Fraction
Selfish	3	0.03	4	0.04
Altruist	1	0.01	1	0.01
Equal Split	5	0.05	4	0.04
Reciprocal	64	0.67	59	0.61
Weakly increasing	3	0.03	3	0.03
Other	20	0.21	24	0.26
	96		96	

Note: If people belong to more than one category they were put in the classification of the order above (e.g altruists are also reciprocal, but are only classified as altruists).

Here the second movers behavior is classified based on the amount they return, and I do this separately for returns to CFM members and to non-CFM members. Consider amounts returned for all possible amount sent (0, 10, 20, 30, 50, 50) and define the following categories of behavior:

- *Selfish: return always 0*
- *Altruist: return always amount received (i.e. three times amount sent)*
- *Equal split: return always half of the amount received*
- *Reciprocal: return at least amount sent*
- *Weakly increasing: return non-decreasing amounts: R(X) ≥ R(X-10) where X is amount sent increasing*
- *Other: any other behavior*

The table A5 shows that the huge majority behave in a reciprocal manner, or can considered to be conditional cooperators: for any amount sent they return at least that amount. This is consistent with results of many trust games. Note, however, that the share returned is not increasing in the amount sent (see Table 2 of the main paper). A non-substantial fraction also appear to divide the surplus equally (equal split) but the other categories are all very small, apart from a rather large group of subjects who display behavior that does not fully fall in one of the other categories. When comparing the results for the two cases, the table suggests that pro-social behavior is observed a bit more when a player is matched with CFM member, but the differences are small.

A further examination of above behavioral categories split according to one's own type (CFM membership status) and other player's type is shown in Table B5. Table B5 shows that both CFM and non-CFM members reciprocate more when matched with a CFM member than when matched with a non-CFM member. At the same time, the real CFM members are, in general, slightly less reciprocal than non-CFM members.

Table B5: Second mover classification by decision behavior and CFM membership

	Selfish	Altruist	Equal Split	Reciprocal	Increasing	Other behavior	Obs.
Behavior of second movers matched with CFM							
CFM member	0.0484	0.0161	0.0484	0.629	0.0484	0.21	62
Non-CFM member	0	0	0.0588	0.735	0	0.206	34
Behavior of second movers matched with non- CFM							

	Selfish	Altruist	Equal Split	Reciprocal	Increasing	Other behavior	Obs.
CFM member	0.0484	0	0.0161	0.581	0.0484	0.306	62
Non-CFM member	0.0294	0.0294	0.0882	0.676	0	0.147	34

Note: If people belong to more than one category they were put in the classification of in the order above (e.g altruist are also reciprocal, but are only classified as altruists).