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The Poor and Their Neighbors: Essays on Behavioral and Experimental Economics

Haileselassie Medhin

UNIVERSITY OF GOTHENBURG

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To the farmers who participated in my experiments

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Haileselassie Medhin
Gothenburg, September 2013

Brief Overview of the Thesis

This thesis comprises of seven self-contained papers. While the papers are quite distinct in the questions they address and each is based on its own dataset, there are some relations between them either in the topic covered, theories applied or empirical methods used. Generally put, the first five papers focus on the application of behavioral and experimental economics to the livelihood and behavior of poor households in developing countries. While the sixth paper also utilizes experimental data from poor farmers, it mainly focuses on subject pool issues related to the generalization of results from laboratory experiments. The seventh paper experimentally compares the cooperative behavior of individuals and teams. Table 1 presents a summary of the isses explored in each paper, together with the behavioral themes in focus and the experimental methods used. The purpose of this overview is to briefly describe the key issues, concepts, methods and findings in each paper.

Table 1. Research topics and empirical methods

Study	Issues in focus	Behavioral topics	Empirical methods
Paper 1	Land conflicts among small-scale farmers	Social dilemmas and inequality aversion	Framed lab-in-field experiments with farmers
Paper 2	Externalities of social learning in small-scale agriculture	Coordination problems and inequality aversion	Lab experiments with students
Paper 3	Positional concerns among the poor	Positional concerns	Survey experiments with farmers
Paper 4	Positional concerns among the poor - reference groups	Positional concerns	Survey experiments with urban dwellers
Paper 5	Attitudes toward uncertainty among poor farmers	Risk aversion and ambiguity aversion	Lab-in-field experiment with farmers
Paper 6	Subject pool issues in laboratory experiments	Inequality aversion vs. efficiency motives	Lab-in-field experiments with farmers; lab experiments with students
Paper 7	Cooperative behavior of teams vs. individuals	Rationality and learning	Lab experiments with students

In most of the development-related issues explored in this thesis, the focus is on the *interaction* of households with other households in their vicinity - broadly referred to as 'neighbors' in the title. More specifically, we are interested in interactions that create *interdependencies* in the welfare of different households, which in turn affect the choices that the households make. Sometimes, this happens when households are involved in a strategic situation, where the action of a household directly affects the welfare of other households, and vice versa. That is, households could be playing some sort of game with each other. For example, **Paper 1** deals with a 'land grabbing game' among neighboring farmers who share a vaguely defined border. **Paper 2** deals with a 'waiting game' among farmers who seek to learn from each other about new technologies. Other times, welfare interdependencies among households could happen in a more subtle way, as in **Paper 3** and **Paper 4**, where households' welfare can be affected not only by what they have, but also by what they have relative to others.

Welfare interdependencies among households have the potential to create economic inefficiencies: vaguely drawn land borders could lead to devastating conflicts among neighboring farmers; neighbors with a strong desire to learn from each other's experience with new technologies could be locked in a waiting game that results in very little relevant experience; or concerns about status could lead to the waste of limited resources on unproductive conspicuous expenditure. But there are often also opportunities for good outcomes to arise. For example, with good institutions, farmers who share a vague border can cooperate and establish clear borders, thereby enhancing their tenure security and friendship. Similarly, if circumstances allow them, neighbors can join hands to experiment with new technologies, or even to innovate.A major objective of the research in this thesis is to understand individual behavior and welfare in such circumstances, and to generate ideas that might help in developing policy tools to deal with potential inefficiencies. This is done by drawing lessons from recent developments in behavioral economics, and by examining experimental data. The reliance on behavioral economics is mainly motivated by the fact that the aforementioned interactions among households involve behavioral dimensions that traditional economic models of the household rarely account for, such as fairness and relative concerns.

One of the key contributions of behavioral economics has been the modeling of *social preferences*. The concept of social preferences is quite broad, and summarizes be-

havioral phenomena where individual decisions are motivated not only by their own wellbeing, but also by that of other individuals. Models of social preferences incorporate motivations such as altruism, inequality aversion, reciprocity, and guilt aversion into economic theory, thereby providing explanations for the many instances where individual behavior deviates from pure self-interest. A huge amount of experimental research now shows that social preferences are indeed an important aspect of individual behavior in many situations, especially when there are welfare interdependencies. A concept of social preferences that is extensively used in this thesis is *inequality aversion*. Models of inequality aversion postulate that individuals dislike outcomes that result in unequal benefits, and hence adjust their choices accordingly. Inequality-averse individuals are assumed to suffer from having less or more than others, but perhaps suffer much more if they have less. This simple concept of fairness is applied in **Paper 1** and **Paper 2**, each dealing with a specific form of strategic interaction among small-scale farmers. **Paper 6** also focuses on inequality aversion mainly from a methodological angle.

Land holdings in many developing countries are not fully demarcated. This often leads to land conflicts among farmers. Such conflicts not only result in the waste of resources (and sometimes life), they also worsen perceptions of tenure security. It is therefore important to develop cost-effective mechanisms that help farmers avoid such conflicts and move toward a peaceful demarcation of their border. **Paper 1** explores how insights from behavioral economics and game theory can be used to develop such mechanisms. We fist show that a seemingly social dilemma problem – like the land grabbing game among neighboring farmers who share a vague border - could in fact be a coordination problem if the neighbors are inequality averse. This insight is then used to develop a cheap policy intervention that helps neighbors coordinate in cooperative outcomes. The basic idea is that, when inequality-averse neighbors are involved in a social dilemma situation, a credible but, a costly (to the farmers themselves) optional mechanism that can be used to insure against aggression can actually result in cooperation. Such a mechanism is cost-effective because, if it works, it will not actually be used: its mere presence is what helps farmers avoid the conflict and focus on cooperation. The relevance of this 'non-interventionist' policy is then tested using a framed lab-in-the-field experiment in the Ethiopian highlands, a place with high prevalence of land conflicts. The experimental results show a guarded support for the theory, but strong enough support that there is a potential for social-preference-

based policy interventions in dealing with social dilemmas. A relevant application of this approach would be, for example, to make slight adjustments in the current implementation of the land certification program in Ethiopia.

Paper 2 explores the link between social learning and experimentation in the process of technology adoption in small-scale agriculture. The basic problem here is that, when farmers have the possibility of learning from the experimentation of other farmers around them, they have the incentive to limit their own experimentation activities. In other words, social learning creates the possibility of free-riding. These incentives to free-ride have the potential to discourage experimentation and delay the adoption of good technologies. The paper models this problem as a coordination game between neighboring farmers who can learn from each other's experimentation. It is first shown that the specific properties of the game depend on the characteristics of experimentation, especially on whether it is possible to share the experimentation burden. While a coordination problem exists in each case, it is shown that the prospect of coordination is better when the experimentation burden is divisible, which in turn suggests room for policy interventions to divide the burden. This hypothesis is then tested using lab experiments that replicate the coordination games. All in all, the experimental results support the hypothesis, and indicate that, when neighbors are fairly homogenous, the net effect of social learning is negative when it is not possible to share the burden of experimentation.

The problem is then further examined through the lens of inequality aversion. It is shown that heterogeneities in attitudes toward inequality can serve as a means of coordinating to achieve an efficient outcome, even when experimentation is not divisible. Perhaps surprisingly, it is shown that it is the 'more selfish' who do the experimentation and the 'less selfish' who free-ride in such a situation. Since the low level of adoption of new technologies is a major reason for the stagnant agricultural productivity in many developing regions, an increased focus on the incentives for on-site experimentation seems warranted.

Another behavioral concept explored in the thesis is *positional concerns* (also termed '*relative concerns*', '*concern for status*' or just '*positionality*'). The concept of positional concerns, sometimes thought of as an aspect of social preferences, focuses on situations where the utility of individuals is not only a function of their own income or consumption, but also the function of their consumption/income relative to other people

in their *reference group*. Simply put, positionally concerned individuals suffer if inequality in consumption or income goes against them, and hence would prefer to be at the top with low absolute consumption/income than to be at the bottom with high consumption/income. This insight has been used in the literature to design choice experiments that can help elicit people's *degree of positionality*, i.e., to what extent they would be willing to sacrifice absolute consumption to change the distribution in their favor. The overall finding from this line of research is that there is considerable concern for positionality among Western societies, while the evidence from the relatively few studies on developing countries is mixed. **Paper 3** and **Paper 4** contribute to this literature by presenting evidence on positional concerns from a relatively poor population. Both papers use a stated-preference experiment, where people are asked to choose between living in different 'societies' that vary in terms of their individual income and the income of a reference group. In addition to increasing the horizon of empirical tests on the topic, understanding the degree of positional concerns in low income places could be important for the design of policies and aid interventions. Aid interventions often target a limited number of people, such as model farmers. If positional concerns are strong in such places, interventions that increase the income of some individuals could 'punish' others. Positional concerns could also push poor people to spend their limited wealth in unproductive expenditures.

Paper 3 explores positional concerns among poor farmers in Northern Ethiopia where the reference group is 'other people in the village'. In addition to just income, people are asked to choose between different aid-sponsored productive packages that bring varying benefits to them and others in their village. The results show that the concern for positionality is extremely low among Ethiopian small-scale farmers. Most people are not willing to lower their absolute level of income to improve their relative standing. For example, they vote for an aid project that brings higher average benefits to their village even if their own benefit from the project is less than average. **Paper 4** mainly focuses on the issue of reference groups. The motivation is to check whether the low positionality observed in some studies of poor societies, including our study in **Paper 3**, is driven by the mis-specification of the reference group. We therefore undertake survey experiments where subjects compare themselves with an array of reference groups, such as friends, relatives, neighbors, etc. The over-all finding is still that positional concerns are very low, and are stable across different reference groups. All in all, the results in the two papers indicate that there is little to worry

about negative externalities of aid interventions to non-beneficiaries.

In addition to welfare-relevant interactions with others, poor farmers also have to deal with uncertainties in production and consumption created by nature, market conditions, or new farm technologies. Their attitudes toward such uncertainties affect their choices, such as their openness to new technologies. **Paper 5** presents an experimental study on this issue, specifically focusing on the relevance of the distinction between *risk aversion* and *ambiguity aversion*. We argue that farmers have limited knowledge about the likelihood of 'good' and 'bad' outcomes related to most new technologies, making ambiguity aversion, rather than risk aversion, the relevant concept. Focusing on risk aversion, as has been the case in the technology adoption literature, can therefore be misleading if farmers have different attitudes toward risk and ambiguity. We find that this is indeed the case. We also compare the behavior of farmers with European university students and find that ambiguity aversion is similar, while Ethiopian farmers are more risk averse.

Inequality aversion is re-visited in **Paper 6** from a slightly different perspective. The focus here is on the relative importance of inequality aversion and efficiency in decision making, especially when people make allocation decisions that solely affect other people. Understanding people's relative preference toward equality and efficiency is important because many real world decisions involve trade-offs between equality and efficiency (e.g., distributional taxation, distribution of aid, etc.). This issue is experimentally explored in the context of an ongoing debate on the external validity of results from laboratory experiments, especially those based on student subject pools. There have been diverging views about the extent to which experimental results based on university students can be generalized. On the one hand, there are studies stressing that Western university students, who are the subjects in most experiments, are part of a unique population that is hardly representative of the average human being. On the other hand, there are studies arguing that there is not much reason for concern about the representativeness of student subject pools as long as the purpose is to test general economic theories.

Our study is based on the belief that the right approach is to try and replicate experiments in different populations, and to draw lessons on potential biases that can be created by population-specific attributes. To demonstrate this, we replicate an experiment that has been at the center of the equality-efficiency debate, but in this case

use a subject pool of Ethiopian farmers and Ethiopian economics students. We find that Ethiopian farmers are more strongly motivated by inequality aversion than by efficiency. A comparison of our results with previous experimental results from Europe shows that there is no significant difference between Ethiopian farmers and European non-economics students, while Ethiopian economics students behave much like European economics students.

Paper 7 uses public good experiments to compare cooperative behavior of individuals and teams, and investigates how team-decision experience affects individual behavior. The issue is important because many cooperative decisions in the real world are undertaken by a collection of individuals, such as communities negotiating the use of a resource with other communities. Previous experimental research has shown that teams exhibit behavior that is much closer to what is predicted by standard economic models of a rational, self-centered decision-maker. A number of explanations are provided for this in the literature, such as increased cognitive ability because of multiple brains, increased strategic thinking triggered by within-team communication; and limited relevance of fairness concerns such as inequality aversion for teams. Understanding differences in the behavior of teams and individuals is therefore very important for design of policies based on social preferences, such as our non-interventionist land conflict avoidance tool in **Paper 1**. Our basic result is in line with the existing literature: we find that teams are more likely to be free-riders (i.e., they are more rational) than individuals.

The novelty in our study is that we have data on each subject deciding both as an individual and as a team. Moreover, some subjects decide as an individual first and others decide as part of a team first. Examining such data sheds more light on what is driving team rationality, and on the impact of team-decision experience on subsequent individual decisions. We find that free-riders are more influential than conditional cooperators (i.e. those who cooperate if they know others are also cooperating) in team decisions. It appears that team decisions give free-riders a chance to successfully promote their rationality. Moreover, individuals learn to free-ride from team-decision experience. That is, free-riders are more likely to 'convert' conditional cooperators than vice versa. A rich area of research shows that institutions such as punishment enhance cooperation among individuals. Future research should explore the relevance of such mechanisms in improving team-to-team cooperation.

All in all, the research in this thesis shows that there is much to learn from recent developments in behavioral and experimental economics regarding important issues in developing countries, such as natural resource management and technology adoption. The livelihood of many people in developing countries is intertwined with each other and their environment. New insights from behavioral economics are becoming increasingly useful in understanding the complexities in such systems, and developing tools that promote technology adoption and enhance cooperation in the management of important resources like land and forests. Experimental economics is complementing these efforts by opening for new and reasonably cheap possibilities of evaluating policies and interventions. This thesis aspires to contribute to these exciting developments in the field of economics.

Paper I

Thanks but No Thanks: A New Policy to Reduce Land Conflict*

Martin Dufwenberg[1,2,3] Gunnar Köhlin[3] Peter Martinsson[3]
Haileselassie Medhin[3]

[1]Bocconi University [2]University of Arizona [3]University of Gothenburg

Abstract

Land conflicts in developing countries are costly. An important policy goal is to create respect for borders. This often involves mandatory, expensive interventions. We propose a new policy design which in theory promotes neighborly relations at low cost. A salient feature is the option to by-pass regulation through consensus. The key idea combines the insight that social preferences transform social dilemmas into coordination problems with the logic of forward induction. As a first, low-cost pass at empirical evaluation, we conduct an experiment among farmers in the Ethiopian highlands, a region exhibiting features typical of countries where borders are often disputed.

Keywords: Conflict, land grabbing game, social preferences, forward induction, Ethiopia, experiment, land reform

JEL Classification: C78; C93; D63; Q15

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1 Introduction

Property rights, trust, and peaceful relations with neighbors are important to individuals' willingness to invest in their land and to economic prosperity.[1] Lack of institutions that secure property rights for land has been deemed a fundamental reason why many sub-Saharan African countries remain comparatively poor (Knack & Keefer, 1995; Goldsmith, 1995; Acemoglu et al., 2001). An important goal for development assistance is therefore to develop cost-effective means to help define and ensure respect for property.[2] We bring to the table a design feature of how such interventions could be implemented in a way that encourage cooperation, make the interventions cheaper and reduce conflict.

Poorly defined tenure rights can also contribute to land related conflicts. During the last decades, there has been an increase in land conflicts in sub-Saharan Africa (Peters 2004). Interestingly enough, the conflict implications of the structure of land property rights has often been neglected in the design and implementation of land reform policies. It is even argued that land policy and titling programs have exacerbated conflicts (Peters, 2009). Land conflicts in rural areas can take many forms: between communities, between farmers and investors or the state, and between farmers themselves. We focus on farmer-to-farmer land conflicts. At first glance, such situations resemble dilemma games, in which individual rationality conflicts with social efficiency. One way to avoid conflict is to use state enforcement power to provide all those services that can ensure peace: detailed surveying and registration and then police, courts, judges, legal counsel, etc. With some local variations, this is the strategy now embraced by many governments and donors as part of mandatory land titling programs. But that can be costly.[3] Our proposal, by contrast, would allow farmers to choose between external enforcement and cooperation. This relies on farmers to vol-

[1] The relevant literatures in support of these claims are too numerous to attempt any serious survey; see e.g. Skaperdas (1992) on property rights, Besley (1995), Friedman et al., (1988), Hayes et al. (1997), Gebremedhin & Swinton (2003), Smith (2004), Deininger & Jin (2006), Goldstein & Udri (2008), Mekonnen (2009) on the role of tenure for investments and agricultural productivity, and Knack & Kiefer (1997) on trust. Witness the developments in Rwanda in 1994 for an example of how things can go terribly wrong when neighbor relations are not peaceful (André & Platteau 1998).

[2] The World Bank has recently stressed the need for research that evaluates the impacts of such reforms, including their cost-effectiveness. Deininger et al. (2011) is an example of such research.

[3] The cost of registration per plot varies greatly. At one end of the spectrum, we have Indonesia where a title costs about $80 (Grimm & Klasen, 2009). At the other end, we have Rwanda (Ali et al., 2011) and Ethiopia (Deininger & Jin, 2006; Deininger et al., 2008) where each certificate costs about $1.

untary restrain themselves from laying claim to their neighbors' land, thus fostering an environment of trust and reduced conflict.

The key idea combines recent work in behavioral economics, on social preferences, and somewhat less recent work in game theory, on forward induction. We first argue that land-grabbing games may actually not be social dilemmas. If the involved parties care about other things than their own material gain (as recent work in behavioral and experimental economics suggests) then the situation is best thought of as a coordination game with multiple Pareto-ranked equilibria. We then introduce our proposal which tweaks-by-policy the land-grabbing game such that a forward induction argument generates coordination on a good outcome. Our suggestion is not to impose mandatory government regulation and control as a means to securing property rights and respect for borders, but rather to have this be a costly option which farmers can forgo. If government-mediated intervention is actively rejected, this signals the intention and expectation that subsequent play will conform with a cooperative pattern.

It would be incorrect to say that our proposal does not concern costly government intervention at all. It involves counterfactual costly government intervention. Intervention is feasible but shunned, and hence no actual intervention cost is incurred. In reality, the government will always need to ensure at least a minimum of legal institutions. This makes the government intervention credible. Still, by allowing for cooperation, the cost to these institutions could be reduced substantially. There is a well-documented allegory to such cooperation in Lin Ostrom's design principles for long-enduring Common Pool Resource institutions (1990) and in Ostrom et al. (1992). Ostrom shows that cooperation in management is possible, and that individuals can make credible commitments and achieve higher joint outcomes without an external enforcer, given conducive institutional settings.

The formal articulation of our ideas is the first contribution of our paper. We view such arm-chair reasoning as valuable per se. However, empirical relevance should not be taken for granted. A second goal of our study is to take first steps toward testing the proposal in practice. To that end, we report the results from a framed field experiment.

The design mixes abstract and realistic features.[4] We rely on an experimental game directly reflecting the behavioral theory we test rather than on allotments of real land.

[4]See Harrison & List (2007) for a discussion of various features of field experiments.

This has the advantage of being affordable. While the game is more abstract than a true land conflict setting, the payoffs are designed to resemble those relevant in the field. In other dimensions the setup is close to that of actual developing economies. We conducted the experiment in the Amhara Region located in the Ethiopian highlands, where borders are often not well defined and often disputed. The current government has ambitions to engage in land certification procedures whereby farmers obtain formal user-right status. Our subjects are farmers from this area, and the game they play is described by drawing realistic analogies to local conditions concerning land borders and conflicting neighbors' claims. We conducted our experiments in villages with relatively high and low levels of reported land conflicts.

This study thus proposes a specific and comparatively inexpensive form of policy that may help to define land property rights and to promote respect for borders. The salient features of this policy would be the availability of a Divider institution and the option to by-pass this Divider for a cooperative solution. Such a policy is particularly relevant when the government formally owns the land but tenure rights are about to be individualized.

Section 2 tells the game-theoretic story that serves as the formal foundation of our policy proposal. Section 3 describes the experimental design and results. Section 4 offers a concluding discussion.

2 Theory

This section presents and theoretically justifies our policy proposal. We structure the material by considering in turn the game form, selfish preferences, social preferences, our policy proposal, forward induction, overall conclusions, and testable hypotheses.

2.1 The game form

Imagine two neighboring farmers, each of whom owns a house with some adjacent land. The border between the houses is not well-defined, but each farmer can lay claim to some section of land extending from his house toward that of his neighbor. The benefit from land is that it can be used for agricultural production and hence yield income. If a farmer lays claim to land to which his neighbor does not lay claim, then

the farmer gets that land at 'full value,' proportional to its size. If both farmers lay claim to some section of land, then there is loss of value due to 'conflict'; the farmers then split only half of the value that the land would have if uncontested, so each farmer gets a quarter of full value.

This situation can be formally described using a game form with features as follows:

- There are two farmers/players, called 1 and 2.

- Each farmer's strategy set equals $\{0, 1, \ldots, T\}$, where T is the total amount of land located between the farmers' houses; a player's strategy indicates how much land adjacent to his house to which he lays claim.

- If a farmer chooses x while his neighbor chooses y, then the farmer gets land value $v(x - z) + \frac{vz}{4}$, where v is the value of uncontested land per unit and z is the number of units of contested land: $z = \max\{x + y - T, 0\}$.

2.2 Selfish preferences

If a farmer cares only about land value, he has a dominant strategy to lay a claim of T. The outcome when both farmers choose accordingly is inefficient; each gets a payoff of $v\frac{T}{4}$ whereas, had each chosen $\frac{T}{2}$, then each would have gotten a payoff of $v\frac{T}{2}$.

In light of the inefficiency, there may be scope for government intervention to ensure property rights and border protection. For example, if enforcing an equal split of land costs C and this is charged equally to the farmers, then each gets a payoff of $\frac{vT-C}{2}$ which is worthwhile if $\frac{vT-C}{2} > v\frac{T}{4}$, or equivalently $C < v\frac{T}{2}$. For example, consider (in anticipation of the upcoming experiment) the case with $T = 4, v = 8$, and $C = 10$. Before considering government intervention, we get the game in Figure 1:

	0	1	2	3	4
0	$0, 0$	$0, 8$	$0, 16$	$0, 24$	$0, 32$
1	$8, 0$	$8, 8$	$8, 16$	$8, 24$	$2, 26$
2	$16, 0$	$16, 8$	$16, 16$	$10, 18$	$4, 20$
3	$24, 0$	$24, 8$	$18, 10$	$12, 12$	$6, 14$
4	$32, 0$	$26, 2$	$20, 4$	$14, 6$	$8, 8$

Figure 1. Monetary payoffs

Strategy 4 is dominant; when both players choose accordingly, they each get a payoff of 8. The outcome is inefficient, because each player would get more than 8 if each player chose 2 or 3. Moreover, both farmers would be better off if an equal split (strategy profile (2,2)) were enforced and the cost C=10 split equally between the farmers, as each would get a payoff of $16 - \frac{10}{2} = 11 > 8$.

2.3 Social preferences

The outcome with government intervention is inefficient in the sense that resources $C = 10$ get wasted. Could there be hope for a better outcome? One reason why this may be feasible arises if the farmers do not just care for land value. This is compelling in light of the recently burgeoning literature on social preference, which argues (with reference to introspection as well as societal and experimental data) that humans often harbor objectives other than own material gain. In response, theorists have developed a variety of models of social preferences.[5] See Fehr & Gächter (2000), Sobel (2005), or Fehr & Schmidt (2006) for reviews and insightful commentary as to why economists should take social preferences seriously.

Different models modify the farmers' utilities in different ways. One may think that it matters greatly to economic analysis which model is considered. While this may be true as regards general games, it is not true as regards the following insights concerning our game form with the farmers: Most models admit as an equilibrium the cooperative outcome where each farmer lays a restrained claim of $\frac{T}{2}$. If the farmers could coordinate on such a 'nice' equilibrium, there would be no need for government intervention to improve the outcome. This rosy outcome is not guaranteed, however; most of the models also admit the high-conflict strategy profile where each farmer lays a claim of T as an equilibrium. Moreover, the equilibria are typically Pareto-ranked, so that equilibrium $(\frac{T}{2}, \frac{T}{2})$ is preferred by each farmer to equilibrium (T, T). The farmers thus face a coordination problem.

In order to make these observation concrete and precise (and then move on to our policy proposal) we now focus on a specific model, namely the Fehr & Schmidt (1999) (F&S) model of inequity aversion. As we explain toward the end of section 2, and

[5] Examples include models of inequity aversion (Fehr & Schmidt 1999, Bolton & Ockenfels 2000), concern for the least well-off individual (Charness & Rabin 2002), reciprocity (Rabin 1993, Dufwenberg & Kirchsteiger 2004, Falk & Fischbacher 2006), or guilt aversion (e.g. Battigalli & Dufwenberg 2009).

show formally in Appendix A, insights similar to the ones we highlight obtain also under other models.[6]

Applied to a two-player game, the F&S model says that if player i gets a dollar payoff of $\$_i$ while co-player j gets $\$_j$ then i's utility equals

$$\$_i - \alpha_i \max\{\$_j - \$_i, 0\} - \beta_i \max\{\$_i - \$_j, 0\}$$

where $0 \leq \beta_i \leq \alpha_i$ and $\beta_i < 1$.

Consider again the case of the farmers' game form with $T = 4$ and $v = 8$. With $\alpha_1 = \alpha_2 = \beta_1 = \beta_2 = 0$ we get the game in Figure 1 as a special case. However, multiple Pareto-ranked equilibria arise if α_i and β_i are large enough. For example, if $\alpha_1 = \alpha_2 = \beta_1 = \beta_2 = \frac{5}{8}$ we get the game in Figure 2 where the equilibria include strategy profiles (2, 2), (3,3), and (4, 4):

	0	1	2	3	4
0	$0, 0$	$-5, 3$	$-10, 6$	$-15, 9$	$-20, 12$
1	$3, -5$	$8, 8$	$3, 11$	$-2, 14$	$-13, 11$
2	$6, -10$	$11, 3$	$16, 16$	$5, 13$	$-6, 10$
3	$9, -15$	$14, -2$	$13, 5$	$12, 12$	$1, 9$
4	$12, -20$	$11, -13$	$10, -6$	$9, 1$	$8, 8$

Figure 2. Social preferences (inequality aversion a la F&S)

Things have improved, but only so much. Whereas the no-conflict outcome of strategy profile $(2, 2)$ is now sustainable in equilibrium, the high conflict outcome of strategy profile $(4, 4)$ cannot be ruled out because that is an equilibrium too.

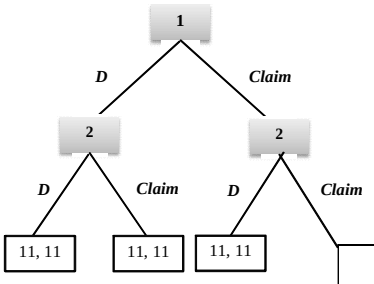
2.4 The no-intervention-agreement proposal

We are now ready to present our policy proposal aimed at ensuring the no-conflict outcome (according to the theory). Augment the above game form with a new option D: each farmer may call on a *'Divider'* who at cost C (paid for equally by the farmers) enforces the $(\frac{T}{2}, \frac{T}{2})$outcome. The Divider represents a government (which sends out a

[6]Even so, equity has indeed been a major policy concern when it comes to land redistribution in Ethiopia, which makes inequity aversion an unusually relevant example.

team of policemen, judges, and behavioral contract-theorists). Then add the following twist: If neither farmer chooses D – the interpretation being that they have 'agreed' to forgo Divider intervention – then they play the same game form as described earlier.

Once preferences are specified, this change of rules generates a '*Divider game*'. With Fehr-Schmidt preferences as before, $T = 4, v = 8$, and $C = 10$, we get the game in Figure 3:



	0	**1**	**2**	**3**	**4**
0	0, 0	-5, 3	-10, 6	-15, 9	-20, 12
1	3, -5	8, 8	3, 11	-2, 14	-13, 11
2	6, -10	11, 3	16, 16	5, 13	-6, 10
3	9, -15	14, -2	13, 5	12, 12	1, 9
4	12, -20	11, -13	10, -6	9, 1	8, 8

Figure 3. Divider Game

2.5 Forward induction

What behavior should be expected in the game of Figure 3? Before proceeding formally, consider the following intuitive chain of arguments:

(i) No rational player rejects D with the intention of following up with 0 or 1; choices 0 or 1 give a player at most 8 in the subgame (following Reject D) so it would have been better to choose D to start with to get $11 > 8$.

(ii) In the subgame, each player should figure out (i) and thus expect the co-player to not choose 0 or 1.

(iii) But each player also should figure out (ii), and thus not choose 4, which would be better than D only if the co-player chooses 0 (which (ii) ruled out).

(iv) But then it does not make sense to choose D because each player should figure out (iii) and so realize that, by rejecting D and then choosing 3, he could get at least 12, because by (ii) and (iii) the co-player will not choose 0, 1, or 4; note that 12 is more than the 11 he would get from D.

(v) The prediction, then, is that players will choose 2 or 3.

Game theorists call the chain (i)-(v) a forward induction argument; past choices tell stories about predicted future choices which in turn may affect initial choices. There is no universally accepted definition of forward induction and different scholars have proposed a variety of solution concepts to capture its spirit.[7] We do not need to enter here a discussion of which concept is best because they all deliver essentially the same prediction for the game in Figure 3. We opt for the simplest solution concept which can capture the chain (i)-(v). Arguably (and following Ben-Porath & Dekel 1992) this is iterated elimination of weakly dominated strategies (IEWDS) applied to the (reduced) normal form of the game in Figure 3, presented in Figure 4:

	D	0	1	2	3	4
D	$11, 11$	$11, 11$	$11, 11$	$11, 11$	$11, 11$	$11, 11$
0	$11, 11$	$0, 0$	$-5, 3$	$-10, 6$	$-15, 9$	$-20, 12$
1	$11, 11$	$3, -5$	$8, 8$	$3, 11$	$-2, 14$	$-13, 11$
2	$11, 11$	$6, -10$	$11, 3$	$16, 16$	$5, 13$	$-6, 10$
3	$11, 11$	$9, -15$	$14, -2$	$13, 5$	$12, 12$	$1, 9$
4	$11, 11$	$12, -20$	$11, -13$	$10, -6$	$9, 1$	$8, 8$

Figure 4. Normal form Divider game

The reader may verify that IEWDS eliminates, in turn, first strategies 0 and 1, then strategy 4, then strategy D, so that finally strategies 2 and 3 survive. If we focus on equilibria involving strategies that survive IEWDS (as do Kohlberg & Mertens 1986, cf. van Damme 1992) one sees that there are two: (2, 2) and (3,3).[8] Note also that, if we go back to the '*No-Divider Game*' (Figure 2) and apply IEWDS, then strategy 4 cannot be ruled out. Strategies 2, 3, and 4 all survive IEWDS.

[7] See e.g. Kohlberg & Mertens (1986), van Damme (1989), Ben-Porath & Dekel (1992), Battigalli & Siniscalchi (2002), Asheim & Dufwenberg (2003).

[8] There is also an equilibrium in mixed strategies where each player chooses 2 with probability $\frac{10}{13}$ and 3 with probability $\frac{3}{13}$, in which each player has an expected payoff of $\frac{166}{13}$.

2.6 Overall conclusions

Our example highlights several insights. First, the old inefficient outcome (4, 4) is no longer viable; we rule out the full-conflict outcome. Second, we also rule out the (D, D) outcome with costly mediated intervention. Thus the cost $C = 10$ is never incurred. Third, each of the predicted equilibria (2, 2) and (3,3) involves an outcome which is better than the outcome with mediated intervention (since players get at least 12 each, rather than 11). Fourth, while the mediated intervention is not used, the fact that it could have been used shaped the analysis. If the D choice were not available we would be back to the game in Figure 2, with its live possibility of a high-conflict (4,4) equilibrium.

How general are these insights? First of all, the arguments require that α_i and β_i are large enough. For example, if the players cared for land value only ($\alpha_1 = \alpha_2 = \beta_1 = \beta_2 = 0$), the forward induction argument could never kick in. To see this, augment the game in Figure 1 with the D option; mutatis mutandis we get the game in Figure 5 in which D is the sole survivor of IEWDS:

	D	0	1	2	3	4
D	$11, 11$	$11, 11$	$11, 11$	$11, 11$	$11, 11$	$11, 11$
0	$11, 11$	$0, 0$	$0, 8$	$0, 16$	$0, 24$	$0, 32$
1	$11, 11$	$8, 0$	$8, 8$	$8, 16$	$8, 24$	$2, 26$
2	$11, 11$	$16, 0$	$16, 8$	$16, 16$	$10, 18$	$4, 20$
3	$11, 11$	$24, 0$	$24, 8$	$18, 10$	$12, 12$	$6, 14$
4	$11, 11$	$32, 0$	$26, 2$	$20, 4$	$14, 6$	$8, 8$

Figure 5. Divider game with selfish players

On the other hand, the insights are robust in the sense that an analysis akin to that we conducted for the game in Figure 4 could have be done with many other combinations of the α_i and parameters (including any combination with $\alpha_i > \frac{5}{8}$ and $\frac{5}{8} < \beta_i < 1$.[9] Moreover, as shown in Appendix A, the results are not limited to the F&S

[9]We do not suggest that $\frac{5}{8}$ is a lower bound. Also, if $\beta_i > \frac{21}{32}$, strategy 4 gets eliminated under IEWDS alongside strategies 0 and 1. Finally, the results do not rely on $C = 10$ specifically; with $\alpha_1 = \alpha_2 = \beta_1 = \beta_2 = \frac{5}{8}$ any C such that 0<C<16 would do (and if $0 < C < 8$ IEWDS would even imply the best outcome: strategy profile (2,2)).

model, as similar conclusions could be drawn using the models of Bolton & Ockenfels or Charness & Rabin.[10]

3 Experiment in the Ethiopian Highlands

What is the empirical relevance of the ideas developed in the previous section? To shed light on this issue, we ran a framed field experiment in a setting which befits our story, and where there would be large potential gains if the proposal worked well. We first describe the site and the design, and then the results.

3.1 Study site, design details and pocedure

The experiment was conducted in eight kebeles (villages) in the East Gojam and South Wollo zones of the Amhara Region in Ethiopia. Four of the villages had a reported high prevalence of land conflicts and the other four had relatively lower prevalence of land conflicts.[11] The region is located in the Ethiopian Highlands, where most people are engaged in small-scale subsistence farming. After the demise in 1974 of one of the longest existing feudal systems in the world, land in Ethiopia was nationalized. The region has since undergone frequent redistributions aimed at bringing more equitable allocation of lands of different quality. The process of redistribution was characterized by a lack of accurate measurement and demarcation. These factors created a situation where most people possess highly fragmented land, sharing poorly defined borders with numerous people, a fertile ground for land disputes (Wan & Cheng, 2001). A steady population growth, coupled with land laws prohibiting sale and exchange of

[10] Forward induction arguments are conceivable also within psychological game-based models (e.g. reciprocity or guilt aversion); compare Battigalli & Dufwenberg (2009, Sections 2 & 5). However, since a proper analysis of psychological games raise many technical and other issues, we shall not explicitly go in that direction but rather be content with the robustness expressed in the text.

[11] Ethiopia consists of 11 regional states, which are divided into sub-regions called zones; the zones are divided into districts (woreda). The districts are divided into sub-districts (kebele), which are in turn constructed of local communities, called got. To simplify for the reader, we call the kebeles villages, which is the closest equivalent. Our sample villages are selected from an existing panel survey that covers 14 randomly selected villages in the region. After ranking the 14 villages based on farmer-to-farmer land conflict prevalence data from the survey, we selected the top four (which we call 'high-conflict' villages) and the bottom four (which we call 'low-conflict' villages) for our experiment. The classification is therefore relative.

land, thereby discouraging migration, exacerbate the problem.[12] The contested land in such an environment is typically not the whole land holding but rather marginal land along a vaguely defined border, similar to the theoretical model we developed in section 2. However, it is conceivable that the negative effects of the conflict could extend beyond the border line per se, for example by imposing transaction costs, and eroding tenure security. Border conflicts among neighbors could also have adverse effects on social values like trust and reciprocity important for other domains of life. Thus, clear definition of borders has considerable efficiency benefits in such an environment.

Our experimental design builds on the theoretical model and the parameterization as described in the previous section. In the experiment, we used the area unit of tilms, which is a local land size unit in the region. One hectare corresponds approximately to 30-40 tilms depending on the land type and local tradition. The average land ownership in the region where we conducted our experiments is approximately 1.27 hectare per household (CSA, 2009). We set the contested land to be 4 tilms, which corresponds to approximately 5% of the total household farm size. These parameters are chosen to reflect local conditions.

We relied on a between-sample design. Subjects were randomly and anonymously matched in pairs. We had two treatments: one without the Divider option as in Fig 2 (called no-Divider treatment hereafter), and another with the Divider option as in Fig 3 (called Divider treatment hereafter). In the no-Divider treatment, subjects could claim any integer number of tilms in the range from 0 to 4. In the Divider treatment, the subjects could choose to call for a Divider, resulting in a definite income, or claim any number of tilms in the range 0 to 4. The players decide simultaneously whether to choose the Divider or claim tilms. In line with the description in Section 2, the Divider rules even if it is only chosen by one of the farmers.

The experiment was conducted in Amharic, the local language spoken in the region. Because a large fraction of the subjects were illiterate, the experiment was orally described. To visualize our examples, we used posters (as in, e.g., Henrich et al., 2001). First the experiment was explained in general terms. Then, by using posters, the outcomes and payoffs of all possible scenarios were illustrated. On the main poster we

[12]Farmers have holding rights, which means they can 'own' the land as long they are cultivating it and can bequeath it to their children, who will continue to hold the land if they cultivate it. Such laws limit market-based consolidation of land and decrease the probability of migration: farmers who choose to leave their villages get no value from their land as they lose their holding rights.

had drawn four boxes in the middle of two houses describing the four tilms that were contested. We filled the boxes with colored slides to represent the claims by the households. We used different colors for the two households. When there was an overlapping claim over a box, i.e., a tilm, it was filled by both colors; resulting in a third color indicating that it is land under conflict. Besides the animated main poster, we had static posters of each outcome to show the monetary pay-off, with real bank notes stapled on to show how much money each farmer would earn in a specific combination of claims by both farmers. The instructions were read repeatedly and all combinations of outcomes were discussed. To make sure that everyone understood the game, subjects were also given the opportunity to ask questions in private. Then, everyone was provided with a decision sheet carefully designed in a manner similar to the posters, limiting the relevance of the ability to read and write for making decisions. Players were then instructed to put a sign that indicates their choice. In the no-Divider treatment, players could claim 0, 1, 2, 3 or 4 tilms. In the Divider treatment, players could either call for the Divider or claim 0, 1, 2, 3 or 4 tilms.

The power of our policy proposal relies on players harboring both social preferences and beliefs, and on those beliefs having certain properties. The importance of beliefs follows from the forward induction argument, as reflected in the comments in Section 2 regarding what players are expected to figure out. It is conceivable that the argument fails not because subjects lack social preferences, but because they do not hold the necessary beliefs. We therefore also collected some data on the subjects' beliefs. After the completion of the decision stage, each player was provided with another form intended to capture his/her belief about the co-player's decision. This form was similar to the decision sheet. Note that no player knew about this stage of the experiment beforehand and the procedure was explained after all decisions were completed. To incentivize belief elicitation, players were told they would earn an additional 5 Birr[13] if they guessed their co-player's decision correctly.

In each of the 8 villages, 60 households were selected randomly for the two treatments of the experiment from a provided village list. That is, we had 15 anonymous pairs for each of the two treatments in each village. We had 16 experiment sessions in total, two for each village, with a total of 240 subjects for each treatment, respectively. Two subjects (one from each treatment) decided to quit the experiment in the middle

[13]Birr is the local currency in Ethiopia. 1 USD was about 13 Birr during the time of the experiment.

and one subject in the Divider treatment declined to make a decision. Thus, our data consists of 239 observations for the no-Divider treatment and 238 observations for the Divider treatment.[14]

In order to avoid contagious effects in our experiment by word-of-mouth communication between subjects of the two different sessions in a village, we had to make sure that they did not meet. On the other hand, we wanted to use the same experimenter in all sessions, which means that we could not run the two treatments simultaneously. We therefore had to hold two sequential sessions in a way that subjects who had participated in the first session did not meet subjects for the second session. Before the first session finished, we gathered all the subjects for the second session in an adjacent room and served refreshments until the subjects of the first session had left the compound.

3.2 Results

The data from the treatments are summarized in Tables 1 and 2. The second columns in each table present the distribution of choices in each treatment. The remaining columns of the tables show how own choice is related to belief about the choice made by the co-player. For example, in Table 1 where there is not a Divider option, among those 103 who choose 2 tilms, 67 thought that their partner would do so as well, while 18 thought that their partner chose 3 tilms and 18 thought that their partner chose 4 tilms.

Table 1. Choices and Beliefs in the No-Divider treatment (n=239)

		Belief of co-player's choice				
	Own choice	0	1	2	3	4
0	0	0	0	0	0	0
1	2	0	0	1	1	0
2	103	0	0	67	18	18
3	54	0	2	17	22	13
4	80	0	0	33	19	28

[14]Note that the decisions of the anonymous co-players of those who dropped out or declined to decide are valid. Payoffs of for the pair-less subjects were calculated by taking their beliefs as their co-player's decision.

Table 2. Choices and Beliefs in the Divider treatment (n=238)

		Belief of co-player's choice					
	Own choice	*D*	0	1	2	3	4
D	73	27	0	0	20	14	12
0	0	0	0	0	0	0	0
1	0	0	0	0	0	0	0
2	92	7	0	0	69	11	5
3	28	3	0	1	18	6	1
4	44	4	0	0	21	12	7

Our theory in section 2 suggests that social preferences combined with the logic of forward induction may lead to more cooperative outcomes in the Divider treatment than in the no-Divider treatment. That is, individuals could avoid conflicts if they are offered a costly outside option which they can voluntarily forgo. To test this prediction using our experimental data, we performed a series of comparisons in the proportion of choices and beliefs within and across the two treatments following the insights outlined in section 2. To start with, rational players should not claim 0 and 1 tilm in either treatment, as these choices are strictly dominated. Our results confirm this is indeed the case: in the no-Divider treatment, no subject chose 0 and only two subjects choose 1; in the Divider treatment, no subjects chose 0 or 1. Also, only three subjects believed their co-player would go for such payoff-dominated choices. This is a clear indication that our subjects have understood the experiment well.

The next prediction that comes out of our theory is that the incidence of conflict decreases in the presence of the Divider option. That is, fewer players are expected to choose 4 tilms in the Divider treatment than in the no-Divider treatment. The basic idea is that, if the outside policy option triggers forward induction in the Divider treatment, players should move away from claiming 4 tilms, as this is a best response only for a choice of 0 and 1 tilm by the other player, given the Divider option. This is also what we find - the proportion of players claiming 4 tilms is 15 percentage points lower in the Divider compared to no-Divider treatment. A two-sample proportion test shows that this difference is statistically significant (*p-value<0.001* for both *one-sided* and *two-sided tests*). This result, however, is not enough to prove that the forward induction is working, as some of those who move away from claiming 4 tilms may choose the Divider itself. Indeed, 30.7% the players in the Divider treatment choose the Divider.

Contrary to the prediction of the forward induction argument, a null hypothesis that this is not significantly greater than zero is rejected at 1% level significance.

The above results show that close to one-third of the players 'get stuck' in the middle of the forward induction argument and fail to forgo the outside option, i.e. they choose the Divider. But it is important to notice that the majority of players do not choose the Divider. Hence, we can still test whether our policy proposal has an effect on behavior by comparing the choices among the players in the no-Divider treatment and the players who carried through with the forward induction and opted out of the Divider option in the Divider treatment.

Specifically, we compare the proportion of claims of 2 tilms and 3 tilms between the no-Divider treatment and the Divider treatment conditional on opting out of the Divider. The proportion of 3 tilm claims decreases as we go from the no-Divider to the Divider treatment even though the difference is not statistically significant (*two-sided p-value=0.2221*). As for 2 tilms, we find a difference between the treatments: a significantly higher proportion of players who reject an available Divider option choose the equal split than those who choose the equal split when no Divider option is available (a difference of 12.7 percentage points, *two-sided p-value=0.010*). This shows that our policy proposal indeed has an effect on choices in the direction suggested by our theory. One explanation for the non-significant difference in choices of 3 could be that those who skipped the Divider went for a 'better' equilibrium of (2,2) than (3,3). To some extent, this can be seen as a situation where forward-inducting players who opted out of the Divider option face a coordination problem and seem to then attempt to coordinate on the higher payoff equilibrium.

A closer look at the beliefs of players in contrast with their choices could shed further light on the choices made. For example, it follows from the theory that players who believe their co-player would claim 4 tilms would also claim 4 tilms if they are in the no-Divider treatment and would choose the Divider option in the Divider treatment.

We find evidence along these lines in our experiment. In the no-Divider treatment, almost half of the players (47.5%) who believe that that their co-player will claim 4 tilms also claim 4 tilms themselves. This proportion is much higher compared to the beliefs for those who claim 2 tilms (a difference of 16.9 percentage points, two-sided p-value=0.005 and one-sided p-value=0.0295) and 3 tilms (a difference of 25.4 percentage

points, both *two-sided* and *one-sided p-values<0.01*).

In the Divider treatment, most of the players who believe their co-player would claim 4 tilms choose the Divider, as expected; all other claims were significantly less likely. This result indicates that a portion of those who did not complete the forward induction process did not believe that the presence of the Divider was enough to entice their co-players toward cooperation. The rational choice was then for them to impose the Divider themselves. This does not necessarily imply that they did not have friendly intentions themselves.

Another prediction arising from social preferences is that players who expect the other player to go for an equal split should also opt for an equal split in either treatment. We find support for this prediction in the data. In each treatment, more than half the players with belief that the other player would claim 2 tilms also claimed 2 tilms and the percentage differences against each of the other options are statistically significant with (both *one- and two-sided*), with p-levels less than *0.0001*.

The belief data also give some insight into how this policy innovation could reduce conflict. By comparing beliefs and choices across treatments, we can better understand what 'type' of players is more likely to be affected by the intervention. We can differentiate between two broad types of players who end up choosing conflict. First, we have those who go for conflict and also believe the co-player will go for conflict. If a player believes that the co-player will claim 4, then the rational response is to claim 4 – with or without social preferences (see figures 1 and 2) in the non-Divider treatment and despite the fact that the player himself might prefer cooperation. The second type of player goes for conflict even though he believes the co-player will go for an equal split. This is consistent with an absence of social preferences (see the differences in pay-offs between figures 1 and 2). It can be said that the latter have limited 'friendly tendencies' compared to the former. Our expectation is that the first type, which responds to the threat of conflict but does not seek conflict, will be given the opportunity to cooperate in the Divider treatment, while the conflict prone might still attempt to claim 4.

Looking at the last columns of table 1 and table 2 sheds some light on this: the proportion of players who claim 4 and also believe the co-player will go for 4 is significantly lower in the Divider treatment. When there is a Divider, the majority of those who claim 4 are those who believe the co-player will go for 2. That is, the presence

of the Divider affects the behavior of players of the first type, those who respond to conflict but do not seek it. In other words, the decline in conflict arises because the presence of the Divider helps those with friendly attitudes to cooperate.

We also analyzed the data for the high and low conflict villages separately (see Appendix B). In the no-Divider treatment, we find significantly that a higher proportion of subjects claim 4 tilms in high-conflict villages compared to low-conflict villages. Moreover, claims of 2 tilms are significantly lower in the high-conflict villages compared to the low-conflict villages when there is no Divider. These results can be seen as indicators of external validity for our experiment. When the Divider is introduced, we do not find a significant difference in behavior between high- and low-conflict villages. Thus, the positive impact of introducing a Divider was larger in villages with relatively higher prevalence of land conflicts. Our policy proposal seems to work better where it is needed the most.

4 Concluding Remarks

We consider a land-grabbing game where selfish players, who desire to get as much for themselves as possible, would be destined for costly conflict. A key initial observation is that social preferences may transform the situation into a coordination game. There is hope in this insight alone; if players coordinate on a 'good' equilibrium, they avoid the conflict. The second key idea is to boost the prospect of this outcome further, drawing on the logic of forward induction. We propose a policy which modifies the game so that players can elect to enforce a cooperative outcome at a cost. The game theoretic prediction is that they would not elect this option and instead coordinate on a good outcome more surely than had the Divider-option never been available.

The costs of land conflict in developing countries are huge, so the potential gains of this policy could be vast. Holden et al. (2011), drawing on a sample of 400 mediators who had mediated 18,620 conflicts in the Highlands of Ethiopia, find that more than half of the conflicts were land-related and almost 20% of them were border conflicts. Almost half of the 1530 conflicts that were referred to courts were border conflicts. Such experiences have therefore precipitated millions of hectares of agricultural land to fall under various kinds of reforms in Africa and elsewhere. The costs of these interventions, and of the potential related conflicts, are high and difficult to carry both

for farmers, for governments, and for international aid agencies. Policies such as the one proposed here are therefore particularly relevant in such settings, where the first steps are being taken to formalize individual user rights to what has previously been either government owned or communally managed land.

Our theoretical results indicate a way to benefit from a design where interventions are made available on a voluntary basis, as opposed to the mandatory programs that are now the norm. Current mandatory certification schemes could fairly easily be adjusted to accommodate such cooperative solutions. The proposal also illustrates how, in principle, policy intervention does not have to be actively managed. One may think of it as allowing for, or promoting, voluntary participation in an outcome with friendly relations. Neighbors facing potentially costly conflict are aided not through hands-on intervention but through counterfactual intervention which could have occurred but did not. When farmers actively express that they do not want the intervention, this coordinates them to cooperate.

To test the empirical relevance of our proposal, we ran an experiment in the Amhara Region of Ethiopia – a natural setting where people have experienced land conflict. We find strong support for the first idea (social preferences generate a coordination game). Players who believe others cooperate often cooperate themselves. We find only guarded support for the second idea (forward induction). The subset of players who discard the costly-Divider option choose, and believe a co-player will choose, the most cooperative strategy to a larger degree than when the Divider option was not available in the first place. The prevalence of high-conflict outcomes is dramatically reduced, especially in areas with high levels of land conflicts, although we did not nearly obtain full coordination on the best possible outcome (in particular because more subjects than predicted by the theory chose to call for the Divider).

We did not make it easy on our subjects. They played the game once, and were offered no opportunity to gain experience. They could not communicate pre-play; if people talk, then perhaps those who understand the forward induction argument will convince others. The design allowed two choices that were consistent with the forward induction argument (2 and 3), possibly making it less transparent. The game we used to model the conflict situation has two stages (Fig. 3), but subjects interact in a perhaps less transparent version corresponding to a reduced normal form game with simultaneous moves (Fig. 4). Finally, the task was rather abstract, involving

labeled choices and payoffs on posters rather than real land. For all these reasons, our experiment represents but a start for serious empirical testing. We hope it inspires follow-up research that modifies features of our design and possibly relies on stronger field components.

In addition, it is natural to reflect on the following rather extreme aspect of our proposal: At face value, it assumes that, once the parties reject the Divider option, then no outside protection is offered whatsoever. Intuitively, that would seem to make rejecting the Divider a rather risky proposition. In practice, the policy can be expected to be coupled with alternative measures, say involving some limited police and court protection even if the Divider option is rejected by all.

We would be happy if a lasting impact of our study were to influence the thinking of development scholars and policy makers through the questions we have articulated: Is what at first glance seems to be a social dilemma really a coordination game? Could a policy involving voluntary participation promote a desired outcome at lower cost than that of heavy-handed government intervention? We have shown, for a specific context that the answer is yes in theory and maybe in practice. We hope to inspire thinking about, and inquiry in regard to, the relevance of these questions more generally. Our specific context may serve as an inspiring metaphor in this connection.

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Appendix A

In section 2, we said that the conclusion we drew using the Fehr-Schmidt model had counterparts in other models, notably those of Bolton and Ockenfels (2000) (B&O) or Charness & Rabin (2002) (C&R). We now show this formally.

Applied to a two-player game, a simple version of B&O's model says that if player i gets \$i while co-player j gets \$j then i's utility equals:

$\$_i - \gamma_i|\$_i - \frac{\$_i+\$_j}{2}|$, where $0 \leq \gamma_i < 1$.

Now note that $\$_i - \gamma_i|\$_i - \frac{\$_i+\$_j}{2}| = \$_i - \gamma_i|\frac{\$_i-\$_j}{2}| = \$_i - \gamma_i'|(\$_i - \$_j)|$, where $\gamma_i' = \frac{\gamma_i}{2}$. Then note that $\$_i - \gamma_i'|(\$_i - \$_j)| = \$_i - \gamma_i' \max\{\$_j - \$_i, 0\} - \gamma_i' \max\{\$_i - \$_j, 0\}$. That is, with two players, the B&O model works just like the Fehr-Schmidt model with the constraint that $\alpha_i = \beta_i$. If we assume that $\gamma_i = \frac{3}{2}$ then $\gamma_i' = \frac{3}{4}$ and we get the same prediction as for the F&S model with $\alpha_i = \beta_i = \frac{3}{4}$, which is the case covered in Section 2.

Next consider the C&R model which, when applied to a two-player game, says that if player i gets $\$_i$ while co-player j gets $\$_j$ then i's utility equals:

$\$_i + \varepsilon_i[\delta_i \min\{\$_i, \$_j\} + (1 - \delta_i)(\$_i + \$_j)]$, where $\varepsilon_i, \delta_i \geq 0$ and $\varepsilon_i, \delta_i < 1$.

In this case, it is harder to generate conclusions analogous to those in section 2, but not impossible. To appreciate this, consider for example the special ('semi-Rawlsian') case where $\delta_i = 1$ and $\varepsilon_i = \varepsilon_j = \varepsilon$, where $0 \leq \varepsilon < 1$ We get $\$_i + \varepsilon_i[\delta_i \min\{\$_i, \$_j\} + (1 - \delta_i)(\$_i + \$_j)] = \$_i + \varepsilon \min\{\$i, \$j\}$, which applied to the Divider game form (Figure 5) yields:

	D	0	1	2	3	4
D	11+11$\mathcal{E}$,11+11$\mathcal{E}$	11+11$\mathcal{E}$,11+11$\mathcal{E}$	11+11$\mathcal{E}$,11+11$\mathcal{E}$	11+11$\mathcal{E}$,11+11$\mathcal{E}$	11+11$\mathcal{E}$,11+11$\mathcal{E}$	11+11$\mathcal{E}$,11+11$\mathcal{E}$
0	11+11$\mathcal{E}$,11+11$\mathcal{E}$	0, 0	0, 8	0, 16	0, 24	0, 32
1	11+11$\mathcal{E}$,11+11$\mathcal{E}$	8, 0	8+8$\mathcal{E}$,8+8$\mathcal{E}$	8+8$\mathcal{E}$,16+8$\mathcal{E}$	8+8$\mathcal{E}$,24+8$\mathcal{E}$	2+2$\mathcal{E}$,26+2$\mathcal{E}$
2	11+11$\mathcal{E}$,11+11$\mathcal{E}$	16, 0	16+8$\mathcal{E}$,8+8$\mathcal{E}$	16+16$\mathcal{E}$,16+16$\mathcal{E}$	10+10$\mathcal{E}$,18+10$\mathcal{E}$	4+4$\mathcal{E}$,20+4$\mathcal{E}$
3	11+11$\mathcal{E}$,11+11$\mathcal{E}$	24, 0	24+8$\mathcal{E}$,8+8$\mathcal{E}$	18+10$\mathcal{E}$,10+10$\mathcal{E}$	12+12$\mathcal{E}$,12+12$\mathcal{E}$	6+6$\mathcal{E}$,14+6$\mathcal{E}$
4	11+11$\mathcal{E}$,11+11$\mathcal{E}$	32, 0	26+2$\mathcal{E}$,2+2$\mathcal{E}$	20+4$\mathcal{E}$,4+4$\mathcal{E}$	14+6$\mathcal{E}$,6+6$\mathcal{E}$	8+8$\mathcal{E}$,8+8$\mathcal{E}$

Figure 6. Divider game with semi-Rawlsian C&R preferences

Now apply IEWDS. In round one, for any $0 \leq \varepsilon < 1$ we can delete 0 and 1. In round two (unlike the case with F&S preferences) we cannot delete 4 on the grounds that this

strategy is dominated by D (4 does better than D against 2 for any $0 \leq \varepsilon < 1$ However, on some reflection, one sees that, if ε is high enough, then 4 is dominated by a mixed strategy which puts appropriate weights on a combination of D and 3.[15] Hence we can eliminate 4. And then, in round 3, because ε is high enough, we can eliminate D (which is dominated by 3). So, in this case, much as in Section 2, strategies 2 and 3 are the game's sole survivors of IEWDS. Note finally that, if we consider the No-Divider modification of the game in Figure 6 (i.e., the same game except that the D choices are removed), and apply IEWDS, then strategy 4 cannot be ruled out. Strategies 2, 3, and 4 all survive IEWDS.

[15]To see this, consider the limiting case where $\varepsilon = 1$, which generates numbers easy to work with; after drawing the desired conclusion, we verify that it must hold also for slightly lower values of ε. Consider player 1 and his mixed strategy which assigns probability p to D and $(1-p)$ to 3, where $0 < p < 1$. A sufficient condition for this mixture to weakly dominate 4 (in the reduced game where 0 and 1 are already eliminated) is that it yields strictly higher utility against each of 2's strategies 2, 3, and 4:

$p22 + (1-p)28 > 24$ [mixture better than 4 if player 2 chooses 2]
$p22 + (1-p)24 > 20$ [mixture better than 4 if player 2 chooses 3]
$p22 + (1-p)12 > 16$ [mixture better than 4 if player 2 chooses 4]

All three inequalities hold if $2/5 < p < 2/3$. Hence any mixed strategy which assigns probability p to D and $(1-p)$ to 3, where $2/5 < p < 2/3$, can be used to eliminate 4 in the game where $\varepsilon = 1$. Given such a strategy, because the above inequalities are all strict and because payoffs change continuously with ε, it can be used also to eliminate 4 in the game where $\varepsilon < 1$, if ε is close enough to 1.

Appendix B

High vs low conflict villages (% of choices)

	High conflict areas			Low conflict areas			Pooled		
Choice	NDiv	Div	Div-*D*	NDiv	Div	Div-*D*	NDiv	Div	Div-*D*
D	na	30	na	na	31.4	na	na	30.7	na
1	0	0	0	0.8	0	0	0.8	0	0
2	33.3	40.0	57.1	52.9	37.3	54.3	43.1	38.7	55.8
3	26.7	10.8	15.5	18.5	13.6	19.8	22.6	12.2	17.6
4	39.2	19.2	27.4	27.7	17.8	25.9	33.5	18.5	26.7

NDiv = No Divider game

Div = Divider game

Div-*D* = Divider game exluding *D* choices

Paper II

Experimentation and Social Learning in Small-Scale Agriculture: A Tale of Two Dilemmas*

Haileselassie Medhin

University of Gothenburg

Abstract

In situations where critical information about new technologies comes from costly experimentation, social learning possibilities create incentives for free-riding. I explore this problem in the context of technology adoption in small-scale agriculture. First, I show that a multistage volunteer's dilemma game arises if experimentation is not divisible and hence should be carried out by a single agent. If experimentation is divisible, the problem becomes a multistage threshold public good game. I then undertake lab experiments to evaluate the net effect of social learning in each game and compare the outcomes. I find that losses from delay in experimentation outweigh efficiency gains from social learning when experimentation is not divisible. On the other hand, efficiency gains from social learning outweigh delay costs when it is possible to share the burden of experimentation. The potential role of social preferences is discussed at the end.

Keywords: *social learning, experimentation, volunteer's dilemma, threshold public goods, technology adoption, free-riding, social preferences, farmers*

JEL Classification: D83, 013, Q12, Q16

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1 Introduction

Millions of farmers in many developing countries practice agriculture under technologies that are sub-frontier, and stay poor because if it. At the same time, it is an often mentioned puzzle that the uptake of many productivity-enhancing technologies is low among small-scale farmers. This has triggered considerable interest in economics over the past few decades.[1] An important factor in the successful adoption of new technologies is learning about their appropriate management vis-à-vis the characteristics of the production problem at hand, a process that often involves costly experimentation by farmers themselves (e.g., Foster & Rosenzweig, 1996; Foster & Rosenzweig, 2010; Everson & Westphal, 1995; Besley & Case, 1997; Bandiera and Rasul, 2006). Such a learning process also has a social dimension.[2] There is now ample evidence that shows how farmers in developing countries systematically extract and utilize information about the application and profitability of new technologies from other farmers around them (e.g., Foster & Rosenzweig, 1995; Munshi, 2004; Conley & Udry, 2010; Bindlish & Eversson, 1997; Van den Broeck & Dercon, 2011).

Social learning gives farmers the possibility of obtaining locally tested practical knowledge on how a new technology can be best fitted to their specific agro-ecological and economic circumstances. This could be particularly useful in situations where critical information about the appropriate use and management of new technologies should come from localized, in-the-field experimentation by farmers themselves. Social learning can therefore aid the speedy and efficient diffusion of new ideas and technologies because not every farmer has to go through a costly process of experimentation. Farmers in such an environment are not just passive users of socially available information from their *information neighbors*[3]; they are also active contributors of hard-gained information to the social information pool. The evidence in Conley & Udry (2010) suggests that farmers are likely to be each other's information neighbors,

[1] See Foster & Rosenzweig (2010) for a recent review of the literature on the determinants of technology adoption in small-scale agriculture.

[2] Social learning is a very broad concept related to a wide range of economic problems. Early contributions include Banerjee (1992); Bikhchandani et al. (1992); Ellison & Fudenburg (1993).

[3] Farmers do not learn from all other farmers around them, but rather discriminate about who to learn from based on a number of criteria such as similarity in agro-economic characteristics and practical experience in the decision problem of concern. Conley & Udry (2010) refer to such systematically selected learning partners as 'information neighbors'. I will refer to the set of information neighbors who can learn from each other's experimentation as an *information neighborhood*.

especially during the time that a new technology is introduced to a village.[4]

This indicates that social learning might have a dark side related to the very fact that makes it attractive: it could affect experimentation. Because experimentation is often the primary source of information (e.g., see Bandiera & Rasul, 2006; Foster & Rosenzweig, 2010), *someone* should do it first for (social) learning to take place. But experimentation could also be very costly. With social learning possibilities, farmers have the incentive to limit their own experimentation in the hope of obtaining relevant ideas from others around them. Simply put, social learning in such circumstances gives the desired information public good properties where agents can free-ride on the experimentation of others. Hence, while social learning could aid the diffusion of new technologies by allowing farmers to cheaply learn from the experimentation of their neighbors, it could also create experimentation failure in the initial stages of a technology's diffusion process.[5]

Social learning can therefore hamper the adoption and diffusion of new technologies that are potentially profitable, but that require significant local experimentation. Despite this, the issue has received very little attention in the literature on agricultural technology adoption. A notable exception is an empirical study by Bandiera & Rasul (2006) on sunflower adoption in Northern Mozambique. Based on the 'target input' learning model (Bardan & Udry, 1999) where farmers undertake repeated trials to sharpen their knowledge of how to use a new technology, they test the hypothesis that the incentives to undertake trials could decrease with the number of adopting farmers in one's network, and find some supporting evidence. Their paper, however, focuses on the case where farmers limit their experimentation because they believe that sufficient experimentation *has been done* or *is being done* by others. It can therefore

[4]Localized experimentation and the careful selection of information neighbors are expected to be more important in places where farm heterogeneities are high. High heterogeneities mean that blanket recommendations (e.g., information from farm public extension services) may not be effective in diffusing new technologies. e.g., see Foster & Rosenzweig (1996), Munshi (2004), Krishnan & Patnam (2012), Babu et al. (2012),

[5]It should be noted that experimentation and learning about a new technology can go beyond the production process. For example, farmers need to know how they will store a new type of crop, how they will maintain a new type of machinery, or how they will transport and sell their surplus. At the same time, much of the information that comes from such a costly process seems reasonably simple to copy. It pays well to be a follower in such circumstances. Poor farmers may therefore have a strong incentive to look for one of their information neighbors to take the plunge. The risk is that no one does, even if it is actually worth it.

be said that their results capture *successful* free-riding, not experimentation delay or failure that might have happened if everyone had adopted the free-riding strategy.

The overall objective of this study is to investigate whether the free-riding externalities *created*[6] by social learning cause significant delay in otherwise profitable experimentation. This is done in a number of steps. First, the problem is formulated into a game-theoretic model. It is shown that the specific dilemma created by social learning depends on the characteristics of the particular experimentation, especially on whether experimentation is *indivisible* or *divisible.* Experimentation is indivisible when it should be carried out by a single farm household to provide the required (public) information. This is modeled as a multistage Volunteer's Dilemma (MVD hereafter) game.[7] If experimentation is divisible, possibilities to 'share' the experimentation burden exist. It is then shown that this changes the ensuing dilemma to a multistage threshold public good (MTPG hereafter) problem.[8]

Next, the equilibrium properties of the MVD and MTPG games are assessed from the perspective of the trade-off between the potential gain from efficient experimentation and the potential loss from delayed experimentation. The two dilemmas have interesting similarities and differences in this respect. Both have multiple equilibria, potentially result in a coordination problem (i.e., experimentation delay). But they are different in one key aspect: efficient equilibria in the MVD game are always asymmetric, while there is a symmetric efficient equilibrium in the MTPG game. This suggests that efficient coordination is easier in the MTPG game than in the MVD game. This hypothesis is tested using lab experiments that closely replicate the two games. For a meaningful comparison of the experimental results, the net welfare effect of social learning in each dilemma is first evaluated against two natural benchmarks: (i) a scenario with no social learning possibility, where everyone should incur the cost of

[6]There could be free-riding problems in social learning that are not created by social learning. For example, it may not be possible for a single farmer to profitably undertake full experimentation with a technology, but experimentation still could be collectively protifitable, creating a classic public good problem. Our interest is in dilemmas for which social learning is fully responsible. For this to happen, experimentation should be privately profitable. That is, we are concerned with experimentation that would be readily undertaken by a rational farmer if there were no social learning possibility.

[7]Volunteers' Dilemmas are a special case of what are often referred to as 'best-shot public good games' (e.g., see Hirshleifer, 1983 and Galeotti et al., 2010). The literature on VDs is too big to review here. Earlier works include Bliss & Nalebuff (1984) and Weesie (1994). For recent studies on the topic, see Kroll et al. (2007) and Cherry et al. (2013).

[8]Threshold public good problems are also widely studied in the literature. See, e.g., Van de Kragt et al. (1983) and Croson & Marks (2000).

experimentation (ii) the best case scenario with social learning, where efficient experimentation is performed without any delay. The experiments therefore evaluate the net effect of social learning in each dilemma, and examine whether the divisibility of experimentation helps reduce the experimentation delay that can result from free-riding incentives.

Finally, the two dilemmas and the related experimental results are examined in light of *social preferences*. A rapidly growing literature shows that behavior in coordination games like the MVD and MTPG can be influenced by non-material motivations such as inequality aversion (see Fehr & Schmidt (2006) and Cooper & Kagel (2013) for extensive reviews of the literature on the topic). Understanding the role of such motivations could be particularly important in the MVD game, as all Pareto-efficient equilibria are asymmetric and hence inherently unequal. This will be explored by extending the game with the inequality aversion model of Fehr and Schmidt (1999). Such a theoretical exercise may therefore shed more light on any treatment effect observed in the lab experiments.

The paper contributes to the literature in a number of ways. To begin with, it connects different strands of literature in development economics, public economics, game theory and behavioral economics to conceptualize a specific economic phenomenon: negative externalities of social learning in small scale agirculture. This could hopefully serve as a framework for future research addressing the issue even in non-agricultural contexts.[9] The ensuing theoretical insights are then tested in an experimental setting. I believe this is a good example of how real-world economic problems can be taken to the lab and evaluated against benchmarks that are also informed by reality. Along the way, the paper also makes some contributions to the literature on how player heterogeneities affect equilibrium properties of finite-time dynamic volunteer's dilemma games (e.g., Bilodeau & Slivinski, 1996; Bilodeau et al., 2004; Otsubo & Rapoport, 2008). For example, it is shown that the result in Bilodeau & Slivinski (1996) - that finite time dynamic volunteer's dilemma games have a unique subgame perfect equilibrium if players are different in benefit to cost ratio - is not always warranted if the game is played in discrete time. It is also shown how inequality aversion affects

[9]Indeed, the free-riding externalities of social learning explored in this paper should not be limited to the problem of technology adoption and diffusion in small-scale agriculture. There could be many examples where agents face the coordination problem of producing expensive, but easily replicable ideas.

the equilibrium properties of such games, and brings new insights into what *type* of people are likely to provide the public good. Perhaps surprisingly, inequality aversion makes people more likely to be free-riders than providers, leaving the burden to the more selfish.

The paper is organized as follows. Section 2 will develop the MVD and MTPG games and describe their equilibrium properties. Section 3 will present the experiment. Section 4 looks back at the games and experimental results using the lens of social preferences. Section 5 concludes.

2 The Two Dilemmas

The aim of this section is to translate the experimentation dilemmas created by social learning into analytically tractable games and discuss their equilibrium properties. To simplify the task, I will assume that the process of experimentation and learning have the following properties:

(i) Experimentation is considered to happen in a fixed and positive time interval - the *experimentation interval* - which is common knowledge to everyone in an information neighborhood. For example, the experimentation interval could be a crop production season. That is, there is a positive time lag between the start of experimentation (i.e., the decision to experiment) and the end of experimentation (i.e., learning). This implies that farmers learn from the *outcomes* of their actions or other people's actions.[10] More importantly, the time lag between actions and outcomes indicates that free-riding farmers have no opportunity to switch back to experimentation without wasting time equivalent to the experimentation interval.

(ii) Everyone in an information neighborhood is assumed to know how to undertake the required experimentation *efficiently*.

(iii) Learning from own-experimentation and others' experimentation are perfect substitutes (e.g., the cost of copying is zero and there are no barriers to the movement of information among information neighbors).

[10]See Chamley (2004) for a detailed discussion of the distinction between learning from *actions* of others *per se* (e.g., I follow my neighbor in switching to a new crop because I believe he switched for good reasons) and learning from *outcomes of actions* of others (e.g., I learn from my neighbor's experience on how to protect my crop from pests). We are mainly concerned with the latter kind of problem.

Given these assumptions, consider that there are n identical farmers in a given village who are information neighbors to each other.[11] Assume that each farmer[12] has a finite productive life span up to T. Moreover, assume that each farmer uses an old technology that provides a utility flow of v_t at a given year t. Now assume that only one of the farmers is offered a new technology at some time period $t = 0$, where he knows that he needs to undertake a costly experimentation for a given number of seasons to acquire critical information that makes the technology profitable. For simplicity, let us assume that experimentation takes exactly one period and costs C. The new technology provides utility flow s_t for each period after the experimentation period $t = 0$, which is strictly higher than v_t. There is no positive utility flow from the new technology during the experimentation period. Given these conditions, the farmer would undertake the experimentation at $t = 0$ if $\sum_{t=1}^{T} s_t - C > \sum_{t=1}^{T} v_t$. Assume that this is indeed the case; i.e., experimentation with the new technology is *privately profitable* at $t = 0$. The farmer therefore undertakes a timely experimentation and successfully adopts the new technology.

Without changing the properties of the new technology, assume now that it was not only one of the farmers who was offered the new technology at $t = 0$, but all in the information neighborhood. Each farmer must now decide to undertake the experimentation right away at $t = 0$ or forgo own experimentation in the hope of acquiring the desired information without paying the cost, and hence earn $v_0 + \sum_1^T s_t$ instead of $v_0 + \sum_1^T s_t - C$. That is, each farmer now has the incentive to delay the costly experimentation that they would have readily undertaken if they did not have the possibility of easily learning from each other. If everyone adopts this free-riding strategy, experimentation will not take place at $t = 0$, and everyone has one season less of benefits from the new technology. They face the same decision again at $t = 1$, where the payoffs from successful free-riding and experimenting are $v_0 + v_1 + \sum_2^T s_t$ and $v_0 + v_1 + \sum_2^T s_t - C$, respectively. If none of them does the experimentation again at $t = 1$, they face the same decision in period $t = 2$, and so on. The farmers in the

[11] Although the number of farmers in an information neighborhood theoretically can be large, it is more likely to be small in the real world. For example, a large information neighborhood would mean higher costs of collecting and organizing information for each member, creating incentives to break up into smaller ones. It is therefore not far from reality to expect many information neighborhoods to be formed even, in a village with homogenous agro-ecological conditions.

[12] The terms 'farmer' and 'player' will be used interchangeably throughout the paper. For simplicity of exposition, all players will be assumed to be male.

information network are therefore locked into a waiting game until at least one of them does the experimentation or the delay in experimentation becomes too long for the technology to provide a positive net return. This is what is referred to as a MVD game.[13]

Note that actions are undertaken simultaneously at the start of each experimentation interval. Players know the decisions made by their information neighbors only after observing the outcome of the action at the end of the experimentation period. That is, information neighbors play the game the first time the new technology is introduced, stop and observe each others' outcomes after the first experimentation interval, then play the game again if there was no experimentation, and so on. Another way of putting it is that players cannot affect the cost of time lapse within each action-outcome interval once they are playing the game in that interval, but they can *avoid* it entirely by coordinating experimentation in an earlier interval. Players in our MVD game therefore play many one-shot VD games, each Pareto-superior to the next one. Except for the first game, the game at each stage is also a direct result of the decisions in the previous game. Specifically, the game at each stage is an outcome of the coordination failure in the previous stage. This play-observe-play property is what sets apart our MVD game (and our experiments) from finite-time dynamic VD games discussed in the literature.

In the reduced form of the game, a given farmer i chooses an experimentation time $t_i^e \in \{0, 1, 2, ...T, T+1\}$, where $t^e = T+1$ means the farmer never experiments. Let us say the game ends at some period t. Denote farmer i's payoff from successful free-riding at t (i.e., $t_i^e > t$) as F_{it} and from experimenting (i.e.$t_i^e = t$) as E_{it}. Then we get:

$$\begin{aligned} F_{it} &= \sum_{0}^{t} v_t + \sum_{t+1}^{T} s_t \\ &\text{and} \\ E_{it} &= \sum_{0}^{t} v_t + \sum_{t+1}^{T} s_t - C = F_{it} - C. \end{aligned}$$

[13] As stated in the introduction, the MVD game here is closely related to dynamic VD games presented in, e.g., Bilodeau & Slivinski (1996), Bilodeau et al. (2004) and Otsubo & Rapoport (2008). The key difference is the 'multistage' property of the game, where simultaneous actions are observed at the end of each stage.

The payoffs in the above game have the following key properties:

(i) $F_{it} > E_{it}$ for all $t \in \{0, 1, 2, ..., T\}$.

(ii) F_{it} and E_{it} are strictly decreasing in t.

(iii) $E_{it} > F_{iT}$ from the assumption of privately profitable experimentation at $t = 0$.

(iv) Provided that experimentation did not happen before T, free-riding at T results in a guaranteed $F_{iT} = \sum_{t=0}^{T} v_t$. That is, if experimentation does not happen before T, it will never happen at T, because $E_{iT} = F_{iT} - C < F_{iT}$.

The above MVD game has multiple equilibria. Specifically, it has n pure strategy *asymmetric* subgame perfect equilibria (SPE) where one of the farmers does the experimentation at $t = 0$ and all others free-ride in all periods. That is, $t_i^e = 0$ and $t_{-i}^e = T + 1$ is an asymmetric SPE for any farmer $i \in \{1, ..., n\}$. This is quite intuitive: no one has the incentive to deviate away from experimentation if they know *all* others will free-ride until the end. Similarly, no one has the incentive to deviate from free-riding if they know that *someone* is experimenting immediately. Notice that these asymmetric equilibria are Pareto-efficient outcomes, where the full efficiency gain of social learning is acquired (there is no waste of experimentation cost and there is no delay). Hence, when experimentation is indivisible, the best case scenario with social learning is simply when there is successful and immediate free-riding by all but one of the farmers.

The game also has a unique *symmetric* subgame perfect equilibrium (SSPE) where each farmer randomizes between experimenting and free-riding in each period. This equilibrium is based on the behavioral assumption that farmers randomize their choices at each stage in a manner that maximizes expected utility in each period, provided that they reach it. The expected utility at a given period t is, of course, not easily calculable, as it also depends on the expected utility of period $t + 1$, which in turn depends on the expected utility $t + 2$, and so on. Hence, we should work our way back from the game at T and calculate the probabilities assigned for each strategy at each period.

The multiplicity of equilibria naturally creates a coordination problem that could lead to delay in experimentation, and hence delay in adoption of the profitable new technology. The key point here is that, if efficient coordination happens in the MVD game, it is bound to be asymmetric.

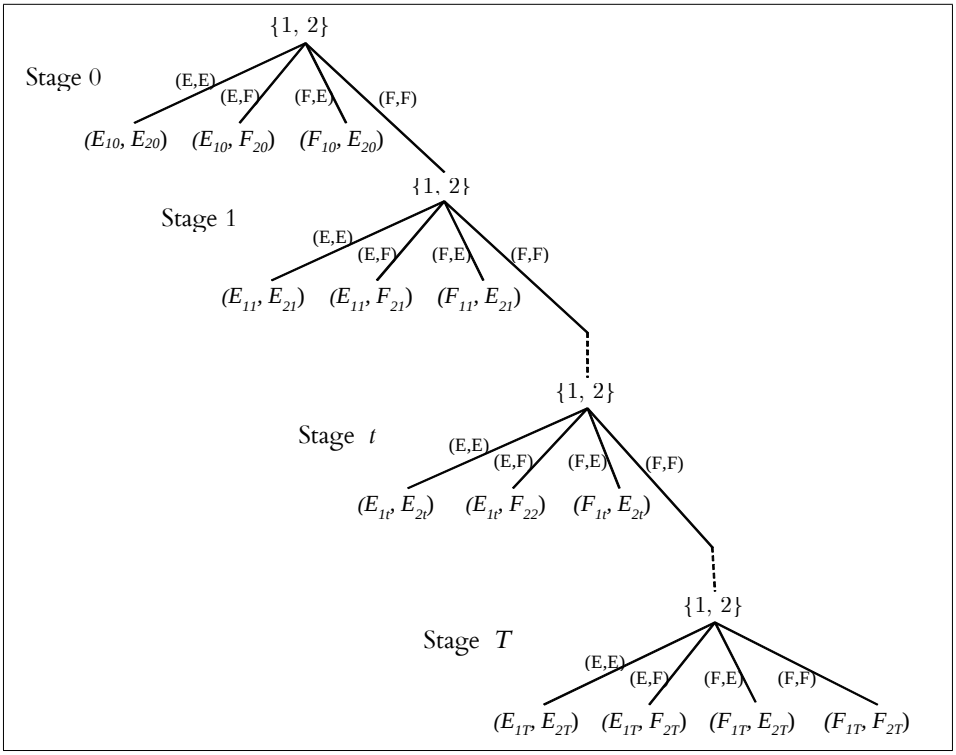

Figure1. The MVD game with two players

(each player can either experiment(E) or wait and copy (F) in each period)

The assumption so far has been that experimentation costs cannot be shared. That is, C should be paid fully by at least one of the farmers for learning to happen. Relaxing this assumption and allowing for 'cost-sharing' effectively changes the game at each period into a TPG game. It is further assumed that experimentation that does not reach the threshold does not provide meaningful information.

In such a game, each farmer i chooses to cover $c_{it} \leq C$ at period t, provided that experimentation has not been done before t. Let us start from $t = 0$ as before. If $\sum c_{i0} \geq C$, each farmer earns $v_0 + \sum_1^T s_t - c_{i0}$. If $\sum c_{i0} < C$,the farmers play the game again at $t = 1$. At $t = 1$, if $\sum c_{i1} \geq C$, each farmer earns $v_0 + v_1 + \sum_2^T s_t - c_{i0} - c_{i1}$. Otherwise the game continues to $t = 1$. More generally, if the game ends at some

period t, a given farmer i earns $\sum_0^t v_t + \sum_{t+1}^T s_t - \sum_0^t c_{it}$. That is, contributions that resulted in an insufficient past experimentation are wasted, as are contributions that resulted in over-experimentation. The game in each period is basically a threshold public good game, making the overall game a MTPG game.

One-shot TPG games have multiple equilibria. Previous studies have focused on the symmetric equilibria, which are also expected to be focal points, and hence a means of coordination (e.g., Cadsby & Maynes, 1999; Spencer et al., 2009; Norman & Rau, 2012). There are also symmetric mixed and pure strategy SPE in the MTPG game. Every strategy profile where $\sum c_{i0} = C$ is an SPE at $t = 0$. The symmetric pure strategy SPE at $t = 0$ is where each farmer covers $\frac{C}{n}$ of the experimentation cost at $t = 0$, and earns $v_0 + \sum_1^T s_t - \frac{C}{n}$. The average payoff in this equilibrium is the same as the average payoff in one of the asymmetric SPE in the MVD game, while the distribution is more equitable in the former. Hence, while both games have Pareto-efficient equilibria, where the full gains from social learning are captured without delay, the MTPG game has the advantage that it has a 'focal' point that could help solve the coordination problem. Whether this helps in abating the negative externalities from social learning is an empirical issue. That is what the lab experiment aims to answer.

3 The Experiment

3.1 Design and procedure

I conducted neutrally-framed lab experiments that replicate the MVD and MTPG games introduced in Section 2. Each information neighborhood in the experiment constituted of two players. In both treatments, anonymously paired subjects played a multistage game called '*Unlocking Boxes*', which proceeds as follows:

- Each player in a pair starts with fixed endowment tokens.

- Then the pair is offered a virtual 'locked box' that holds an *endowment multiplier*, but can only be unlocked at a fixed cost. In the MVD treatment, players make a simultaneous binary decision on whether or not to pay the unlocking cost. In the MTPG treatment, they choose how much of the cost to cover.

- If the box is unlocked, the remaining endowment of each player (i.e., after any unlocking costs) is multiplied by the endowment multiplier (which is a positive integer),

regardless of who paid the unlocking cost or who contributed how much, and the game ends.

- If the pair cannot manage to unlock the box, the game continues to stage 2 where the same pair of players are offered another box with a *lower* endowment multiplier.

- If they cannot manage to unlock the second box, they are again offered a third box with an even lower multiplier.

.... and so on...

The game in each treatment therefore ends at the stage where a box is unlocked, or when the multiplier of an offered box becomes unity (in which case no one is expected to unlock a box). Because multipliers of offered boxes decrease as the game continues to the next stage, the earlier the game ends, the higher the average take-home earnings.

Table 1. Payoff Structure

			Payoffs in MVD treatment			Payoffs in MTPG treatment (with own contribution c_{it} for player i at stage t)	
Stage	Box name	Mult.	Pay 30	Follow and co-player pays	Both Follow (play at:)	$c_{1t}+c_{2t} \geq 30$	$c_{it}+c_{jt}< 30$ (play at:)
1	Box10	10	700	1000	Stage 2	10*(100-c_{i1}	Stage 2
2	Box9	9	630	900	Stage 3	9*(100-c_{i1}-c_{i2})	Stage 3
3	Box8	8	560	800	Stage 4	8*(100-$\sum_1^3 c_{it}$)	Stage 4
4	Box7	7	490	700	Stage 5	7*(100-$\sum_1^4 c_{it}$)	Stage 5
5	Box6	6	420	600	Stage 6	6*(100-$\sum_1^5 c_{it}$)	Stage 6
6	Box5	5	350	500	Stage 7	5*(100-$\sum_1^6 c_{it}$)	Stage 7
7	Box4	4	280	400	Stage 8	4*(100-$\sum_1^7 c_{it}$)	Stage 8
8	Box3	3	210	300	Stage 9	3*(100-$\sum_1^8 c_{it}$)	Stage 9
9	Box2	2	140	200	Stage 10	2*(100-$\sum_1^9 c_{it}$)	Stage 10
10	Box1	1	70	100	100	100-$\sum_1^{10} c_{it}$	100-$\sum_1^{10} c_{it}$

The details of the game in each treatment and the parameters used are presented in Table 1. The starting endowment was set at 100 tokens for both treatments. The cost of opening a box was 30 tokens regardless of its multiplier. The two choices in the MVD treatment were presented as *Pay 30* and *Follow*. Players can choose to pay any amount between 0 and 30 tokens in the MTPG treatment. The multiplier of the box in the 1^{st} stage was 10; the multiplier of the box in the 2^{nd} stage was 9; and so on up to the 10^{th} stage, where the multiplier is just 1, and it does not make sense for either player to unlock the box. Boxes were named after their multipliers.

After decisions in each stage, each player in a pair receives feedback on the outcome (of course, if a player choses to pay 30 tokens in either treatment, he already knows the outcome). In the MVD game, successful followers at any stage of the game were asked to indicate at which stage they would have switched to paying had the game continued. Similarly, players in the MTPG treatment who contributed less than half for a successful opening of a box were asked to indicate at which stage they would have contributed at least half of the cost. This information was combined with the observed '*paying stage*' of those who actually ended up paying 30 tokens in the MVD treatment and contributing at least half if the unlocking cost in the MTPG treatment to construct a measure free-riding tendency for each player, referred to as the '*switching stage*'.

The experiments were conducted with paper and pencil among business and economics students at Mekelle University, Ethiopia. We used a between-sample design where each treatment was carried out on a different sample. 106 subjects (53 pairs) participated in the MVD treatment and 94 subjects (47 pairs) participated in the MTPG treatment. Each token in the experiment was valued at 0.10 Ethiopian Birr and all subjects were paid a show-up fee of 10 Ethiopian Birr .

3.2 Results and discussion

I will first describe how the games evolved in each treatment. The left side of Figure 2 shows how the game evolved in the MVD treatment. The extent of free-riding is striking: well over two-thirds of the subjects chose to follow in the first stage. Only one-fourth of those who chose to follow actually succeeded with their free-riding strategy, and the rest went on to play with each other in stage 2. The proportion of free-riding

is even higher in stage 2, where 83% of the players choose to follow. This trend continued, and none of those who reached stage 8 (about 8% of the total subjects) chose to pay after that, and hence failed to open any box. It therefore seems that there is a tendency to stick to free-riding among players who had the 'bad luck' of being paired with another free-rider.

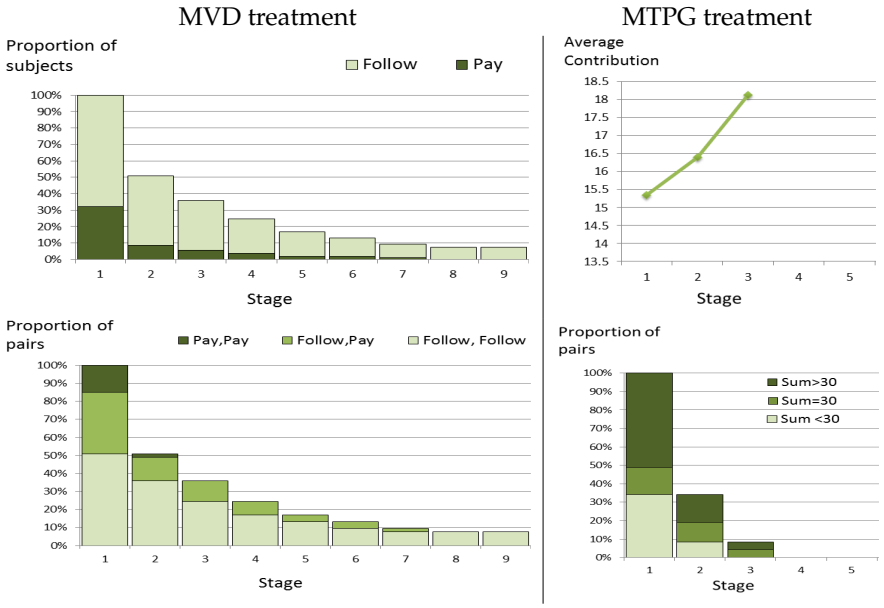

Figure 2. Evolution of the games

The right panel of Figure 2 presents the outcomes of the MTPG treatment. In contrast to the MVD treatment, it is evident that the game ends much more quickly in the MTPG treatment for the average pair. As the top panel shows, the average contribution at the first stage is half of the unlocking cost. But high variance means that 30% of the pairs could still not raise a sufficient amount to end the game at the first stage (with the average deficiency close to 13 tokens) and had to play the game at stage 2. The average contribution increases in stage 2 and for those who reach stage 3, and the game was over for all pairs by stage 3. That is, not only did all pairs manage to open a box in the MTPG treatment, they also did it much faster than the average pair in the

MVD treatment. Another difference between the two treatments is that, contrary to what happened in the MVD treatment, players who had the 'bad luck' of playing the game again moved away from free-riding.

The results in Figure 2 show that free-riding incentives embedded in social learning would create a delay in experimentation, much more so in a MVD type environment. But this is not enough to draw conclusions about the net effect of social learning. There is a need to factor out the positive effect of social learning that arises from the fact that not every farmer undertakes experimentation, even if it may come in late, as in the experiments, where not every player pays the unlocking cost. Social learning may therefore cause delay, but still the efficiency gains may be big enough to make it attractive. The experiment allows for making such welfare comparisons and identifying the net effect of social learning, both in the MVD and MTPG dilemmas. There are two natural benchmarks for such comparison. The first is a scenario where social learning is not possible. In the experiment, this would translate to a trivial choice where a single player either pays 30 in the 1^{st} stage and earns 700 tokens, or waits and earns less.[14] The second is a scenario where the maximum possible efficiency gain from social learning is reaped. This would translate into a situation where there is no delay in each of the experiments and exactly 30 tokens would be paid to unlock the first box by each pair, and hence the average payoff would be 850 tokens.

Table 2 presents the average payoffs for each treatment together with the average payoffs in each benchmark, as well as *end-stage* and the *switching stage.*[15] While the average switching stage can be considered a measure of free-riding tendencies, the end-stage indicates the delay of technology adoption that is caused by such free-riding tendencies.

[14] I did not have an experimental treatment for this because I thought it would be quite absurd to have an experiment asking subjects to choose between more and less money.

[15] The switching stage for the TPG is assumed to be the stage where a player would contribute at least half of the unlocking cost. Note that the switching stages for each treatment have values between 1 and 10, where 1 means a player pays 30 (contributes at least 15) at the first stage and 10 means a player free-rides until the end.

Table 2: Delay and welfare effects of social learning

	Benchmark 1 (no social learning)	Benchmark 2 - efficient social learning	VD treatment	TPG treatment
Average End-stage	1	1	2.72 (2.58)	1.42 (0.64)*
Average Switching stage	1	1	4.92 (3.72)	1.35 (0.96)
Average payoff	700 (0)	850 (150.7)	674.9 (238.8)	745.5 (116.9)

*Standard deviations in parentheses

Delay as measured by the average end-stage is significantly higher in the MVD treatment compared to the MTPG treatment (*two-tailed t-test; p-value<0.0001*), confirming the contrast we observed in Figure 2. The average payoff in the MVD treatment is lower by about 3.5% than it would have been if there were no possibility for social learning. Even if this difference marginally misses statistical significance (*two-tailed t-test, p-value=0.13*), it is still an important result. The result indicates that social learning takes place - not every one pays the cost of experimentation - but delay in experimentation has more than eaten up the efficiency gains from social learning. The message is quite strong: the average farmer in such an environment would be better off without social learning. On the contrary, payoffs in the MTPG treatment are higher than the no-social-learning benchmark by 6.5% (statistically significant at 1%). But it is important to note that MTPG payoffs are still much lower than the Pareto- efficient outcome (i.e., efficient cost-sharing) because of the small delay and wasted resources.

The average payoffs in Table 2 exactly match what happened in the experiments. It could be helpful to have a more general measure of welfare that takes into consideration that players may not be paired in the exact way they were in the experiment. The only variable that can be utilized in this respect is the switching stage. Given each player's switching stage, what would be the average payoff from a randomly matched pair? Such a value would give us a more general welfare measure that allows for random pairings of players with different free-riding tendencies. The results of this simulation exercise indicate that the average *expected* payoff from randomly matched pairs in the MVD treatment is 676 tokens. Again, this result shows that the inefficiency from experimentation delay outweights the benefits from social learning, when the learning process does not allow for sharing the burden of experimentation.

4 Social Preferences

The theoretical framework in Section 2 assumes homogenous information neighbors with complete information. The experiment followed suit by equalizing the endowment, experimentation cost and other payoff-relevant attributes of the games. This allowed for a simple and clear comparison of the two dilemmas. Indeed, such symmetry may not be far off from reality: information neighbors are likely to be very similar to each other in terms of agro-ecological properties, livelihood structure, or even wealth (e.g., Conley and Udry, 2010). But information neighbors may be different in a key behavioral aspect that cannot be controlled by design features of the experiment: social preferences. As indicated in the introduction, there is now ample evidence showing that behavior in strategic interactions could be influenced by non-material preferences, such as preferences on the distribution of payoffs from a given outcome. The existence of such preferences might affect the outcome of the social learning dilemmas. In addition to creating asymmetries in the 'real' utility that players get from a given outcome, social preferences can also create information asymmetries, as players could have limited information about their counterparts' social preference characteristics. Understanding the role of social preferences might therefore provide new insights into how the coordination problems operate in the real world, and shed some light on the experimental results in the previous section. This section will explore this issue, mainly focusing on the MVD game.[16]

The first step is to explicitly model social preferences in the MVD game. The literature offers a number of alternative models of social preferences (e.g., Fehr & Schmidt, 1999; Bolton & Ockenfels, 2000; Charness & Rabin, 2002; Battigalli & Dufwenberg, 2009). The inequality aversion model by Fehr and Schmidt (1999, F&S hereafter), provides a simple way of thinking about social preferences in dilemmas like the social learning problem. The basic F&S inequality aversion model stipulates that, in situations where there is a joint determination of payoffs through strategic interactions, inequality averse agents suffer proportionally to the mean distance between their 'material' payoff and others' payoff. When the inequality is in their favor, they suffer by the advantageous inequality aversion parameter β. When the inequality is against them,

[16]The focus on the MVD game is partly motivated by the aim of shedding more light on the deeper coordination problem observed in the experimental games. Moreover, the existence of social preferences does not bring major changes to the TPG game, but rather establishes the case for the equitable shared-experimentation focal point (e.g., Norman & Rau, 2012).

they suffer by the disadvantageous inequality aversion parameter α. It is assumed that $\alpha \geq 0, \beta \geq 0, \beta \leq 1$ and $\beta \leq \alpha$. For example, if n inequality averse players are involved in a strategic game where each player $i \in n$ earns material payoffs p_i from a given outcome, player i's utility from the outcome is:

$$p_i - \frac{\beta_i}{n-1} \sum \max(p_i - p_{-i}, 0) - \frac{\alpha_i}{n-1} \sum \max(p_{-i} - p_i, 0).$$

Adopting the above F&S-augmentation in the MVD game suggests that both successful free-riders and experimenters would suffer from any asymmetric outcome, but free-riders would suffer less than experimenters. This is important because all Pareto-optimal outcomes in the MVD game are asymmetric. To see how such preferences affect outcomes of the game, I will assume that players are homogenous in all other aspects except for their social preference parameters. I will also focus on a game with $n = 2$.

In the reduced form of the the F&S MVD game, as with the basic game, each player $i \in \{1, 2\}$ chooses an experimentation time $t_i^e \in \{0, 1, 2, ...T, T+1\}$ where $t^e = T + 1$ means the farmer never experiments. Let us say the game ends at some period t. If $t_i^e > t$, player i earns the F&S-augmented utility from free-riding F_{it}^{fs}. If $t_i^e = t$, player i earns the F&S-augmented utility from free-riding E_{it}^{fs}. Specifically:

$$F_{it}^{fs} = \sum_{0}^{t} v_t + \sum_{t+1}^{T} s_t - \beta_i C$$

$$E_{it}^{fs} = \begin{array}{c} \sum_0^t v_t + \sum_{t+1}^T s_t - (1+\alpha_i)C \text{ if } t_j^e > t;\ j \neq i;\ j \in \{1,2\} \\ \sum_0^t v_t + \sum_{t+1}^T s_t - C \text{ if } t_j^e = t;\ j \neq i;\ j \in \{1,2\} \end{array}$$

The basic properties of the MVD game introduced in Section 2 are still intact. Given the assumptions that $\beta_i \leq 1$ and $\beta_i \leq \alpha_i$, free-riding brings higher utility than experimenting at a given period, even in the presence of F&S preferences. Social preferences do not matter if both players experiment, because payoffs would be equal. A key change is that the payoff from experimenting at a given period t depends on whether or not the other player also experiments. Given that he experiments, player i would prefer if player j experiments too, as he would suffer from disadvantageous inequality otherwise. Note that this adds another layer to the experimentation dilemma created by social learning: not only does social learning bring incentives to delay own experi-

mentation in the hope of free-riding on others, it also makes experimentation carried alone more costly.

As it will be clear shortly, social preference affects the equilibrium property of the MVD as a form of payoff *heterogeneity* among players. It is therefore helpful to first understand how payoff heterogeneities among players affect the outcome of the game. In an attempt to focus the paper on the issue at hand, this is done separately in Appendix A. As can be seen in the Appendix, two concepts are relevant in this respect: the *redundancy period* and the *delayed-copying permissive (DCP)* property of payoffs. I will therefore briefly explain these two concepts and related results below.[17] After that, I will analyze the equilibrium properties of the F&S both under complete and incomplete information.

Simply put, the redundancy period, denoted as t_i^*, is the period where, because of delay, experimentation with a new technology ceases to be privately profitable for a given farmer i involved in the game. Note that the basic assumption on the private profitability of experimentation is only binding for experimentation carried without any delay. As time passes without experimentation, farmers will have less and less time left to benefit from the new technology after experimentation and they could eventually reach a point where the new technology is not worth experimenting with any more. As elaborated in Appendix A, the redundancy period has special importance to the equilibrium analysis of the MVD game for one key reason: free-riding becomes a dominant strategy at and after the redundancy period. The second concept, denoted as the delayed-copying permissive (DCP) property of payoffs, is related to the relative value of free-riding compared to the cost of delay. By adopting a free-riding strategy in a given period, players risk being *unsuccessful* free-riders and having to play the game again. Such players could therefore end up regretting their decision. But an unsuccessful free-riding decision at period t could be cause for regret immediately at $t+1$ or at a later period. We know that the game at $t+1$ is Pareto-inferior to the game at t because of the cost of delay. But it is still possible that a player gets a higher payoff by successfully free-riding at $t+1$ than experimenting at t. We say payoffs in the game are delayed-copying permissive (DCP) if they have this properly. If unsuccessful free-riding is regretted immediately, we say payoffs are non-DCP.

[17] While the terminology and the application to the agricultural technology problem is mine, the basic idea of the redundancy period is already shown by Bilodeau & Slivinski (1996) for a dynamic VD game with finite-time. The results related to the DCP property of payoffs are, however, entirely mine.

The redundancy period and the DCP property of payoffs have fundamental implications for the equilibrium of the MVD game with heterogenous players. Simply put, when players have complete information, differences in redundancy periods lead to a unique and Pareto-efficient SPE if the payoffs are DCP for the lower redundancy period player. In the two player game, the SPE is such that the player with the higher redundancy period experiments, while the player with the lower redundancy period free-rides. Differences in redundancy periods do not bring the desired Pareto-efficient SPE if payoffs are non-DCP. This is because non-DCP payoffs curtail the backward induction process that might have happened because of separation in redundancy periods.

4.1 Social preferences under complete information

I will specify the equilibrium implications of social preferences in the MVD game in light of the above results related to the role of player heterogeneities. Consider the two-player F&S MVD game with $\beta_1 = \beta_2 = \alpha_1 = \alpha_2 = 0$. Becasue we assume that the two players are identical in all other aspects, this simply becomes the basic MVD game. Both players in such a game would have the same redundancy period $t_1^* = t_2^* = t^*$.[18] Note that t^* is the period when E_{1t}^{fs} is not higher than F_{1T}^{fs} for the first time. As pointed out in section 2, such a game would have multiple equilibria at $t = 0$.

Now consider the game with $\alpha_1 > 0$. Because this implies that $\sum_0^t v_t + \sum_{t+1}^T s_t - (1+\alpha_1)C < \sum_0^t v_t + \sum_{t+1}^T s_t - C$, a high enough α_1 could bring forward the period where $E_{1t}^{fs} \leq F_{1T}^{fs}$. That is, if aversion towards disadvantageous inequality is strong enough, it effectively shifts the player's redundancy period backward from where it would have been if only material payoffs were considered. Denote the redundancy period without and with social preferences as t_i^* and t_i^{sf}, respectively. Then:

Lemma 1 *There is some α' such that $t_i^{sf} < t_i^*$ if $\alpha_i \geq \alpha'$.*[19]

It then follows that, if players are different in their α_i, they could end up having different F&S augmented redundancy periods (t_i^{sf}). Specifically, the player with a suf-

[18] Note that the material redundancy period t^* in our experiment is stage 10 for both players.

[19] Recall that the no-social-preferences t_i^* is where $\sum_0^T v_t \geq \sum_0^t v_t + \sum_{t+1}^T s_t - C$ for all $t \geq t_i^*$ and $\sum_0^T v_t < \sum_0^t v_t + \sum_{t+1}^T s_t - C$ for all $t \geq t_i^*$. That is, $\sum_0^T v_t \geq \sum_0^t v_t + \sum_{t+1}^T s_t - (1+\alpha_i)C$ for all $t \geq t_i^{sf}$ and $\sum_0^T v_t < \sum_0^t v_t + \sum_{t+1}^T s_t - (1+\alpha_i)C$ for all $t \geq t_i^{sf}$. Given these, it can be easily shown that there is some α' such that $t_i^{sf} < t_i^*$ if $\alpha_i \geq \alpha'$.

ficiently lower α_i has higher t_i^{sf}. Because the separation of redundancy periods leads to the emergence of a unique Pareto-efficient SPE at $t = 0$, a difference in α_i that is strong enough to lead to differences in t_i^{sf} then leads to the emergence of an efficient outcome in the F&S MVD game, provided that payoffs are DCP for the player with the higher α_i.

Proposition 2 *If α_i is sufficiently higher than α_j for two players i and j ($i, j \in \{1, 2\}$ in the F&S MVD game), then $t_i^{sf} < t_i^{sf}$. It also follows that, under complete information, the F&S MVD game has a unique SPE at $t = 0$ where $t_i^e = T+1$ and $t_j^e = 0$ if F&S-augmented payoffs are DCP for player i.*[20]

That is, if players are sufficiently different in their social preference parameters, and know about each other's α, the player who suffers less from disadvantageous inequality would undertake the experimentation, while the player who suffers more from disadvantageous inequality ends up free-riding. Differences in aversion towards advantageous inequality do not matter as long as the assumption that $\beta_i \leq 1$ holds.[21]

Social preferences can also aid in efficient coordination in the MVD game in another way: they can change a borderline non-DCP (material) payoffs into a DCP F&S-augmented payoffs. Consider a technology that is non-DCP for player i when social preferences are not considered, i.e., $F_{i(t+1)} = \sum_0^{t+1} v_t + \sum_{t+2}^{T} s_t < E_{it} = \sum_0^t v_t + \sum_{t+1}^{T} s_t - C$. When social preferences matter, $F_{i(t+1)} = \sum_0^{t+1} v_t + \sum_{t+2}^{T} s_t - \beta_i C$ and $E_{it} = \sum_0^t v_t + \sum_{t+1}^{T} s_t - C - \alpha_i C$. We know that $\beta_i \leq 1$. Hence $\beta_i C \leq C$. But since α_i does not have an upper bound, there is some α^* where $\sum_0^{t+1} v_t + \sum_{t+2}^{T} s_t - \beta_i C < \sum_0^t v_t + \sum_{t+1}^{T} s_t - C - \alpha_i C$ for all $\alpha_i \geq \alpha^*$ and $\sum_0^{t+1} v_t + \sum_{t+2}^{T} s_t - \beta_i C \geq \sum_0^t v_t + \sum_{t+1}^{T} s_t - C - \alpha_i$ for all $\alpha_i < \alpha^*$. Hence, payoffs become DCP for player i if $\alpha_i \geq \alpha^*$. Again, what matters is the intensity of aversion towards disadvantageous inequality.

The above results show that the existence of F&S-like social preferences can help in bringing about efficient coordination in an otherwise multiple-equilibria game, both by creating separation in redundancy periods and by adjusting the utilities from payoffs so that backward induction happens. A few things are worth mentioning about these results. First is the implication about 'what type of person' ends up undertaking

[20] The proof is similar as that for Proposition 6 in Appendix A.

[21] It is easy to see that the equilibrium properties of the above F&S game are exactly the same as the basic game in section 2 if players are identical in their aversion toward disadvantageous inequality (i.e. $\alpha_1 = \alpha_2$) and have complete information.

the experimentation (i.e., supplying the public good) in such a situation. Imagine a game between a selfish player (i.e., $\beta = 0$ and $\alpha = 0$) and a non-selfish player with positive β and sufficiently high α, who are otherwise homogenous. The above result predicts that the selfish player supplies the public good while the 'non-selfish' player free-rides. Considering the usual association of social preferences with cooperative behavior, this is a pretty surprising turn of roles. Second, the MVD game would still have multiple equilibria (i.e., as in the basic game) if players are identical in their aversion toward disadvantageous inequality (i.e., $\alpha_1 = \alpha_2$) and have complete information. Third, one can be sure that social preferences lead to a unique and efficient SPE at $t = 0$ only if players would have had the same redundancy periods when social preferences were not considered. If players have different 'material' redundancy periods (t_i^*) to begin with, it is possible that social preferences 'equalize' the F&S redundancy periods (t_i^{sf}) if the player with the higher t_i^* also has proportionally higher α_i, just enough to 'correct' the difference between t_i^*. Therefore, social preferences can also play a negative role in the coordination problem by creating multiple equilibria.

4.2 Social preferences under incomplete information

The assumption so far was that all players in an MVD game know the extent of each others' aversion to disadvantageous inequality. The results indicate that, if otherwise symmetric information neighbors know each other so well that they also know who will be hurt the least from inequality, they leave the experimentation to that specific farmer and the rest free-ride. But the assumption that everyone knows about each others' social preference parameters is not warranted. Players are most likely to know their own α but to be uncertain about their information neighbors' α. This would mean that players know their own t_i^{sf} but not that of their information neighbors. That is, even if they know the time period after which they will never experiment, they do not know for certain when their neighbors will switch to never experimenting. How would such information asymmetry affect the outcomes of the MVD game?

Consider a game between two players, 1 and 2, that is symmetric in material payoffs. Both players have the same material redundancy period, i.e., $t_1^* = t_2^* = t^*$. Since β is not relevant, assume that $\beta_1 = \beta_2 = 0$. Each player $i \in \{1, 2\}$ knows his own α_i. Moreover, assume that both players have a common prior about the probability distribution of α, represented by a cumulative distribution function $F(\alpha)$, positive over the

interval $[0, \infty]$.

The first implication is that, for each period t in the game, there is some α_t^f such that a player with $\alpha_i \geq \alpha_t^f$ has an F&S redundancy period $t_i^{sf} \leq t$. That is, for each period t, a player with α_t^f or higher would have a dominant strategy to free-ride at t. Put differently, player i is *in* the game at t if he has $\alpha_i < \alpha_t^f$ and *out* of the game at t if he has $\alpha_i \geq \alpha_t^f$. Players therefore know whether they are in or out of the game at any given period, but have only a probabilistic belief as to whether their counterpart is in or out. In the extreme, this implies that farmers could seek to free-ride on a neighbor whose α is so high that he would actually never experiment even if experimentation is materially profitable.

Unlike players who are out at a given period, players who are in do not have a dominant strategy to free-ride. The behavior of the in players therefore depends on their expectations about the behavior of their counterpart as represented by $F(\alpha)$ and the material payoffs in the game. To the extent that they believe the other player is out of the game, they respond best by experimenting immediately. To the extent that they believe the other player is in the game, they decide based on their expectations about the player's α actions given their own α. Given these, a symmetric equilibrium for 'in' players at a given period t can then be characterized as a decision rule where a player experiments at t if his α_i is at least higher than a threshold α_t^*; and free-rides if $\alpha_i < \alpha_t^*$. Note that α_t^* cannot be higher than the dominant strategy triggering value α_t^f.

Now consider the game at $t = 0$ from the perspective of a player i who is in (i.e., $\alpha_i < \alpha_0^f$). Define the equilibrium in such a case where a player experiments with probability ρ_0^* and free-rides with probability $1 - \rho_0^*$. The equilibrium property should satisfy the equilibrium condition that neither player gains from deviating, given that each player is using the threshold α_0^*, and hence experimenting with probability ρ_0^*. Imagine that such a symmetric equilibrium was played out in $t = 0$ but that experimentation did not take place. The players now face each other in $t = 1$ with the new knowledge that everyone's α is at least as high as α_0^*. That is, they update the probability distribution $F(\alpha)$. Consequently, the equilibrium threshold for $t = 1$ will also be updated to $\alpha_t^* \geq \alpha_0^*$, together with the equilibrium probabilities; and the game will proceed as in $t = 0$. This iterative process continues until the period when at least one of the players' α is part of the experimentation segment.

This shows that uncertainty can lead to delay in experimentation even when play-

ers differ in their aversion toward disadvantageous inequality. It could be some time before an inequality-averse farmer is convinced that his neighbor is probably much more inequality-averse than he is, and hence he had better undertake the experimentation himself before it is too late and he is out of the game for good. Moreover, such a decision process can also result in over-experimentation at any given period (i.e., the αs of both players lie in the experimentation segement at a given period), taking away the efficiency gain from social learning

This brings a new insight into what might have driven the inefficiency in the MVD experiment. As indicated earlier, the possibility that players may be motivated by social preferences cannot be rejected. It then follows that the subjects in the MVD treatment might have played an incomplete information game. That also means the observed inefficiency might have been a result of the asymmetry of information about social preferences. Both the delay in experimentation and over-experimentation observed in the MVD treatment are consistent with what would happen in a game of imperfectly informed inequality averse players.

Similarly, the possibility that social preferences might have played a role in in the MTPG experiment cannot be rejected. Contrary to the MVD treatment, F&S like preferences might play a positive role in the MTPG treatment, even when players have incomplete information. This is because aversion toward advantageous inequality also becomes an important factor, further strengthening the case for the shared-experimentation equilibria (see, e.g., Norman and Rau, 2012). The treatment effect we observed in our experiment is therefore consistent with the possibility that players might have been influenced by social preferences.

5 Concluding Remarks

This study is based on the premise that learning is at the core of technology adoption in agriculture. Learning is the process by which farmers gather and evaluate information about relevant attributes of new technologies. Not only do farmers need information to know whether a technology is suitable to them, they also need information on how to use a new technology. Farmers often undergo a costly experimentation to get such critical information. But, with the presence of social learning possibilities, not every farmer has to carry the burden of experimentation. It is cheaper to delay one's own

experimentation and learn from others. The paper sought to answer whether these free-riding incentives embedded in social learning delay the adoption of potentially profitable technologies.

First, I show that incentives to free-ride in social learning create a *coordination problem*, but the nature of the specific coordination game depends on whether the burden of experimentation can be shared. The indivisible experimentation problem is formulated as an MVD game, while the divisible experimentation problem is formulated as an MTPG game. Then I present experimental evidence evaluating the net effect of social learning in each game. The two games are then compared. The core result from the experiments is that the negative externalities from social learning are likely to be much higher when experimentation is indivisible. Even if complete failure of experimentation is rare, only happening in less than 10% of our games, the cost of delay in experimentation is high enough that the net effect of social learning is negative. Things improve significantly when there is the possibility to share the experimentation task. However imperfectly, most players manage to share the burden in time and earn more than they would have if it were not possible to share costs or copy from others.

The significant difference between outcomes in the MVD and MTPG treatments shows that the capacity to free-ride on one's neighbor and the capacity to cooperate with him are not very far from each other. Free-riding incentives in social learning lead to delay in experimentation mainly because farmers lack a means of coordinating their actions. Mechanisms that support shared-experimentation could therefore be helpful in reducing negative externalities of social learning. For example, new technologies can be promoted in a way that gives incentives for information neighbors to form an 'experimentation group' in a similar manner to credit groups in microfinance or marketing cooperatives.

The second half of the paper explores the potential role that social preferences could play in the coordination games created by social learning, especially focusing on the MVD game. The lessons from the exercise can be summarized as follows: (i) under complete information, an F&S MVD game has the same properties as the basic MVD game if players are identical in their aversion toward disadvantageous inequality; (ii) under complete information, heterogeneity in social preferences can serve as a means of coordinating in an efficient outcome; (iii) social preferences can contribute to the emergence of efficient experimentation in the MVD game by 'smoothing' the link

between utilities from payoffs of different periods, and triggering backward induction; (iv) delay in experimentation and over-experimentation can happen if players have incomplete information about other people's social preference parameters. On the other hand, presence of social preferences is expected to further establish the case from efficient equilibria in the MTPG game, even under incomplete information. The experimental results in Section 3 are consistent with these patterns.

All in all, the paper shows that social learning could lead to the seemingly puzzling outcome that farmers do not take up new methods and technologies that are potentially profitable. Delay in the adoption of a *good* technology can happen as a result of coordination problems in the learning process. Even small delays caused by such simple coordination problems could have serious ramifications in the real world. For example, they could lead to the erroneous conclusion that farmers are not taking up new technologies because they are not profitable, potentially leading to the scrapping of good technologies. Understanding these effects requires incorporating the behavior and actions of agents who promote new technologies into the coordination game, which is left for further research. More research is needed to identify the kinds of technologies and environments that are susceptible to such coordination problems, as well as to develop new policy tools.

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Appendix A:

MVD game with heterogeneous infomation neighbors

Here I will formally define the redundancy period and the DCP property of payoffs, as well as elaborate on their importance to the equilibrium properties of the MVD game when players are not homogenous.

Consider the game with n information neighbors who are no longer assumed to be identical, so that each farmer i will have a life span T_i. Let F_{it} and E_{it} denote farmer i's payoffs from successful free-riding and experimenting in each period $t \in \{0, 1, 2, ..., T_i\}$, respectively. Without making any specific assumptions about the flows of returns, let us keep the basic properties of payoffs in the MVD game the same: (i) Free-riding brings a higher payoff for any given period than experimentation ($F_{it} > E_{it}$); (ii) Payoffs from free-riding and experiments are strictly decreasing in time; (iii) Experimentation is privately profitable at $t = 0$; (iv) it does not make sense to undertake experimentation at T_i. We know from (iv) that farmers will never experiment during their last period because they will not have time to benefit from the new technology. Would the same farmer get a positive return by experimenting at $T_i - 1$? The answer depends on whether the return from the new technology at T_i is higher than the cost of experimentation. If the return does not cover the experimentation cost, the farmer will not experiment at $T_i - 1$ either. Following such an inquiry backwards, we will reach a period where the return from the new technology offsets the experimentation cost. In summary:

Lemma 3 *There is a time period t_i^* for each player i such that $E_{it} \leq F_{iT_i}$ for all $t \geq t_i^*$ and $E_{it} > F_{iT_i}$ for all $t < t_i^*$.*

Lemma 4 *$t_i^e = T_i + 1$ for all $t \geq t_i^*$.*

t_i^* is the redundancy period of farmer i. As stated in Lemma 2, if a farmer does not undertake the experimentation before his redundancy period, he will *never* experiment. Put differently, free-riding becomes a dominant strategy for farmers once they reach their redundancy periods. The redundancy period, or rather differences in the redundancy periods of players, has a fundamental implication for the equilibrium of the MVD game. The effects of heterogeneity among information neighbors on the

game's equilibrium evolution is also connected to whether such heterogeneities create differences in redundancy periods. To see this in a simple way, we will hereafter focus on a game with two players, 1 and 2. The equilibrium effect of differences in redundancy periods depends on an important payoff characteristic, namely the delayed-copying permissive (DCP) property:

Definition 5 *We say that payoffs are **delayed-copying permissive (DCP)** for a given player $i \in \{1, 2\}$ if experimentation cost and returns are such that $F_{i(t+1)} \geq E_{it}$ for all $t < t_i^*$. We say that payoffs are **delayed-copying non-permissive (non-DCP)** for a given player $i \in \{1, 2\}$ if $F_{i(t+1)} < E_{it}$ for all $t < t_i^*$.*

A DCP technology is simply one where the cost of experimentation and returns are such that farmers would still get a higher return from successful free-riding during period $t+1$ than from experimenting during period t, because regret is not immediate when a technology is DCP. That is, the cost of experimentation is so high that a farmer would still choose to suffer one more period of delay if he 'knows' that his neighbor will experiment in the next period. Put differently, under DCP technology, players have one more chance for free-riding to bring higher payoffs, even if free-riding was not successful the first time around. Players in a non-DCP technology have only one chance of benefiting from free-riding compared to what they would get from experimenting in a given period. Note that the incentive to free-ride in a given period is intact in both DCP and non-DCP technologies. Moreover, a technology may be DCP for one farmer in an information neighborhood and non-DCP for another.

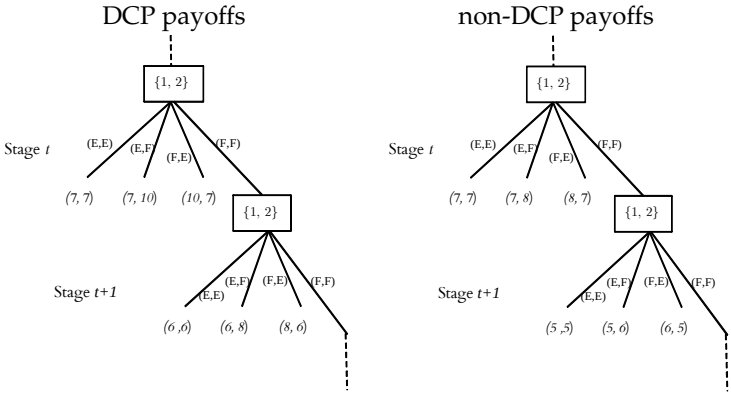

Figure A1. Examples of games with DCP and non-DCP payoffs

Given these conditions, the equilibrium of the MVD game with heterogeneous players (i.e., $t_1^* \neq t_2^*$) will be as follows:

Proposition 6 *Let $t_1^* > t_2^*$; then, under complete information, the MVD game has a unique SPE at $t = 0$ where $t_1^e = 0$ and $t_2^e = T_i + 1$ if payoffs are DCP for player 2.*

Proof. Let us consider the game at t_2^*. Player 2's dominant strategy at $t \geq t_2^*$ is to free-ride. Because player 1 knows this, he will choose to experiment at t_2^* as $E_{1t_2^*} > F_{1T_1}$. So we have a unique Nash equilibrium at t_2^* where player 1 experiments and player 2 free-rides. As a result, the game at $t_2^* - 1$ would be such that the payoffs if both players free-ride are $\left(E_{1t_2^*}, F_{2t_2^*}\right)$. Because $F_{2t_2^*} \geq E_{2(t_2^*-1)}$ by the DCP property, the game at $t_2^* - 1$ would also have a unique SPE where player 1 experiments and player 2 free-rides. Following the same logic of backward induction, the game would have a unique SPE at $t = 0$ where player 1 experiments and player 2 free-rides ■

Corollary 7 *Let $t_1^* > t_2^*$; the MVD game does not have a unique SPE at $t = 0$ if the technology is non-DCP for player 2.*

Proof. The game at t_2^* has a unique Nash equilibrium where player 1 experiments and player 2 free-rides. The game at $t_2^* - 1$ would be such that the payoffs if both players free-ride are $\left(E_{1t_2^*}, F_{2t_2^*}\right)$. Because $F_{2t_2^*} < E_{2(t_2^*-1)}$ by the non-DCP property, the game at $t_2^* - 1$ no longer has a SPE. As a result, the game at $t = 0$ also has multiple equilibria. ■

The above results show that, if heterogeneities among information neighbors are such that they have different redundancy periods, the farmer with the largest redundancy period does the experimentation and all others free-ride, provided that the technology is DCP for that farmer.[22] That is, there would not be any delay caused by free-riding incentives in social learning.

The coordination power of differences in redundancy periods is first shown by Bilodeau & Slivinski (1996) for a general dynamic VD game with finite-time. However, they analyze a continuous-time game that implicitly assumes away the relevance of

[22]When there are more than two players, it is not necessary that all players have different redundancy periods for the unique equilibrium to arise. What is required is that there is a *single* player with a redundancy period higher than that of all the others (the rest of the players might as well have the same redundancy period). In other words, there would still be multiple equilibria in the game if two players have a tie for the highest redundancy period and all others have different redundancy periods.

the DCP property for the uniqueness of the SPE at the start of the game. The above results, however, show that the DCP property is actually a necessary condition for the unique and efficient equilibrium to arise when the game is played in discrete-time, as in our social learning problem. If the technology is non-DCP for the high redundancy period farmer, the equilibria of the game would be as if the information neighbors were homogenous. In other words, non-DCP technologies mute the power of redundancy period separation in bringing efficient social learning.

Paper III

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Does Positional Concern Matter in Poor Societies? Evidence from a Survey Experiment in Rural Ethiopia

ALPASLAN AKAY
IZA, Germany
University of Gothenburg, Sweden

and

PETER MARTINSSON and HAILESELASSIE MEDHIN *
University of Gothenburg, Sweden

Summary. — We investigate attitudes toward positionality among rural farmers in Northern Ethiopia using a survey experiment. On average, we find very low positional concerns both for income *per se* and for income from aid projects. The results support the claim that positional concerns are positively correlated with absolute level of income. The implications of our results on implementation of aid projects are discussed.

Key words — Africa, Ethiopia, positional concern, relative income, survey experiment

1. INTRODUCTION

Positional (or status) concern is a frequently discussed and well-documented phenomenon in developed countries (e.g., Clark, Frijters, & Shields, 2008; Johansson-Stenman, Carlsson, & Daruvala, 2002; Solnick & Hemenway, 1998). Veblen (1899/2005, p. 36) introduced this issue as a broad phenomenon across society by writing "no class of society, not even the most abjectly poor, forgoes all customary conspicuous consumption." A result of positional concerns is that the utility derived from a good depends not only on the absolute amount of the good consumed, but also on the amount of the good consumed relative to the amount consumed by others. Positional concerns have been empirically investigated very recently using data on self-reported happiness (or "subjective well-being" or "life satisfaction") from surveys and also from survey experiments. [1] In the happiness framework, the average income of others (often denoted "comparison income" or "relative income") is used as an indicator to measure positional concerns. The impact of relative income on happiness is then studied, while controlling for the subject's own absolute income. The general conclusion from this line of research is that happiness is significantly and negatively affected by the income of others in developed countries (e.g., Clark & Oswald, 1996; Clark *et al.*, 2008; Ferrer-i-Carbonell, 2005; Luttmer, 2005; McBride, 2001), whereas more mixed results are found in developing countries (e.g., Kingdon & Knight, 2007, in South Africa; Akay & Martinsson, 2011, in Ethiopia; Kingdon, Song, & Gunatilaka, 2009, in China; Bookwalter & Dalenberg, 2009, in South Africa; Knight & Gunatilaka, 2010, in China). Tailored survey experiments constructed to explicitly identify the degree to which individuals care about absolute and relative income or consumption have also shown that people do have positional concerns both for income and for consumption of specific goods, such as cars and holidays (see, e.g., Frank, 1999; Hirsch, 1976, for a general discussion and for example, Alpizar, Carlsson, & Johansson-Stenman, 2005; Carlsson, Johansson-Stenman, & Martinsson, 2007; Johansson-Stenman *et al.*, 2002; Solnick & Hemenway, 1998, 2005, 2007, for experimental findings). [2, 3] Note that these two approaches are based on different utility considerations. For example Kahneman, Wakker, and Sarin (1997) and Kahneman (2000) discuss the importance of distinguishing between experienced utility and decision utility, where the happiness is an ex-post experienced utility and the decision is an ex-ante utility. These two measures of utility will for sure show the same utility when the individual is rational and fully informed. However, it is likely that these two requirements are not completely fulfilled, but still the research using happiness data and survey experimental data show same tendencies regarding concerns for positionality.

There is growing evidence that the impact of relative concerns might be heterogeneous across countries with different national incomes. Particularly, it is a widely held view that the positional concerns of very poor people are lower than those of rich people. Frey and Stutzer (2002) argue that, when absolute income is above some subsistence level, other factors such as relative income start to influence well-being. In a recent paper, Clark *et al.* (2008) discuss this issue more generally and argue that positional concerns increase as one moves from poorer to richer countries. In another cross-country study, which covered eight countries, Corazzini, Esposito, and Majorano (2009) found that the importance of relative concerns in the perception of poverty increases as one goes from developed to developing countries. Hence, positional concerns

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may also influence people in very poor countries. Yet, we know very little about positional considerations among very poor individuals since research on the issue, using either happiness or experimental data, is generally based on data from Western countries, which are rich in absolute terms. Using happiness data from a large household survey in extremely poor villages in the highlands of rural Ethiopia, Akay and Martinsson (2011) show that people do not seem to be concerned with their relative income position. Kingdon *et al.* (2009) find, for relatively poor rural Chinese households, that the income of other rural households is positively correlated with their happiness. Bookwalter and Dalenberg (2009) find a similar positive effect on happiness for expenditures in South Africa. Caporale, Georgellis, Tsitsianis, and Yin (2009) find that relative income has a negative impact on happiness in Western European countries, while the opposite is found for Eastern European countries, which have lower absolute income levels. The positive relative income effect found in communal societies such as rural China and transition countries such as Eastern Europe can be explained by for example "altruism," Hirschman and Rothschild's (1973) type "tunnel effect," or "demonstration effect" hypotheses (Ravallion & Lokshin, 2000; Senik, 2004; Kingdon *et al.*, 2009; Hirschman & Rothschild, 1973). In villages with kin relationships or high degree of social capital, we may observe higher intra-group solidarity, and hence altruism, among the members of the village. On the other hand, as Hirschman and Rothschild argue, if a person observes an increase in the income level of people in his/her reference group, then he/she may perceive good prospects for a future increase in his/her own income, which may result in positive positional concerns.

The objective of the present paper is to test whether positional concerns matter among extremely poor people in Northern Ethiopia. We use a survey experiment to overcome some of the potential difficulties and biases when using happiness data especially related to accurate measurement of income and determination of reference group. Any study aiming to analyze the positional concerns based on a happiness framework should of course use an accurate measure of income and this is difficult to obtain in farming societies in developing countries. In developed Western countries, income is typically easy to measure since most people have a salary. They might also be eligible for various allowances, which are usually well-documented. However, in farmer societies, a substantial part of people's income comes from own farming activities in addition to seasonal work. The problem is, first, recalling amount harvested and, second, converting the outcome from farming activities to an income measure. Moreover, it is difficult to report income per time unit due to seasonality in income related to when harvest occurs. Another important issue when analyzing positional concerns is to determine the actual "reference groups" of the individuals, as discussed in Clark and Senik (2010). In most data sets, no information on individuals' reference groups has been collected. To reduce the potential bias from ad-hoc created reference groups, the researcher should test the robustness of the findings by creating many different reference groups (Kingdon *et al.*, 2009; Clark & Senik, 2010; Senik, 2009).

We use two survey experiments that focus on positional concerns in two different dimensions: (i) yearly income from all sources and (ii) income from an aid package. In both cases we use a similar design as in, for example, Alpizar *et al.* (2005). The experiments were conducted in Northern Ethiopia,[4] which is one of the poorest regions in the world. We surveyed farmers in the village of *Abraha We Atsbaha*[5] in the Tigray Regional State. In this region, most people depend on rain-fed subsistence agriculture. Thus, using the first experiment, we test for positional concerns for overall income and, more specifically, for the claim in Clark *et al.* (2008) that positional concerns for income are lower among the very poor—which to our knowledge is an empirical question that is untested via survey experiments. Moreover, the region is known for its recurrent droughts, and it was also the location of the 1984 Ethiopian famine, which killed more than 1 million people. As a result, the area has received substantial humanitarian aid (e.g., Dercon, 2004; Jayne, Strauss, & Yamano, 2002; Jayne, Strauss, Yamano, & Molla, 2001) and aid-based development activities such as food-for-work programs (e.g., Holden, Barrett, & Hagos, 2006), in addition to farm and health extension services, productive safety net programs, and cheap credit packages. The second experiment tests whether there are positional concerns for income from an aid package. Thus, this paper also contributes to the methodological discussion on the positional concerns of the poor and investigates particularly whether positionality is an issue to be considered for aid-based development projects in these regions of the world. This is a potentially important issue for the well-being of those who receive a smaller aid package or are not selected to receive any aid package at all. Aid packages are often unevenly distributed among households in a village, especially when related to new agricultural technology or other investments, resulting in income differences among households.[6] If there is substantial concern for positionality among people, then it is possible that the overall welfare[7] effect of an aid package is negative. Theoretically, such welfare loss can be corrected by incorporating appropriate adjustments to the optimal tax rules applied (e.g., Aronsson & Johansson-Stenman, 2008). In practice, such adjustments are difficult in economies with progressive income tax, let alone in this part of Ethiopia where the only tax is an annual land tax ("*gibri meret*" in Tigrinya, which is the local language). Thus, understanding the nature and level of positionality is therefore an important input in the design of aid-financed development programs. The remaining part of the paper is organized as follows. Section 2 gives the design of the experiment and the method behind it. Section 3 presents the results. The last section discusses the implications of our results on the provision of development programs and concludes the paper.

2. EXPERIMENTAL DESIGN

The two most common ways to model relative position in a utility framework are (i) a ratio comparison utility function, $U = v(x, x/\bar{x})$, where x is the individual's income and $\bar{x}$ is the average income in the reference group (e.g., Boskin & Sheshinski, 1978; Layard, 1980; Persson, 1995) and (ii) an additive comparison utility function, $U = v(x, x - \bar{x})$ (e.g., Akerlof, 1997; Knell, 1999; Ljungqvist & Uhlig, 2000). In the present paper, we chose to apply the following additive comparison utility function:

$$v = (1 - \gamma)x + \gamma(x - \bar{x}),$$

where γ measures the marginal degree of positional concern, that is, the proportion of the total change in utility that comes from an increase in relative income after a marginal increase in own income.

In order to test the effect of positional concern in both dimensions, that is, income *per se* and income from aid package, we applied a survey experiment. We created a scenario describing the situation where individuals are about to make a decision. In the experiment on income, subjects were told that they could choose to live in one of two villages and that

the annual income (from all sources) earned by them and other people differed in each village. The income was described as the annual income in Ethiopian birr (ETB).[8] Both experiments consisted of six pair-wise choices.

The income across the choice sets was constructed to measure the degree of positional concern. For example, in the first choice situation, an individual's yearly income in alternative A was ETB 2,990 and the average yearly income for others in the village was ETB 3,900; in alternative B, the individual's yearly income was ETB 2,860 and the average yearly income in the village was ETB 2,600 (See Box 1 in Appendix A for a full description of the scenario read to the subjects).[9]

If a respondent was indifferent between living in these two villages, then we have, in the case of the additive comparison utility function, that

$$x_A - \gamma\bar{x}_A = x_B - \gamma\bar{x}_B.$$

The marginal degree of positional concern for the first choice set can be calculated as

$$\gamma = \frac{x_A - x_B}{\bar{x}_A - \bar{x}_B} = \frac{2990 - 2860}{3900 - 2600} = 0.1.$$

Thus, the marginal degree of positional concern γ is equal to 0.1 in this case. If the respondent chose to live in village A, then $\gamma < 0.1$, and vice versa. The subjects were asked to make repeated choices between the two villages, which provided information about their degrees of positional concern, at least within an upper and lower bound. The construction of our survey was such that alternative A remained the same throughout the experiments, while the levels varied for alternative B. The levels used in the experiments, together with the implicit marginal degree of positional concern, are summarized in Table 1.[10]

We determined the implicit marginal degree of positional concerns for a specific individual as follows. If an individual preferred alternative A over alternative B_i ($i = 1$–6), then we knew that the marginal degree of positional concern was below the level calculated at that specific choice situation; if alternative A was not chosen, then we knew that the marginal degree of positional concern was at least equal to the level calculated at that specific choice situation. In the survey, the respondents were asked to make choices until alternative A was chosen, meaning that each respondent was presented with a maximum of six choices for each type of income.[11]

The aid package experiment was constructed in the same way. Subjects were told that they could choose between two bee-keeping packages, which differed in the income they would generate to them and to other people in the village. The income from bee-keeping in a village was described as the annual income for them and the mean annual income for the others in the village from this activity. This was then followed by six paired choices to trace the marginal degree of positionality in a similar manner as in the experiment described above. (See Box 2 in the Appendix A for a full description of the scenario read to the subjects).

Since a large fraction of our subjects had no formal education and many were illiterate, the instructions were given verbally in the local language (Tigrinya), and the same procedure was applied for the socioeconomic questions. To check for consistency, the instructions were first translated to the local language and then translated back to English by two different individuals. The experiment was conducted at the village farmers' association hall. A total of 150 randomly selected[12] household heads were contacted and asked for their willingness to participate in an economic experiment a few days prior to the experiment date. Ninety-four of the invited farmers showed up and the experiment was conducted with three trained enumerators (all three authors were present during the whole process; one of the authors is fluent in Tigrinya).[13]

3. RESULTS

Table 2 shows the frequency distribution of the marginal degree of positional concerns for the two survey experiments. Most of the subjects chose alternative A in the first choice situation (60.6% and 65.7% for income *per se* and income from aid packages, respectively) and thus indicated a very low degree of positional concern. Approximately two-thirds of the respondents showed the same degree of positionality in both experiments (50% were nonpositional in both experiments). The calculated median degree of positionality was zero for both experiments, and the mean degree of positionality was 0.158 for income *per se* and 0.177 for income obtained from the aid projects.[14] The results obtained from raw data support the hypothesis that the concern for relative position in poor societies is a very small component of people's total utility.

We also analyzed the degrees of positional concern in the two experiments using regression analyses. Table 3 reports interval regressions for the two experiments, where the marginal degree of positionality is explained by socio-demographic and economic characteristics of the subjects.[15,16] Income and land size had no significant influence on positional concerns. Female subjects were significantly less positional than male subjects in both experiments. People with higher church attendance seem to be less positional in the aid-package experiment.[17,18]

The regression results presented above can also be used to estimate the mean degree of positional concern conditional on socio-demographic and economic characteristics of the subjects. To calculate the mean degree of positional concern as well as confidence intervals, we used the bootstrap technique (see, e.g., Efron & Tibshirani, 1998). We first predicted the marginal degree of positionality for each individual using

Table 1. *Alternatives in experiment*

	Income experiment		Aid-package experiment		Implicit marginal degree of positionality
	Own income	Average annual income in the village	Own income from aid package	Average annual income from aid package in the village	
Alternative A	2,990	3,900	690	900	
Alternative B_1	2,860	2,600	660	600	0.1
Alternative B_2	2,730	2,600	630	600	0.2
Alternative B_3	2,600	2,600	600	600	0.3
Alternative B_4	2,470	2,600	570	600	0.4
Alternative B_5	2,340	2,600	540	600	0.5
Alternative B_6	2,210	2,600	510	600	0.6

Table 2. *Results of the experiments*

		Total income							Total	Freq.	Cum. Freq.
	Marginal degree of positionality	$\gamma < 0.1$	$0.1 \leqslant \gamma < 0.2$	$0.2 \leqslant \gamma < 0.3$	$0.3 \leqslant \gamma < 0.4$	$0.4 \leqslant \gamma < 0.5$	$0.5 \leqslant \gamma < 0.6$	$\gamma \geqslant 0.6$			
Income from aid package	$\gamma < 0.1$	48	2	3	7	2	0	0	62	65.66	65.66
	$0.1 \leqslant \gamma < 0.2$	1	2	0	2	1	0	0	6	6.38	72.34
	$0.2 \leqslant \gamma < 0.3$	1	0	0	1	0	1	0	3	3.19	75.53
	$0.3 \leqslant \gamma < 0.4$	7	0	1	7	1	0	0	16	17.02	92.55
	$0.4 \leqslant \gamma < 0.5$	0	0	0	0	2	0	0	2	2.13	94.68
	$0.5 \leqslant \gamma < 0.6$	0	0	0	0	1	1	0	2	2.13	96.81
	$\gamma \geqslant 0.6$	0	0	0	0	2	0	1	3	3.19	100.0
Total		57	4	4	17	9	2	1	94		
Freq.		60.64	4.26	4.26	18.09	9.57	2.13	1.06			
Cum. Freq.		60.64	64.89	69.15	87.23	96.81	97.87	100.0			

Table 3. *Interval regression estimates of the degree of positionality*

	Total income		Income from aid projects		Mean (std.)
	Coefficient (std. err.)	*p*-Value	Coefficient (std. err.)	*p*-Value	
Age/100	0.437 (0.711)	0.539	0.923 (0.767)	0.229	0.430 (0.146)
Age-squared/10,000	−0.595 (0.732)	0.416	−0.896 (0.757)	0.236	0.206 (0.142)
Married	0.053 (0.033)	0.113	0.066 (0.039)	0.086	0.787 (0.412)
Female	−0.080 (0.041)	0.049	−0.087 (0.038)	0.022	0.511 (0.503)
Good health status	−0.002 (0.036)	0.964	0.034 (0.035)	0.322	0.712 (0.454)
Number of adults at home	−0.007 (0.016)	0.659	0.006 (0.016)	0.701	2.691 (1.304)
At least one year of education	−0.053 (0.036)	0.144	0.007 (0.041)	0.870	0.521 (0.501)
Log (land size in hectares)	−0.025 (0.037)	0.493	−0.010 (0.035)	0.819	0.546 (0.554)
Log (*per capita* income in Birr)	−0.033 (0.025)	0.196	−0.022 (0.029)	0.454	4.462 (0.770)
Membership in farmer association	−0.026 (0.046)	0.570	−0.041 (0.045)	0.336	0.287 (0.455)
Membership in religious group	0.065 (0.039)	0.093	0.035 (0.036)	0.232	0.617 (0.489)
Frequency of church attendance (weekly)	−0.007 (0.009)	0.494	−0.021 (0.010)	0.045	3.241 (1.629)
Constant	0.287 (0.236)	0.225	0.071 (0.261)	0.786	
Ln(Sigma)	−1.883	0.000	−1.879	0.000	
#Observations	91		91		

Note: Dependent variable is the interval of the lower and upper bounds of the degree of positional concerns. Health status is obtained from a question with the following range of answers: 1 = very good; 2 = good; 3 = neither good nor poor; 4 = poor; and 5 = very poor. The variable used here is a dummy: health status = 1, if subject answers 1, 2, or 3. Robust standard errors (White-Huber) in parentheses.

estimated parameters and then calculated the mean level of predicted marginal degree of positional concerns for the sample, which is conditional on the socio-demographic and economic characteristics of the subjects. This procedure was repeated using 1,000 bootstrap samples. The mean degree of positionality was 0.188 for income *per se,* with a 95% bias-corrected bootstrap confidence interval of (0.021, 0.354). The mean level of predicted marginal degree of positional concerns for income from the aid package was 0.268, with a 95% confidence interval of (0.086, 0.451). The hypothesis that the mean marginal degree of positional concern equals zero can be rejected for both goods (p-value = 0.027 and p-value = 0.004, respectively).

To our knowledge, only a handful of published experiments have aimed at analyzing positional concerns of individuals, and most of them have been conducted in relatively richer regions. For instance, Carlsson, Johansson-Stenman *et al.* (2007) estimated a mean degree of positional concern for income in the range of 0.59–0.71, using a random sample of the Swedish population, while Alpizar *et al.* (2005) estimated the degree to be 0.45 using a sample of Costa Rican university students. The study that we consider closest to our study is Carlsson, Nam, Linde-Rahr, and Martinsson (2007), which used poor Vietnamese farmers and found a median degree of positionality of 0.25 (mean degree of positionality is 0.28). Overall, our results from Northern Ethiopia show a lower degree of positional concerns compared to previously conducted survey experiments.

Even if the core elements are the same, our experimental design differs slightly in wordings between the income *per se* and income from aid-packages experiments. The latter case may influence the decision by a subject differently since the decision for aid-packages may also affect the income of other people in the village (while there is no room for such strategic behavior in the income *per se* experiment). Thus, the preferred aid-package by a subject may also imply considerations of other motives such as other-regarding preferences to other villagers (see overview in, e.g., Fehr & Schmidt, 2006, chap. 8 and for laboratory experiment by for example, Charness & Grosskopf, 2000) and self image (Benabou & Tirole, 2006). The implication for our study is then that the aid-package positionality estimates might then be confounded with other-regarding preferences and self image. Thus the estimates may then reflect the lower bounds of the actual positionality figures. However, our design does not allow us to disentangle those effects. Since the difference in the degree of positionality between income

and aid projects experiments is small, we do not expect the impact of other factors besides positionality to be large.[19]

4. DISCUSSIONS AND CONCLUSION

This paper investigates whether extremely poor individuals have positional concerns in the dimensions of income *per se* and income from an aid package. We used a survey experiment approach where individuals chose between two villages to live in (two aid packages to choose from) and where their own as well as others' income differed between the villages (between the aid packages). In order to test the impact of the positional concerns among the very poor people, our tailor-made experimental design allowed us to calculate the marginal degree of positional concern. We found that the estimated mean degree of positional concern is very low, approximately 0.2, compared to what is found in the literature. Overall, our results are consistent with the idea discussed by Clark *et al.* (2008) as well as in Frey and Stutzer (2002), that is, that individuals in poorer countries are less concerned with positionality. Ethiopia is an ethnically fragmented country with different ethnic groups speaking different languages and living in different parts of the country. Although this suggests that our results might be region specific, they are in fact very similar to what was found in a happiness study in a neighboring region settled by another ethnic group (Akay & Martinsson, 2011).

An important issue from a policy perspective is the extent of positional concerns for income *per se* and especially for income from implemented aid packages. Our results imply that others' income has only a small impact on the utility of individuals who live in the same village, and approximately two-thirds of our subjects show almost no positional concern. Given the design of the experiment, we cannot rule out that a fraction of the subjects actually have negative positional concerns, which can be explained by high degree of altruism among village members or the "tunnel-effect" analogy of Hirschman and Rothschild (1973). Aid-based development projects often select a smaller fraction of households in a village to be "model farmers" for a new farm technology or improved input, while in other cases aid projects may target certain households in a village according to some criteria such as income, gender, or family size. In times of development, targeted or differentiated aid packages seem to be justified from a welfare perspective. Our results indicate that there does not seem to be an immediate need to consider positional concerns in the implementation of aid projects. However, in the long-run when absolute income has increased above a threshold, there might be such a need.

Our findings are in line with the hypothesis that there are lower positionality concerns in poor countries than in rich countries. However, a more interesting question is what lies behind such a divide. One way to answer this is to look for local factors that may explain the level of positionality in a specific area and to establish a link between these factors and income level. There are several potential counterworking explanations for the low degree of positional concern in our experiment. Although the people in the investigated region do not live in a particularly communal way, the role of kinship relations is very important in these rural regions compared to urban areas in Western countries. The existence of informal networks (such as a labor exchange network like Debo) and insurance mechanisms (such as rotating savings and credit associations like Iquib) is also important. Moreover, our subject pool consists of farmers who are dependent on foreign aid for daily food during most of the year. As a result of the above mentioned factors, individuals may attach a higher value to the overall welfare of their village. These factors indicate that one way to understand the rich–poor difference in positionality concerns is to look into differences in the organization of production, traditions, local (informal) institutions, and the relationship between individuals. For example, it is conceivable that positionality is lower in places where informal risk-sharing schemes are the main source of insurance against income shocks. This paper is a first attempt to explore positional concerns of the very poor in terms of income *per se* and development aid projects. In order to fully understand welfare implications of positional concerns, researchers should set out to incorporate socio-cultural factors such as informal networks and also try to estimate the income threshold after which the positional concerns kicks in.

NOTES

1. A survey experiment differs from both laboratory and field experiments. A survey experiment is not monetary incentivized whereas in a laboratory experiment individuals are paid according to their choices. Field experiments have the feature of being conducted in people's normal life without them knowing they are part of an experiment.

2. For laboratory experiments on positionality see, e.g., McBride (2010).

3. In the original work by Jeremy Bentham, the hedonic experience of an event was measured in utilities determined by its pleasure and pain, and this corresponds to experienced utility. It is, however, outside the scope of the present paper to discuss whether decision or experience utility should be used by for example, policy makers (see, e.g., the discussion in Kahneman and Sugden, 2005).

4. In 2002, Ethiopia's purchasing power parity per capita was 2.05% of that of the United States. Ethiopia ranked the seventh poorest country in the world and one of the top recipients of foreign aid (World Bank, 2004).

5. Abraha We Atsbaha has a *kebele* status, the lowest administrative level in Ethiopia, and was randomly selected from a list of kebeles in the region. The study was conducted in March 2008.

6. In most cases, aid projects target a specific sub-set of people either because of limited resources or due to prioritizing certain groups of people, or both. This is also consistent with recent discussion by major donors namely to increase the effectiveness of aid by directing it to those who need it most or where it is most productive (e.g., Dollar & Pritchett, 1998; Nunnenkamp & Thiele, 2006). At macro level, a number of donor countries have narrowed their focus to fewer countries, and specific areas. At household level, there are two major reasons for the uneven distribution of aid packages. The first relates to targeting of specific groups and areas based on certain criterion. For example, the Productive Safety Net Program in Ethiopia (PSNP), the largest social protection program in sub-Saharan countries outside South Africa, focuses on very poor households within poor districts (e.g., see Gilligan, Hoddinott, & Taffesse, 2009). The second main reason relates to the practice of focusing on model farms in the promotion of new agricultural techniques. In

Ethiopia, this approach is used by government and nongovernment projects. For example, the International Livestock Research Institute's IPMS project, funded by the Canadian International Development Agency (CIDA), focuses on farm extension services to selected farmers in selected districts in northern Ethiopia. Even if aid initiatives are usually short-lived, the income differences they create could persist, or even increase, in the long-run since households with higher income are better equipped to cope with shocks, get access to credit, and undertake additional investments.

7. The definition and aggregation of "welfare" is a highly debated issue in economics. We would like to note that in the present paper, we use happiness as a proxy for the latent utility of individuals, following, for example, Clark and Oswald (1996) and Fleurbaey (2009).

8. The official exchange rate was US$ 1 = ETB 9.67 at the time of the survey.

9. We used the phonetic translations of "A" and "B" for identification of the choices, which were two random characters in the Ge'ez script without any clear connection to an alphabetic order as well as having a day-to-day use (unlike *A* and *B*). Thus, we believe that these features have reduced potential order effect that might have arisen if the first two characters in Ge'ez have been used in the experiment, since they play similar roles as *A* and *B* in the English language.

10. The implicit marginal degree of positional concern with the ratio comparison utility function took the values 0.110, 0.224, 0.345, 0.471, 0.605, and 0.746 ($x_A/\bar{x}_A^{\gamma} = x_B/\bar{x}_B^{\gamma}$), and the marginal degree of positional concern was calculated with $\gamma = \ln(x_A/x_B)/\ln(\bar{x}_A^{\gamma}/\bar{x}_B^{\gamma})$.

11. Note that each subject faced a maximum of six choice situations in each experiment. It is possible that a subject ended with fewer choice situations depending on when he/she switched to *A*.

12. The subjects were randomly selected from a total of 584 households in the village of Abraha We Atsbaha. The selection was based on a random draw from a village household roster acquired from the *kebele* administration. In order to make sure that our sample is representative, we conducted the experiment on a day where there was no work in a nearby road construction project.

13. After finishing our survey experiment, we conducted a risk and a bargaining experiment with opportunities to earn money. No one knew what kind of experiments we would conduct until they had finished with the positionality experiment. The whole process took about 6 hours. The average earning was about ETB 18 and no subject went home with less than ETB 15. During the time of our experiment, the daily salary in the region was approximately 15–20 ETB.

14. To calculate the mean, we use the mid-value of the marginal degree of positionality for the nonextreme responding subjects, while for the extreme responses we set the value to 0 or 1.

15. In the interval regression, we set the lower and upper bounds, as shown in Table 2. In case of extreme choices, i.e., where *A* was chosen in the first choice set or *A* was never chosen, we set the lower bound to 0 in the former case and the upper bound to 1 in the latter case.

16. We also estimated a probit regression to test the sensitivity of our results to the model specification by defining the dependent variable as 0 when an individual chose society *A* in the first choice situation, i.e., the individual is not positional at all, and 1 otherwise, i.e., making a choice indicating some degree of positionality. The results are pretty similar in terms of the signs of the parameters and significance compared to the interval regression presented. The results are presented in Appendix B (Table B1).

17. The results given in Tables 2 and 3 are robust to specification of a ratio utility function. The results are available upon request.

18. The village, which was named after its centuries-old rock-hewn churches, is almost entirely Orthodox Christian.

19. We are thankful to an anoymous referee for making this point.

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APPENDIX A

Box. 1.Instructions for income experiment

Imagine that you can choose to live one of two villages. Your yearly income and the average yearly income of the others in the villages differ between the two villages.

I will tell you the amount of income for you and the average income for others in each village. I will ask you to choose which village you would like to live in. Let me illustrate this choice by the following example.

Village A:

Your yearly income is 2,530 Birr.

The yearly average village income is 3,300 Birr.

Village B:

Your yearly income is 2,310 Birr.

The yearly average village income is 2,200 Birr.

In this example, your yearly income is 220 Birr more in Village A than in Village B. In Village A, you earn 770 Birr less than average income in the village, while in Village B you get 110 Birr more. Given this difference, you can either choose to live in Village A or B. (*Repeat question and example*)

Now, I'll ask you to make your choice between different villages.

(*Using the table below, ask the first question in the following way*)

In Village A, your yearly income is 2,990 Birr, while the average yearly income in the village is 3,900 Birr. In Village B_1, your yearly income is 2,860 Birr, while the average yearly income in the village is 2,600 Birr. In which village, A or B_1, do you want to live?

(*If the respondent chooses A, stop. If respondent chooses B1, ask her/him to choose between A and B2. Do not change the format of the question except for the numbers. Follow the same procedure for the other choices.*)

Alternatives	Your yearly income (Birr)	Yearly average income in the village (Birr)
A	2,990	3,900
B_1	2,860	2,600
A	2,990	3,900
B_2	2,730	2,600
A	2,990	3,900
B_3	2,600	2,600
A	2,990	3,900
B_4	2,470	2,600
A	2,990	3,900
B_5	2,340	2,600
A	2,990	3,900
B_6	2,210	2,600

Box 2.

Instructions for aid-package experiment.

Assume that you are introduced to two different bee-keeping aid packages which will make you earn money. These packages are distributed for free by the government. You can choose one of the packages you want for your village and *there will be no cost to you or to the village.*

Your income and the income of other people in your village changes with the package you prefer from the two alternatives.

I will tell you the amount of income for you and for others in the village in each alternative, and then I will ask you to choose one of the two. Let me illustrate this choice by the following example.

Alternative A:

Your yearly income from the package is 575 Birr.

The yearly average village income from the package is 750 Birr.

Alternative B:

Your yearly income from the package is 550 Birr.

The yearly average village income from the package is 500 Birr.

In this example, you get the 25 Birr more in Alternative A than in Alternative B. In alternative A, you get 175 less than average income in the village, while in Alternative B you get 50 Birr more. Given this difference, you can either choose A or B for your village. (*Repeat question and example*)

Now, I'll ask you to make your choice between different alternatives.

(Using the table below, ask the first question in the following way)

In package A, your yearly income is 690 Birr, while the average yearly income in your village is 900 Birr. In package B_1, your yearly income is 660 Birr, while the average yearly income in your village is 600 Birr. Which alternative package, A or B_1, do you want for you village?

(*If the respondent chooses A, stop. If respondent chooses B1, ask her/him to choose between A and B2. Do not change the format of the question except for the numbers. Follow the same procedure for the other choices.*)

Alternatives	Your yearly income from the package (Birr)	Yearly average income for your village from the package (Birr)
A	690	900
B_1	660	600
A	690	900
B_2	630	600
A	690	900
B_3	600	600
A	690	900
B_4	570	600
A	690	900
B_5	540	600
A	690	900
B6	510	600

APPENDIX B

Table B1. *Probit regression of the degree of positionality*

	Total income		Income from aid projects	
	Coefficient (std. err.)	*p*-value	Coefficient (std. err.)	*p*-value
Age/100	−1.209 (7.842)	0.877	6.027 (7.517)	0.423
Age-squared/10,000	0.419 (7.933)	0.958	−5.487 (7.657)	0.474
Married	0.463 (0.448)	0.301	0.754 (0.450)	0.094
Female	−0.442 (0.371)	0.234	−0.354 (0.370)	0.339
Good health-status	−0.399 (0.349)	0.253	−0.202 (0.351)	0.565
Number of adults in home	−0.185 (0.136)	0.174	−0.065 (0.132)	0.622
At least 1 year of education	−0.390 (0.374)	0.298	0.053 (0.369)	0.885
Log (land size in hectares)	−0.050 (0.304)	0.870	0.157 (0.295)	0.596
Log (*per capita* income in Birr)	−0.186 (0.197)	0.345	0.047 (0.189)	0.801
Membership in farmer association	0.261 (0.392)	0.505	−0.401 (0.399)	0.316
Membership in religious group	0.616 (0.343)	0.073	0.324 (0.323)	0.317
Frequency of church attendance (weekly)	−0.082 (0.107)	0.442	−0.164 (0.105)	0.120
Constant	1.495 (2.166)	0.490	−2.027 (2.171)	0.351
Pseudo R^2	0.101		0.093	
#Observations	91		91	

Note: The dependent variable is 0 if an individual chooses society A in the first choice situation and 1 otherwise. Health status is obtained from a question with this range of answers: 1 = very good; 2 = good; 3 = neither good nor poor; 4 = poor; and 5 = very poor. The variable used here is a dummy: health status = 1, if subject answer is 1, 2 or 3. Robust standard errors (White-Huber) are in parentheses.

Paper IV

Positional Concerns among the Poor: Does Reference Group Matter?

-Evidence from Survey Experiments*

Alpaslan Akay[1,2] Lisa Andersson[1]
Peter Martinsson[1] Haileselassie Medhin[1]
[1]University of Gothenburg [2]IZA

Abstract

In general, previous research on positional concerns suggests a lower degree of positional concerns among people from poor countries. Yet the evidence is limited and most often builds on the assumption that people's reference groups are given, (often referring to other people in the society) and are the same across all individuals. In this paper, we test if low positional concerns found in the literature may be due to misspecification of the reference groups. We contribute to the limited literature by estimating the positional concerns in a low-income country considering various reference groups. We do so by testing the effect of different reference groups on the positional concerns of a representative sample of individuals in urban Ethiopia. We use a tailored survey experiment that is modified to include multiplicity of reference groups. The results show a low degree of positional concern for income, and that the degree of positional concern is highly stable across different reference groups.

Keywords: *Reference groups, income comparison, experiment, subjective well-being*
JEL Classification: D60, C90

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1 Introduction

Concerns about positionality (or status) have been widely discussed by many scholars, including Adam Smith and Karl Marx and later, e.g., Veblen (1899), Duesenberry (1949), and Hirsch (1976). In the last couple of decades, positional concerns for income or consumption have been hot topics in economics (Clark and Oswald, 1996; Frank, 1999; Akay and Martinsson, 2011). Positional concern implies that individuals compare their income or consumption level with "relevant other" individuals or groups of people. In other words, the utility that people derive from income or a good does not only depend on the absolute amount of income or goods consumed, but also on the amount of income or goods consumed relative to the amount of income earned or goods consumed by others. There is a growing empirical literature investigating positionality concerns in the context of optimal taxation (e.g., Boskin and Sheshinski, 1978.; Ljungqvist and Uhlig, 2000; Alpizar et al., 2005; Aronsson and Johansson-Stenman, 2008), labor supply (e.g., Neumark and Postlewaite, 1998; Woittiez and Kapteyn, 1998; Park, 2010), saving and investment (e.g., Abel, 1990; 2005), and migration (Knight and Gunatilaka, 2010; Akay et al., 2012b), to mention a few.

The impact of positional concern on individual utility has been studied using both survey experiments (e.g., Solnick and Hemenway, 1998; 2005; 2007; Johansson-Stenman et al., 2002; Alpizar et al., 2005; Carlsson et al., 2007a; Akay et al., 2012a) and subjective well-being data (e.g., Clark and Oswald, 1996; McBride, 2001; Ferrer-i-Carbonell, 2005; Luttmer, 2005; Clark et al., 2008). The general conclusion from both approaches is that the utility is significantly and negatively affected by the income of others in rich developed Western countries. A limited literature on positional concerns in low-income countries presents more mixed results: a positive positional concern is reported by some studies reflecting tight community ties and altruistic preferences among the poor, while other studies find that the income of others does not significantly affect the utilities of the poor (e.g., Kingdon and Knight, 2007; Carlsson et al., 2007b; Bookwalter and Dalenberg, 2009, Knight and Gunatilaka, 2010; Ravallion and Lokshin, 2010; Akay and Martinsson, 2011; Akay et al., 2012a).

One of the important issues in the studies of positionality is the choice of relevant others, or "reference group," with whom individuals make comparisons. The term "reference group" was first explored in studies in social psychology. Runciman (1966) emphasizes the role and importance of choice of reference group for estimates

of positional concerns. He recognizes that an individual can have multiple reference groups depending on the topic and context. However, the general approach used in the economic literature is to make *a priori* judgment of the composition of reference groups based on characteristics such as geographical proximity, age, education, race, and/or gender, without taking into account that all individuals do not necessarily share the same reference group, and that people could have several simultaneous reference groups that affect their utilities in different ways. Moreover, in the context of low-income countries, the reference groups may also have more complex structures since the members of the community might rely on informal insurance systems in the absence of more formal insurance mechanisms. There is vast evidence showing that people in developing countries often form informal insurance and risk-sharing networks based on close geographic proximity and kinship (e.g., De Weerdt and Dercon, 2006; Fafchamps and Gubert, 2007). Thus, the lower degree of positionality often found in the literature may simply be an artifact of the construction of reference groups similar to those used for rich developed countries. The objectives of this study are twofold. First, we investigate the positional concerns of the poor using survey experiments to bring new evidence to the literature. Second, we address the issue of multiple or simultaneous reference groups among the poor by relaxing the assumption that everyone compares their own income with only one single reference group. We do this by exploring positional concerns relative to an array of possible reference groups defined using different comparison orbits of social proximity.

The experiment was conducted among 260 randomly selected residents of Addis Ababa, the capital city of Ethiopia. The experimental nature of our study allows us to specify different reference groups that are believed to represent key social groupings presumed to exist in every society, and investigate how positional concerns differ across reference groups among the poor. We control for six reference groups – *friends, neighbors, relatives, colleagues, people of the same age, and all other people in the city*. These groups are defined based on different physical and social comparision orbits that we believe the respondents are likely to have interaction and common attributes with, and that have been proposed and used as relevant points of reference in other studies (e.g., Carlsson et al., 2009; Knight et al., 2009; Clark and Senik, 2010a; Carlsson and Qin, 2010).

The results obtained in our analysis can be summarized as follows: We find very

low positional concerns compared to estimates from developed countries, confirming previous results from rural Ethiopia in Akay and Martinsson (2011) and Akay et al. (2012a). There is some heterogeneity in positional concerns across different reference groups, but again, even the highest marginal degree of positionality is much lower than the average from developed countries. In our econometric analysis, which controls for various individual socio-demographic and economic characteristics, we find that the positional concerns *vis-à-vis friends, neighbors, relatives, colleagues* and *all other people in the city* are not statistically significantly different than zero though there is some variation. The positional concerns are somehow higher and statistically significant when people compare their income with *people of the same age*. We also report that the positional concerns are heterogeneous across some socio-demographic and economic characteristics of individuals. Marital status and education seem to be the most important socio-economic determinants of positional concerns.

The remaining part of the paper is organized as follows: The next section discusses previous literature on positional concern and the issue of reference group. Section 3 gives the experimental design. Section 4 presents the results using interval regressions. We also estimate the mean degree of positionality using bootstraping conditional on the socio-dempographic characteristics of the individuals. Section 5 discusses the implications of the results and concludes the paper.

2 Positional Concerns and Reference Groups: What do We Know?

2.1 Methods and literature

Empirical investigation of positionality in the literature draws on two distinct approaches. The first approach is based on survey experiments to directly identify the degree to which individuals care about absolute and relative income or consumption by asking individuals to choose between different societies in which they prefer to live, where the societies differ in the individual's own and others' average level of income. The overall results from these survey experiments show that people do have positional concerns both for income and for consumption of specific goods, but that the degrees vary by goods and location (see Solnick and Hemenway, 1998; 2005; 2007; Johansson-

Stenman et al., 2002; Alpizar et al., 2005; Carlsson et al., 2007a; 2007b; 2009; 2010; Akay et al., 2012a for experimental findings).[1] A second, parallel, approach is based on self-reported subjective well-being data, collected through "happiness" or "life satisfaction" questions in surveys. The impact of positionality on subjective well-being is then investigated using relative income, which is defined as the mean (or median) income level of the reference group. The general welfare implication obtained from studies conducted in rich Western countries is that people care about other people's income, and that subjective well-being is negatively affected by the income of others (Clark and Oswald, 1996; McBride, 2001; Senik, 2005; Ferrer-i-Carbonell, 2005).

However, the literature examining positional concern in transition and developing economies is limited and the results are more mixed (see Clark and Senik, 2010b for a comprehensive review). Akay et al. (2012a) conduct a survey experiment – similar to the one in this paper – among very poor rural Ethiopian farmers. They find very low positionality for income in general and for the income obtained from an aid project. Using a similar survey experiment, Carlsson et al. (2007b) find low degree of positionality among farmers in rural Vietnam, while a higher degree of positionality is found by Carlsson and Qin (2010) among farmers in rural China. Results from studies using the subjective well-being approach in low-income countries are in line with those found using survey experiments. Ravallion and Lokshin (2010) investigate relative income effects in Malawi and find that relative comparison does not seem to matter for most of the sample, but for the relatively well-off (including those living in urban areas) subjective well-being does seem to fall with average neighborhood income. A similar result is found by Akay and Martinsson (2011) for rural farmers in Ethiopia. They use subjective well-being data and various alternative ad hoc reference groups and show that the mean income level of the reference groups does not significantly affect the well-being of poor rural farmers in Ethiopia. In contrast, Fafchamps and Shilpi (2008) use data from Nepal to test whether poor and more isolated households care less about relative consumption, and find that relative consumption negatively affects subjective well-being even at low absolute or relative levels of consumption.

Some evidence obtained from the subjective well-being approach contrasts the finding from developed countries and shows positive effects of income comparisons

[1] Positionality has also been investigated in controlled laboratory experiments (e.g., Clark et al., 2010; McBride, 2010).

in developing and transition economies. Kingdon and Knight (2007) find neighbors to be positive rather than negative comparators, and that subjective well-being rises with average income in the immediate neighborhood in South Africa. This result is confirmed by another study from South Africa by Bookwalter and Dalenberg (2009), who find that at low levels of income and expenditures the benefit of living among wealthier people outweighs the negative effect of being the poorest in a peer group. The positive effects of higher income of others found in some studies are in line with the "tunnel effect" conjectured by Hirschman (1973). An increase in the income of the reference group is interpreted as an encouraging prospect of future income gains. In poorer contexts, risk-insurance mechanisms, altruistic preferences, and fellow feelings in the community have been suggested as the main explanations of the positive relative income effect (Kingdon and Knight, 2007).

2.2 What is really a reference group?

A crucial aspect in the studies of postional concerns is the specification of a reference group. The term "reference group" was first used by Hyman, though the idea behind the concept can be traced much further back in time in the literature and tradition of thoughts in social psychology (Hyman, 1942; 1960). Hyman highlights the difficulties of pre-judging the reference group that people use as their social framework for comparison, and argues in favor of empirically determining the reference group that people are likely to employ (Hyman, 1960, p.390). It is suggested in the literature that people make active choices when it comes to reference groups to serve self-relevant goals such as *self-enhancement* and *self-improvement*. Self-enhancement refers to a strategy of downward comparison where the indiviudal compares himself with people who are less fortunate in order to feel better about their own situation, while self-improvement refers to upward comparison where people compare themselves with indiviudals who perform better or are more fortunate in order to enhance one's own motivation and performance (see Falk and Knell, 2004 for a more detailed discussion). Despite the mounting evidence on the importance of positional concerns in economic decisions, most economic studies, whether they use a survey experiment or a subjective well-being method, suffer from a lack of information about the relevant reference groups and how these reference groups are formed. The reference group is almost always assumed to be exogenously given, and most often assumed to be the same

across all individuals. The common approach in subjective well-being studies is to include one single reference group, refined using various socio-demographic characteristics (e.g., the same age cohort as in McBride, 2001; the same geographical area as in Blanchflower and Oswald, 2004 and Luttmer, 2005; the same region, education level, and age as in Ferrer-i-Carbonell, 2005). Among other things, such an approach could pose a challenge in the interpretation and use of positionality estimates if the specified reference group is not the relevant comparator. People could also have multiple reference groups simultaneously, and hence exhibit different levels of positional concerns vis-à-vis different reference groups. The issue of multiple reference standards therefore poses a serious challenge to the empirical investigation of positional concerns if survey and experimental instruments fail to fully capture an individual's reference group spectrum.

We are only aware of four studies (Carlsson et al., 2009; Knight et al. 2009; Clark and Senik, 2010a; Carlsson and Qin, 2010) that investigate potential reference groups by explicitly asking people with whom they compare themselves. Clark and Senik (2010a) investigate the degree of income comparison using the third wave of the European Social Survey covering 18 European countries. The survey asks people who they are most likely to compare their income with. Of those who identified a reference group[2], 36% stated that they are most likely to compare their income with colleagues, 15% with friends, 6% with family members, and 7% with others. The choice of reference group was shown to be closely related to regular social interactions. Knight et al. (2009) use data from rural China where the respondents were directly asked who they compare themselves with. The most common comparator group was people in the village (40%) followed by neighbors (29%), while 7% compare themselves to relatives. Only 11% had a reference group outside the village (i.e., people in the township, county, city, or elsewhere in the country). When asking respondents in their experiment in rural China about their reference groups for income comparisons, Carlsson and Qin (2010) found small differences across the suggested reference groups, yet found neighbors, people in the village, and off-farm migrants in the city to be the most likely comparison groups, and people in the township or city to be the least likely comparison groups. Carlsson et al. (2009) investigate and quantify the degree of positionality within and between castes in India using a sample of university students.

[2]About one third of the respondents, 36%, stated that they do not compare their income.

Their results show that the negative effect on an individual's utility from an average increase in income in her own caste is larger than the positive effect on utility from an increased income of her own caste compared to the income of other castes.

A few studies also look at a set of different reference groups in order to assess the relative impact of different types of comparisons. Senik (2009) investigates the relative importance of internal and external comparison on well-being in all countries in the former socialist bloc, and finds internal comparison to one's own past living standard to outweigh all external comparison groups (parents, former colleagues, and high school friends). External comparison is however found to be more important than individuals' self-ranking in the social ladder. No clear-cut results are found with respect to the relative importance across external comparison groups, but former colleagues and schoolmates seem to play an equally important role, outweighing comparisons with one's parents. Kuegler (2009) investigates the effect of relative income against various reference groups (siblings, friends, own past income, and parents' living standards in the past) using perceived relative income from Venezuela. Siblings turn out to be negative comparators, while no statistically significant results are found for any of the other reference groups. Kingdon and Knight (2007) test two different reference groups based on spatial proximity (neighbors) and social proximity (same race), and find that neighbors are positive comparators while a higher income in a reference group consisting of people of the same race has a negative effect on subjective well-being. Akay et al. (2012b) find that the well-being of Chinese rural-to-urban migrants depends on several reference groups and that well-being is positively affected by the income of urban workers but negatively affected by the income of other migrants and workers from the home region. Taken together, the results from these studies suggest in different ways that the choice of reference group matters for the direction and magnitude of relative comparison, which in turn underlines the importance of better understanding of how reference groups are formed.

3 Experimental Design

3.1 Setup

To test for positional concern across different reference groups, we constructed six versions of the survey experiment where individuals' own income was compared to

the income of friends, neighbors, relatives, colleagues, people of similar age, and all other people in the city. For each reference group, subjects were presented with a scenario describing two states of the world, referred to as societies, which only differ in the monthly income of the subject and the average monthly income of the people in the reference group in question. Subjects were then asked to choose in which of the two societies they would prefer to live. The income was expressed in the local currency Ethiopian birr (ETB) and the official exchange rate was US$ 1 = ETB 16.80 at the time of the survey (see Appendix A for the details of the instructions).

3.2 Preferences: modeling positional concerns

There are various ways to empirically specify the utility function to allow for positional concerns. The most common specifications are (i) the ratio comparison utility function, $U = v(x, x/\bar{x})$, where x is the individual's income and $\bar{x}$ is the average income in the reference group (e.g., Boskin and Sheshinski, 1978; Layard, 1980; Persson, 1995) and (ii) the additive comparison utility function, $U = v(x, x - \bar{x})$ (e.g., Akerlof 1997; Knell 1999; Ljungqvist and Uhlig, 2000). In this paper we apply the following additive comparison utility function:

$$\begin{aligned} v &= (1-\gamma)x + \gamma(x - \bar{x}), \\ 0 &\leq \gamma \leq 1, \end{aligned}$$

where γ measures the *marginal degree of positional concern*, i.e., the proportion of the total change in utility related to an increase in relative income when an individual's own income is marginally increased.

3.3 The marginal degree of positional concern

To elicit the degree of positionality, or more correctly the positionality interval, for each individual, respondents are asked to make pair-wise choices between societies that differ in own and others' income levels for all six reference groups. The income levels in each choice set for each reference group are systematically constructed to measure the degree of positionality. Starting from a choice with the lowest degree of positionality, individuals are presented with up to six successive choices until the

respondent switches to the choice where she cares more about the absolute income than the relative income.

(Box 1 about here)

An example scenario used in the experiment is presented in Box 1. In the beginning, the individual chooses between a *Society A* where her monthly income is lower than the average monthly income of the reference group, and a *Society* B_1 where her monthly income is higher than the average monthly income of the reference group but lower than her income in *Society A*. If the individual chooses *A*, the experiment for the specific reference group stops since the individual has revealed her actual interval of positionality, i.e., lower than the implied degree of positionality. If the individual chooses B_1, she is asked to choose between *Society A* and *Society B*2, where her income is further lower than in B_1, but still higher than the income level of the reference group, which is the same as in B_1. For example, for the experiment with neighbors as a reference groups (see Box 1), the individual has an income of 800 Birr per month in *Society A* while the average income of her neighbors is 900 Birr. On the other hand, her income is 770 Birr in *Society* B_1 and that of her neighbors is 600 Birr. Her income decreases by 30 Birr in *Society* B_2 while the average income of her neighbors stays at 600 Birr. The 30 birr decreases continue until B_6, where the individual's monthly income drops to 620 Birr. Since the choice is always against *Society A*, the degree of positional concern increases as we go from Society B_i to Society B_{i+1}. The session ends if the individual chooses *Society A* or has reached the last choice set (B_6).

When the subject is indifferent between *Society A* and *Society* B_i, then we know that $x_{i,A} - \gamma \bar{x}_A^r = x_{i,B} - \gamma \bar{x}_B^r$. From this, we can then calculate the marginal degree of positional concern from the above example given in Box 1:

$$\gamma = \frac{x_A - x_B}{\bar{x}_A^r - \bar{x}_B^r} = \frac{800 - 770}{900 - 600} = 0.1.$$

When the subject chooses *Society A* (for this example), then it implies that the subject has a degree of positionality lower than 0.1 ($\gamma < 0.1$). We present repeated choices between the two societies. Using the stopping choice situation (when the subject chooses *Society A*), we calculate the degree of positional concern of each individual within an upper and lower bound.

The reference groups used are presented in a subsequent order for each respondent. People may learn or get alienated answering similar questions, or may want to appear consistent. Since the survey experiment contains six reference groups presented after each other, there is a possibility of order effect in their responses, which can be caused by learning, fatigue or wish to be consistent, or a combination of them. In order to limit biases that may arise from these effects, we randomized the order in which the reference groups were presented. It could be argued that the choice sets within a reference group should also be randomized, but we argue that this could create a very high cognitive burden and potentially also confusion for indiviuals, and hence we decided to refrain from this. Another design issue relates to which income levels to use in the choice sets. We thought that using the same income levels across reference gropus may induce individuals to try to be consistent. Thus, we decided to choose slightly different income levels, all just above subsistance level. Table 1 presents the full summary of the experiment. Note that even though the income levels are different in each choice situation, the implicit degree of postitionality is the same across reference groups, changing between 0.1 and 0.6.

(Table 1 about here)

The experiment was conducted among 260 individuals in Addis Ababa, Ethiopa. The mean per-capita daily income of the households in the sample is 3.79 PPP dollars. We employed five local interviewers, who received training prior to the experiment. We conducted a face-to-face interview with each subject in the local language (Amharic). To ensure consistency, the instructions were first translated to the local language and then translated back to English by two different individuals. The experiment was part of larger household survey. After the experiment had been conducted, the respondents participated in a migration and remittances survey that included a wide variety of socio-economic questions.

4 Results

As discussed in the previous section, the key measure in our empirical investigation is the marginal degree of positionality. We start by presenting a descriptive analysis of the unconditional mean marginal degree of positionality. We then estimate the mean

marginal degrees of positionality for different reference groups by using econometric models conditional on individual characteristics.

4.1 Descriptive analysis

By using the design features presented in Table 1, we can calculate the unconditional mean marginal degree of positionality. Table 2 summarizes the frequency distributions of marginal degree of positionality intervals across the six reference groups. As can be seen from the table, most people chose *Society A* in the first choice situation. Almost two-thirds of the subjects displayed a very low degree of positionality for each reference group. We can conclude from these results that regardless of which reference group we consider, the unconditional degree of positionality is very low in our sample, which is in line with the existing findings in the literature. There could however be heterogeneity across socio-demographic and economic characteristics of the individuals, which we investigate in more detail below.

(Table 2 about here)

To estimate the mean marginal degree of positionality, we assume that the actual value of the positionality for each individual lies in the middle of each positionality interval. Note that our design cannot identify the maximum or minimum positional concerns. We have to make some assumptions. The mid-value for the highest positional concern is assumed to be 0.8 by considering that the maximum positional concern is 1, and the mid-value of the lowest positional concern is assumed to be 0.05 by considering that the lowest positional concern is 0.[3] The mean marginal degrees of positionality are presented in the Table 3, together with the standard deviations and confidence intervals.

(Table 3 about here)

The mean marginal degrees of positionality estimates are found to be very small, as expected from the descriptive statistics given above. These results are highly in line

[3]We have also experimented with some other lower and upper limits. The result is basically the same.

with Akay and Martinsson (2011) and Akay et al. (2012a), who find very low positionality estimates in rural Ethiopia. We are mainly interested in the relative difference between the positionality parameters across reference groups. The lowest positionality estimate is obtained when subjects compare their income with their relatives, which could be due to strong family relationships and possible altruism between extended family members. The highest positionality is found vis-à-vis neighbors. We compare the experimental data pairwise using t-tests. We find significant differences in the positionality across reference groups. Test results for the mean difference suggest that the difference is statistically significant in the case of positionality experienced toward *neighbors* and *relatives* (*p-value=0.031*); *neighbors* and *same age people* (*p-value=0.099*); and *neighbors* and *all other people in Addis* (*p-value=0.027*).

4.2 Results by socio-demographic characteristics

Although positional concerns are generally low among the respondents in our sample, there may be some variations across different socio-demographic groups. We sort the subjects by their socio-demographic and economic characteristics and estimate the mean degree of positionality for each group. The results are reported in Table 4 by (i) male and female; (ii) employed/self-employed and all other subjects (students, unemployed, housewives, retired people etc.); (iii) married, divorced/widowed and single; (iv) low level of education (no formal education and incomplete primary school education), medium level of education (incomplete secondary education and secondary education), and high level of education (completed secondary education and studied at higher level, or degree at a level above secondary ecucation).

(Table 4 about here)

There are important relationships between the socio-demographic and economic characteristics of subjects and their attitudes toward positionality across reference groups. Females are slightly more positional except vis-à-vis colleagues. Employed are more positional vis-à-vis friends but less positional toward the other reference groups compared to unemployed subjects. There is a clear relationship between positional concerns and marital status – the positionality parameter is larger for married compared

to divorced/widowed and single subjects. The largest positionality parameter is obtained among married subjects vis-à-vis neighbors. The level of education is also found to be highy related with positional concerns.

We also investigate which factors explain the degree of positional concern for each reference group using regression analyses. Our dependent variable of interest is the marginal degree of positionality. The experimental setup gives us a dependent variable with a lower and an upper bound, and thus we use an interval regression specification. The lower and upper bounds of the intervals are specified as in the first column of Table 2. As before, we have to make some assumptions for the extreme choices. We assign 0 for the lower bound of the first interval and 1 for the upper bound of the last interval. In our regressions, we control for various exogenous variations: age, gender, marital status, occupation, household size, education, migrant status, household income, location in Addis Ababa, and ethnic groups (the locations in our sample are the sub cities Kirkos, Arada, Addis Ketama, Yeka, and Gullele; ethnic groups are Amhara, Oromo, Tigray, and Others). Table 5 reports interval regression estimates. The variation in the marginal degree of positionality is explained by several variables. For example, female subjects are more positonal *vis-à-vis neighbors*, and single subjects are less positional toward all reference groups except for *people of the same age*. These result are in line with the descriptive statsitics presented above.

(Table 5 about here)

4.3 Estimating conditional degree of positionality

One of our aims is to use estimated regression parameters presented above to estimate the mean degree of positional concern conditional on socio-demographic and economic characteristics of the subjects. To calculate the mean degree of positional concern as well as confidence intervals, we use the bootstrap technique (see, e.g., Efron and Tibshirani, 1998). We first predict the marginal degree of positionality for each individual using estimated model parameters and then calculate the mean level of predicted marginal degree of positional concerns for each bootstrap sample, which is conditional on the socio-demographic and economic characteristics of the subjects. This procedure is repeated for 1,000 bootstrap samples. Table 6 presents the conditional mean marginal rate of positionality for the overall sample and for the selected

socio-demographic groups. Results are presented for each of the reference groups separately. Again, it is clear from Table 6 that the positional concerns are very low. The fact that most estimates are insignificant indicates that, conditional on observed individual characteristics, positional concerns are basically zero. The only statistically significant mean marginal degree of positionality is obtained for the reference group *people of the same age*. Significant estimates toward this reference group are also found for four of the socio-demographic groups. However, the level of the positionality is much lower than that is found in developed countries.

(Table 6 about here)

We also control for the order effect with 12 different combinations of the experimental design. However, in order to check the sensitivity of the results we include dummies for the order categories in the interval regressions. We estimate the marginal degree of positionality using 1,000 bootstrap replications. The results are not reported here since they are virtually the same as the results presented in Table 6.[4]

5 Discussions and Conclusion

In this paper we have estimated the marginal degrees of positional concern of poor people in an urban setting using various reference groups explicitly introduced into a survey experiment. We conduct our experiment among 260 individuals living in urban Ethiopia by modifying existing survey experiments used in the literature. A detailed econometric analysis indicates that the poor do have low positional concerns, and that the low positional concerns are not an artifact of a misspecification of reference groups. There are differences across reference groups, yet the low positionality for income persists vis-à-vis all reference group definitions.

Our results suggest that the only significant estimate of the marginal degree of positionality is toward the reference group *people of the same age*. While the marginal degree of positionality is still low, the fact that the "same age" reference group stands

[4]We also estimated the mean marginal degree of positionality using Spearman-Karber, which is a nonparametric estimator. This estimator is robust to sample size. In this estimator the data is interpreted as a failure or duration time data. The results obtained from this experiment is highy in line with the results reported in Table 6.

out from the other reference groups could have interesting implications when it comes to the role of social proximity, informal mechanisms, and positional concerns. The insignificant estimates found for positional concerns toward the reference groups *relatives*, *friends*, *neighbors*, and *colleagues* may be explained by relationship attributes, e.g., altruism and informal support systems, that imply low positional concerns toward reference groups. There is no meaningful way age similarity could be used as a network formation mechanism, while it is reasonable to think that people compare their achievements with those of others of similar age, resulting in significant income comparison estimates. On the other hand, the reference group *all other people in the city* could be too intangible to the individual to make meaningful comparisons.

In this paper, we have systematically investigated multiple reference groups using a survey experiment approach. However, more work remains to be done to identify and explain the underlying relationships between reference groups and degree of positionality and how these relationships are shaped by the socio-economic proximity generated through informal mechanisms between individuals in low-income countries.

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Appendix A

Experiment instructions

Now I want to ask you some questions related to income.

Imagine that you can choose to live in one of two different societies, Society A and Society B. Your monthly income and the average monthly income of different groups of people differ between the two societies. Except for the income differences, other things like living expenses are exactly the same in the two societies.

For each society that we will consider, I will tell you the amount of your monthly income and the average monthly income of the group. Then I will ask you to choose which society you would like to live in.

Let me illustrate this choice by the following example. In this example, we will just name the group of people "other people."

Society	**Your own income Birr/Month**	**Average income of Other people Birr/Month**
Society A	**800**	**900**
Society B	**770**	**600**
Which society do you choose to live in?		

In this example, your yearly income is 30 birr more in Society A than in Society B. In Society A, you earn 100 birr less than the average income of other people in the society, while in Society B you get 170 birr more. Given these differences, you can either choose to live in Society A or B. *(Repeat question and example)*

Now, I'll ask you to make your choice between the different societies.

(For each table of a reference group, ask the first questions in the following way. Do not change the order the tables from what is given in this questionnaire!)

In Society A, your monthly income is _____ birr, while the average monthly income of _____ in the society is _____ birr. In Society B_1, your monthly

income is ____ birr, while the average monthly income of _____ in the society is _____ bbirr. In which Society, A or B_1, do you want to live?

(If the respondent chooses A, stop and proceed to the next table. If respondent chooses B_1, ask her/him to choose between Society A and Society B_2. If respondent chooses B_2, ask her/him to choose between Society A and B_3. Continue in a similar manner for the rest of the choices. Do not change the format of the question except for the numbers. Follow the same procedure for the other tables.
Remember! Do not change the order of the tables as it is given in this printout and always start from the first choice in each table!)

Others in the society		
Society	**Your own income Birr/Month**	**Average income of others Birr/Month**
A	**960**	**1080**
B1	**924**	**720**
Which society do you choose to live in? *(Circle choice. If the choice is A, stop and go to next next page, if the choice is B_1, proceed below)*		
A	**960**	**1080**
B2	**888**	**720**
Which society do you choose to live in? *(Circle choice. If the choice is A, stop and go to next next page, if the choice is B_2, proceed below)*		
A	**960**	**1080**
B3	**852**	**720**
Which society do you choose to live in? *(Circle choice. If the choice is A, stop and go to next next page, if the choice is B_3, proceed below)*		
A	**960**	**1080**
B4	**816**	**720**
Which society do you choose to live in? *(Circle choice. If the choice is A, stop and go to next next page, if the choice is B_4, proceed below)*		
A	**960**	**1080**
B5	**780**	**720**
Which society do you choose to live in? *(Circle choice. If the choice is A, stop and go to next next page, if the choice is B_5, proceed below)*		
A	**960**	**1080**
B6	**744**	**720**
Which society do you choose to live in? *(Circle choice.)*		

Box 1. Sample question.

Now I want to ask you some questions related to income.

Imagine that you can choose to live in one of two different societies, Society A and Society B. Your monthly income and the average monthly income of different groups of people differ between the two societies. Except for the income differences, other things like living expenses are exactly the same in the two societies.

For each society that we will consider, I will tell you the amount of your monthly income and the average monthly income of the group. Then I will ask you to choose which society you would like to live in.

Let me illustrate this choice by the following example. In this example, we will just name the group of people "other people."

Society	**Your own income Birr/Month**	**Average income of other people Birr/Month**
Society A	**800**	**900**
Society B	**770**	**600**
Which society do you choose to live in?		

In this example, your yearly income is 30 birr more in Society A than in Society B. In Society A, you earn 100 birr less than the average income of other people in the society, while in Society B you get 170 birr more. Given these differences, you can either choose to live in Society A or B. *(Repeat question and example)*

Now, I'll ask you to make your choice between the different societies.

(For each table of a reference group, ask the first questions in the following way. Do not change the order of the tables from what is given in this questionnaire!)

Tables

Table 1. Summary of the experiment (in ETB).

		Reference groups											
		Friends		*Neighbors*		*Relatives*		*Colleagues*		*Same age people*		*All other people*	
Implied degree of positionality if indifferent		Own income	Friends' income	Own income	Neighbors' income	Own income	Relatives income	Own income	Colleagues' income	Own income	Income of same age people	Own income	Income of all other people
Alternative A		640	720	800	900	760	855	880	990	680	765	824	927
Alternative B1	0.1	616	480	770	600	732	570	847	660	655	510	793	618
Alternative B2	0.2	592	480	740	600	703	570	814	660	629	510	762	618
Alternative B3	0.3	568	480	710	600	675	570	781	660	603	510	731	618
Alternative B4	0.4	544	480	680	600	646	570	748	660	578	510	700	618
Alternative B5	0.5	520	480	650	600	618	570	715	660	553	510	670	618
Alternative B6	0.6	496	480	620	600	589	570	682	660	527	510	640	618
#Subjects		260		260		259		260		260		259	

Table 2. Frequency distribution of marginal degree of positionality with alternative reference groups.

	Reference groups											
	Friends		*Neighbors*		*Relatives*		*Colleagues*		*People of same age*		*All other people*	
	Freq.	%	Freq.	%	Freq.	%	Freq.	%	Freq.	%	Freq.	%
$\gamma < 0.1$	194	74.33	181	69.35	201	77.01	202	77.39	199	76.25	205	78.54
$0.1 < \gamma < 0.2$	11	4.21	23	8.81	15	5.75	15	5.75	8	3.07	10	3.83
$0.2 < \gamma < 0.3$	18	6.9	14	5.36	12	4.6	12	4.6	19	7.28	12	4.6
$0.3 < \gamma < 0.4$	9	3.45	13	4.98	12	4.6	8	3.07	8	3.07	10	3.83
$0.4 < \gamma < 0.5$	9	3.45	5	1.92	5	1.92	2	0.77	9	3.45	5	1.92
$0.5 < \gamma < 0.6$	0	0	0	0	1	0.38	2	0.77	2	0.77	2	0.77
$\gamma > 0.6$	19	7.28	24	9.2	13	4.98	19	7.28	15	5.75	15	5.75
#Subjects	260		260		259		260		260		259	

Table 3. Unconditional mean marginal degree of positionality by reference groups.

	mean	standard deviation	95% confidence interval	
			lower	*upper*
Friends	0.151	0.221	0.124	0.178
Neighbors	0.166	0.238	0.137	0.195
Relatives	0.129	0.192	0.105	0.152
Colleagues	0.140	0.217	0.113	0.166
People of the same age	0.141	0.206	0.116	0.166
All other people in Addis	0.133	0.203	0.108	0.157
Overall	0.141	0.134	0.125	0.158

Table 4. Unconditional mean marginal degree of positionality by reference groups and socio-demographic characteristics of subjects.

reference groups	Male	Female	Employed/Self-employed	Unemployed /others	Married	Divorced /Widowed	Single	Low-Education	Medium-Education	High-Education
Friends	0.134	0.163	0.166	0.138	0.164	0.163	0.126	0.147	0.158	0.150
	(0.207)	(0.230)	(0.232)	(0.210)	(0.242)	(0.234)	(0.182)	(0.217)	(0.212)	(0.240)
Neighbors	0.160	0.170	0.160	0.172	0.203	0.174	0.120	0.151	0.169	0.191
	(0.235)	(0.240)	(0.235)	(0.240)	(0.282)	(0.231)	(0.177)	(0.224)	(0.233)	(0.268)
Relatives	0.111	0.141	0.110	0.145	0.156	0.146	0.085	0.147	0.122	0.103
	(0.156)	(0.213)	(0.163)	(0.213)	(0.223)	(0.198)	(0.137)	(0.205)	(0.193)	(0.160)
Colleagues	0.141	0.139	0.121	0.156	0.161	0.149	0.108	0.131	0.156	0.134
	(0.222)	(0.215)	(0.197)	(0.234)	(0.252)	(0.226)	(0.162)	(0.204)	(0.237)	(0.219)
People of the same age	0.104	0.167	0.123	0.157	0.132	0.155	0.138	0.137	0.168	0.113
	(0.158)	(0.231)	(0.179)	(0.226)	(0.207)	(0.217)	(0.196)	(0.192)	(0.238)	(0.184)
All other people in Addis	0.128	0.136	0.136	0.130	0.161	0.143	0.094	0.150	0.122	0.113
	(0.199)	(0.206)	(0.205)	(0.201)	(0.228)	(0.213)	(0.157)	(0.216)	(0.208)	(0.168)
Overall	0.130	0.150	0.132	0.150	0.159	0.155	0.112	0.144	0.149	0.126
	(0.124)	(0.141)	(0.117)	(0.148)	(0.140)	(0.150)	(0.108)	(0.141)	(0.135)	(0.120)
#Subjects	108	152	122	138	96	75	89	118	79	63

Table 5. Interval regression estimation results by alternative reference groups.

Dependent variable=intervals of marginal degree of positionality

	Reference groups					
	Friends	*Neighbors*	*Relatives*	*Colleagues*	*People of same age*	*All other people*
Age	-0.005	-0.002	0.001	-0.002	-0.006	-0.004
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)
Age-squared	0.00003	0.00001	-0.00003	0.00001	0.00001	0.00001
	(0.00004)	(0.00004)	(0.00003)	(0.00005)	(0.00004)	(0.00003)
Female(=1)	0.025	0.015	-0.009	-0.007	0.047 *	-0.011
	(0.030)	(0.029)	(0.024)	(0.027)	(0.028)	(0.025)
Married(=1)	-0.022	-0.0001	0.006	-0.007	-0.020	-0.005
	(0.032)	(0.030)	(0.032)	(0.032)	(0.027)	(0.025)
Single(=1)	-0.106 **	-0.102 ***	-0.089 **	-0.070 *	-0.062	-0.094 ***
	(0.047)	(0.038)	(0.035)	(0.043)	(0.041)	(0.031)
Paid worker(=1)	0.022	-0.041	-0.049 *	-0.023	-0.023	-0.002
	(0.030)	(0.028)	(0.026)	(0.027)	(0.028)	(0.028)
Self-employed(=1)	0.010	0.00001	-0.040	-0.038	-0.022	-0.021
	(0.031)	(0.042)	(0.029)	(0.032)	(0.028)	(0.027)
Secondary education	0.034	0.074 **	-0.008	0.062 *	0.053 *	-0.019
	(0.031)	(0.033)	(0.027)	(0.032)	(0.030)	(0.030)
High education	0.014	0.094 **	-0.012	0.038	0.000	-0.026
	(0.035)	(0.041)	(0.029)	(0.035)	(0.024)	(0.033)
Migrant to Addis	0.012	-0.029	-0.013	0.011	0.031	-0.027
	(0.023)	(0.024)	(0.022)	(0.025)	(0.023)	(0.024)
Log (household size)	-0.044 *	0.007	0.008	0.032	0.016	-0.005

	(0.023)		(0.020)		(0.016)		(0.022)		(0.020)		(0.018)	
Log (household income)	-0.015	*	-0.026	**	-0.007		-0.012		-0.013		0.001	
	(0.008)		(0.011)		(0.007)		(0.009)		(0.008)		(0.007)	
Other income (=1)	-0.015		-0.026		-0.002		0.012		-0.005		0.020	
	(0.022)		(0.027)		(0.022)		(0.026)		(0.023)		(0.021)	
Constant	0.415	***	0.412	***	0.274	*	0.284	**	0.319	**	0.283	**
	(0.155)		(0.155)		(0.143)		(0.138)		(0.142)		(0.134)	
Regions in Addis (a)	yes		yes		yes		yes		yes		yes	
Ethnic group in Addis (b)	yes		yes		yes		yes		yes		yes	
Prob>chi-squared	0.003		0.017		0.003		0.390		0.017		0.059	
Sigma	0.161	***	0.175	***	0.145	***	0.164	***	0.154	***	0.152	***
	(0.011)		(0.013)		(0.012)		(0.014)		(0.012)		(0.013)	
Pseudo-loglikelihood	-483.072		-499.356		-460.424		-488.155		-475.957		-471.689	
#obs	258		258		257		258		258		257	

Notes: The upper limit is assumed to be 1 and lower limit is assumed to be 0 in the interval regressions;
(a) there are 5 regions in our sample: Kirkos, Arada, Addis Ketama, Yeka, Gullele (Kirkos is excluded);
(b) there are 4 ethnic classifications: Amhara, Oromo, Tigray and Others (Amhara is excluded);
[*],[**], and [***] indicate significance at 10%, 5%, and 1% level.

Table 6. Marginal degree of positional concerns: 1,000 bootstrap estimates and confidence intervals with percentile method.

	Reference groups					
	Friends	*Neighbors*	*Relatives*	*Colleagues*	*People of same age*	*All other people*
All						
MDPC	0.073	0.003	0.056	0.070	**0.147** *	0.018
std.err.	(0.088)	(0.091)	(0.071)	(0.073)	**(0.081)**	(0.074)
PCI	(-0.026,0.322)	(-0.012,0.363)	(-0.011,0.263)	(-0.012,0.277)	**(-0.012,0.298)**	(-0.018,0.275)
Males						
MDPC	0.071	-0.005	0.122	0.141	0.107	0.031
std.err.	(0.111)	(0.124)	(0.088)	(0.116)	(0.088)	(0.108)
PCI	(-0.039,0.364)	(-0.068,0.446)	(-0.054,0.299)	(-0.056,0.412)	(-0.040,0.297)	(-0.069,0.350)
Females						
MDPC	0.144	0.052	0.054	0.023	**0.271** **	0.048
std.err.	(0.113)	(0.115)	(0.091)	(0.095)	**(0.109)**	(0.090)
PCI	(-0.039,0.408)	(-0.031,0.439)	(-0.035,0.336)	(-0.039,0.316)	**(-0.045,0.397)**	(-0.042,0.307)
Employed/Self-employed						
MDPC	0.018	-0.039	0.070	0.078	0.127	0.002
std.err.	(0.120)	(0.115)	(0.086)	(0.091)	(0.084)	(0.103)
PCI	(-0.055,0.420)	(-0.052,0.403)	(-0.028,0.327)	(-0.042,0.340)	(-0.034,0.299)	(-0.044,0.344)
Unemployed						
MDPC	0.110	0.045	0.022	0.054	0.159	0.036
std.err.	(0.098)	(0.109)	(0.090)	(0.107)	(0.118)	(0.090)
PCI	(-0.039,0.355)	(-0.023,0.412)	(-0.026,0.324)	(-0.036,0.404)	(-0.039,0.442)	(-0.055,0.320)
Married						
MDPC	0.171	0.116	**0.335** ***	0.135	**0.395** ***	**0.218** *
std.err.	(0.123)	(0.150)	**(0.119)**	(0.136)	**(0.107)**	**(0.122)**
PCI	(-0.064,0.435)	(-0.082,0.524)	(-0.0412,0.401)	(-0.083,0.464)	**(-0.053,0.381)**	**(-0.052,0.413)**
Single						

MDPC	0.035	0.014	0.033	0.131	0.102	-0.010
std.err.	(0.093)	(0.092)	(0.077)	(0.090)	(0.114)	(0.084)
PCI	(-0.041,0.322)	(-0.042,0.348)	(-0.041,0.288)	(-0.028,0.328)	(-0.049,0.391)	(-0.050,0.289)
Widowed/Divorced						
MDPC	0.172	-0.165	0.079	0.111	0.159	0.101
std.err.	(0.150)	(0.129)	(0.105)	(0.137)	(0.130)	(0.120)
PCI	(-0.080,0.539)	(-0.041,0.459)	(-0.059,0.365)	(-0.083,0.476)	(-0.073,0.448)	(-0.077,0.389)
No education						
MDPC	0.148	0.125	0.084	0.081	**0.253** **	0.153
std.err.	(0.103)	(0.101)	(0.085)	(0.094)	**(0.100)**	(0.102)
PCI	(-0.041,0.377)	(-0.042,0.364)	(-0.025,0.318)	(-0.041,0.339)	**(-0.046,0.364)**	(-0.047,0.356)
Middle education						
MDPC	0.110	-0.038	0.072	0.213	**0.232** *	0.048
std.err.	(0.132)	(0.146)	(0.110)	(0.135)	**(0.128)**	(0.116)
PCI	(-0.041,0.435)	(-0.082,0.522)	(-0.064,0.392)	(-0.063,0.533)	**(-0.048,0.347)**	(-0.069,0.403)
High education						
MDPC	0.047	0.008	0.041	0.004	0.052	-0.043
std.err.	(0.155)	(0.184)	(0.107)	(0.145)	(0.137)	(0.116)
PCI	(-0.090,0.548)	(-0.067,0.666)	(-0.070,0.410)	(-0.075,0.510)	(-0.054,0.526)	(-0.043,0.412)

Notes: Each result is obtained using 1,000 bootstrap replications to interval regressions;
MDPC is the marginal degree of positional concern;
Std.err. is the bootstrap standard error and PCI is the percentile method confidence intervals;
[*],[**],and [***] indicate significance at 10%, 5%, and 1% level.

Paper V

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Attitudes toward uncertainty among the poor: an experiment in rural Ethiopia

Alpaslan Akay · Peter Martinsson · Haileselassie Medhin · Stefan T. Trautmann

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Abstract We investigate risk and ambiguity attitudes among Ethiopian farmers in one of the poorest regions of the world. Strong risk aversion and ambiguity aversion were found with the Ethiopian farmers. We compared their attitudes to those of a Western university student sample elicited by the same decision task. Ambiguity aversion was similar for farmers and students, but farmers were more risk averse. Our results show that ambiguity aversion is not restricted to Western student populations, and that studies of agricultural decisions may benefit from explicitly considering ambiguity attitudes.

Keywords Risk attitudes · Ambiguity attitudes · Poverty · Agriculture

JEL Classification D81 · C93 · O12

1 Introduction

In June 2008 USAID transported 5000 layer and broiler chicks to Helmand province in Afghanistan to build the foundation for a privately owned poultry industry. The goal

A. Akay
IZA (Institute for the Study of Labor), Box 7240, 53072 Bonn, Germany
e-mail: akay@iza.org

P. Martinsson · H. Medhin
Department of Economics, University of Gothenburg, Box 640, 405 30 Göteborg, Sweden
e-mail: peter.martinsson@economics.gu.se

H. Medhin
e-mail: haileselassie.medhin@economics.gu.se

S. T. Trautmann (✉)
Department of Economics, TIBER & CentER, Tilburg University, Tilburg, The Netherlands
e-mail: s.t.trautmann@uvt.nl

of the development program was to provide new income opportunities, and especially to provide a licit alternative to the production of opium poppy. For farmers, the payoffs from the current activity of growing poppy and from the potential alternative of poultry farming are both uncertain. Because of their experience with poppy production, and their inexperience with poultry, it is conceivable that Afghan farmers feel more competent in assessing the uncertainty involved in poppy than those in poultry farming.

In decision under uncertainty, research has shown that people distinguish between prospects for which they have a clear probability assessment or feel competent because of their own expertise, and prospects for which probabilities are unknown and they feel less competent (Abdellaoui et al. 2010; Heath and Tversky 1991). The extreme case of objectively known probabilities (e.g., of tails coming up in a coin flip) is called *risk*, and the extreme case of completely unknown probabilities (e.g., likelihood of rain tomorrow) is called *ambiguity*. Ellsberg (1961) suggested that people often prefer to bet on risky prospects instead of ambiguous prospects, even if expected utility theory implies indifference. Confirming Ellsberg's conjecture, *ambiguity aversion* has been found in many empirical studies, including under market conditions and with monetary incentives (Cabantous 2007; Halevy 2007; Muthukrishnan et al. 2009; Sarin and Weber 1993).

A significant number of decisions under uncertainty is made by farmers and fishermen in developing regions of the world who often live near or below the poverty line, and for whom uncertainty affects their existence. Uncertainty in such settings has usually been studied assuming well defined probabilities of the possible outcomes. In many decisions, however, it is more likely that ambiguity as defined above prevails, with little information about actual probabilities available. Typical examples include the uptake and adaptation of new crops, new production technologies (e.g., fertilizer) and investments that involve unknown risks (e.g., water harvesting). While uncertainty has been identified as an important determinant of such farm technology adoption and subsequent effects on economic growth (Feder 1980; Feder et al. 1985; Kebede 1992), the literature does not differentiate between the effect of risk aversion and ambiguity aversion.

The aim of this article is to experimentally test whether ambiguity aversion is prevalent among small scale farmers. In particular, the current study reconsiders the findings of the only study so far on ambiguity attitudes in farming societies (Henrich and McElreath 2002). Henrich and McElreath studied risk and ambiguity attitude among Chilean Mapuche small scale farmers and found no evidence for ambiguity aversion. They argued that ambiguity aversion may be driven by cultural factors, and that it does not generalize to non-Western farming societies. More generally, Henrich and McElreath's study makes the important point that uncertainty attitudes may not always generalize from typical undergraduate student populations toward culturally and demographically different groups that are of economic interest.[1] However, their interpretations may not be completely convincing either. Two points of concern with their results are that their experiment had little power to identify ambiguity aversion,

[1] Giordani et al. (2010) also demonstrate this fact in a study mapping cross-cultural differences in uncertainty attitude across countries in the European Union.

and that there was no control experiment using a typical participant pool to put the findings into the perspective of the larger literature.[2] As we will show below, differences in the decision tasks compared to previous studies with student samples at Western universities can provide an obvious explanation for the observed ambiguity attitudes in the absence of a control group.

This article measures risk and ambiguity attitudes among small scale farmers in rural Ethiopia using an experiment with real monetary incentives, and compares the results to data from university students in the Netherlands facing the same decision tasks. Our Ethiopian participants differ from typical undergraduate subject pools in terms of their occupation, wealth, and cultural background. We find clear evidence for ambiguity aversion with both the Ethiopian farmers and the Dutch students. The result shows that studies of farming decisions may benefit from the inclusion of ambiguity. Farmers are more risk averse than the students. For the farmers we relate their risk and ambiguity attitudes to socio-economic variables and health status. Poor health is positively related to risk and ambiguity aversion. The next section gives a description of the participant pool and introduces the experimental design. The results are presented in Sect. 3 and discussed in Sect. 4.

2 Participants and experimental design

2.1 Participants

The experiment was conducted in the village of Abraha We Atsbaha in the northern highlands of Ethiopia. The majority of the Ethiopian population resides in the highlands, where small-scale subsistence agriculture is the main economic activity. Highland agriculture in Ethiopia is characterized by population pressure, extreme land fragmentation, severe soil degradation, and heavy dependence on rainfall. As a result, the overall outcome is one of the lowest agricultural productivity levels in the world. During the last few decades, the number of droughts has exacerbated the problem, especially in the northern parts of the country. Abaraha We Atsbaha is one of many poor villages in a region where most people depend on food aid programs to survive between the two annual harvests.

Our sample consisted of 92 adults with little or no formal education, and 30% of those who participated in our experiment were illiterate. Subjects were randomly selected from a list of 584 households, with either the male or female household head participating. All subjects were small scale farmers and mainly growing wheat, maize, barley, and teff. Most families also own some livestock such as cattle and sheep. All participants were Christians.

[2] In particular, Henrich and McElreath rejected ambiguity aversion because they found that a majority of farmers preferred an ambiguous prospect paying either 5000 pesos or zero, over a sure payoff of 1000 pesos. Note that for a risk and ambiguity neutral subject this payoff calibration implies a preference for the sure payoff over the ambiguous prospect only for expected probabilities smaller than 20%. Thus, even a significantly risk and ambiguity averse agent may prefer the prospect over the sure payment.

2.2 Payoffs

Each participant could win up to 20 Ethiopian birr (ETB). At the time of the experiment the exchange rate was ETB 9.67 = US$ 1. In this region, the daily wage for unskilled farm labor varies between 10–15 Birr, depending on the season. Thus, the stakes involved roughly corresponds to 2 daily wages.

2.3 Procedure

We elicited each participant's *certainty equivalents* for a risky and an ambiguous prospect: the sure payment such that the subject is indifferent between receiving the prospect or the sure amount. The risky prospect allowed the participant to bet on the color of a ball drawn from a bag with exactly 5 white and 5 yellow balls, and to win ETB 20 if they correctly guessed the color. This prospect thus offers a 50-percent chance to win the prize. The ambiguous prospect allowed participants to bet on the color of a ball drawn from a bag with 10 balls, where the proportion of white and yellow balls was unknown. If subjects guessed the color correctly, they won ETB 20.

These two prospects represent the risky and ambiguous option in the Ellsberg (1961) two-color choice task. The ambiguous option is always at least as good as the risky option. If participants are indifferent between betting on either color in the ambiguous option, they should be indifferent between playing the bet with the risky option or with the ambiguous option. In this case, they will have identical certainty equivalents for both options. If they believe that there are more white balls than yellow balls in the ambiguous bag, they will bet on white in the ambiguous prospect and should prefer this prospect over the risky prospect. A similar argument holds if the participants believe that there are more yellow balls in the ambiguous bag. A preference for the risky prospect thus reveals ambiguity aversion.

For each prospect, we elicit participants' certainty equivalents using a choice list. Subjects made 20 choices between a sure payoff and playing the prospect, and these choices were arranged in an ordered list. The sure payoff increases from ETB 1 to ETB 20 when going down the list. For very small sure payoffs, most participants will prefer to play the prospect; for very large sure payoffs, most participants will prefer the sure cash. That is, most participants will switch from sure cash to playing the prospect at some point. We calculate the certainty equivalent as the midpoint between the lowest sure payoff for which the participant takes the sure cash and the highest sure payoff for which the participant prefers to play the prospect.[3]

Choice lists are popular in experimental economics studies (e.g., Holt and Laury 2002). Our lists involve the simplest possible structure, with each choice involving the same risky (ambiguous) prospect and some sure amount. Note that this choice list methodology differs from the list employed by Binswanger (1980), where participants were asked to choose one prospect from a list of prospects that differed with respect to

[3] Illustrations and instructions are provided in the online appendix: http://dl.dropbox.com/u/11242744/Akayetal2011_Online%20Appendix.pdf

their expected payoff and variance, and the selected prospect then served as an index of risk aversion. Our method directly elicits the certainty equivalent of each prospect.

Participants made choices in one choice list for each prospect, and therefore, they made 40 choices in total. After the participants made all choices, one of these choices was randomly selected for real play for each participant. Depending on the decision in the selected choice problem, the participant received either the sure cash amount or played the prospect with a chance to win ETB 20.

Because most of our subjects had no formal education and many were illiterate, the instructions were given verbally in local language, using posters as visual aids. All probabilities and randomizations were demonstrated using balls and dice, and no explicit reference to probabilities was given. Visual aids have been shown to improve the understanding of risks by participants without formal training in probability theory and were clearly necessary in our sample (Carlsson et al. 2004; Corso et al. 2001). The prospects and the betting tasks were demonstrated for the risky option by filling the bag with 5 white and 5 yellow balls. A subject chose a color by putting a ball of this color on the table. Next, a ball was randomly drawn by the participants from the bag. If the colors matched, the subject was paid ETB 20. The actual experiment was conducted with one participant at a time in a private area. The binary choices between the prospects and the sure amounts of money were presented to the participant one choice at a time. The experimenter filled out the choice list according to the participant's preference in each choice until all 40 choices had been filled out.

2.4 Control group

As a comparison standard, we use data from an experiment with undergraduate university students at a Dutch university facing the same decision task as above (Trautmann et al. 2011, experiment 4). The tasks and randomizations were done in the same (non-computerized) way as for the farmers. The prize was €50 for the two prospects for the student sample, and 2 of 79 students were randomly selected for real play of their choices. Students received written instructions and filled out the choice lists themselves. The student experiments were conducted in a classroom.

3 Experimental results

3.1 Risk and ambiguity attitudes

Risk attitudes

The certainty equivalents for the risky prospect allow us to control for risk attitude in the measurement of ambiguity below. Risk attitudes are of independent interest, however, and we report the data here. In this section, we assume expected utility with power utility and report constant relative risk aversion (CRRA) coefficients. This is the most common specification in the literature and we can thus benchmark our results to previous findings. With the simple two-outcome gain prospects studied here,

Table 1 Distribution of constant relative risk aversion parameters in Ethiopian farmers versus university student samples

	Risk neutral/ loving	Mildly risk averse	Risk averse	Highly risk averse
	$\rho \leq 0.15$ (%)	$0.15 < \rho \leq 0.41$ (%)	$0.41 < \rho \leq 0.68$ (%)	$\rho > 0.68$ (%)
Ethiopian farmers ($n = 92$)	22	11	10	58
Dutch students ($n = 79$)[a]	19	35	44	1
U.S. students ($n = 93$)[b]	19	19	23	39

[a] Trautmann et al. (2011)
[b] Holt and Laury (2002, p. 1649, Table 3, last column). Identical tasks in Ethiopia and the Netherlands. A slightly different task has been used for U.S. student by Holt and Laury, with all choice options involving only non-degenerated gambles

the results do not change if we assume linear utility and interpret risk aversion in terms of probability weighting as in rank dependent utility and prospect theory.[4]

The median coefficient of relative risk aversion in the Ethiopian sample is $\rho = 0.73$, which is significantly larger than the median of $\rho = 0.34$ in the Dutch student sample (Mann–Whitney U test, $z = 4.391$, $P < 0.01$). Table 1 shows that the percentage of risk-neutral and seeking participants is similar in both groups, but that among the farmers there are few mildly and medium risk averse. In particular, 41 of the 92 participants in Ethiopia preferred the sure payoff in all choices. The table also includes the distribution of CRRA parameters estimated by Holt and Laury (2002) for a sample of U.S. students using real payoffs up to $77 (see Holt and Laury 2002, p. 1649, Table 3, last column). This study is often used as a benchmark in the economics literature. Their study indicates more risk aversion than did the Dutch study and the distribution was closer to our Ethiopian sample. However, Holt and Laury (2002) had only about 40% highly risk-averse participants, compared to the 60% highly risk averse in our experiment. Thus, the main difference between the farmers and the students is the presence of a significant minority in the former group that strictly avoids uncertainty.[5]

Ambiguity attitudes

Ambiguity attitude refers to the difference between the evaluation of the risky prospect and the ambiguous prospect. As a measure of *ambiguity aversion*, we employ the value

[4] Because we have only one indifference point (one certainty equivalent for one risky prospect), we would have to restrict the analysis to single-parameter probability weighting functions. Estimation of more flexible weighting functions requires more information and therefore more complex elicitation procedures (Abdellaoui 2000; Bleichrodt and Pinto 2000; Booij et al. 2010). See Botzen and Van den Bergh (2009) and Humphrey and Verschoor (2004) for such measurements in an environmental/agricultural context.

[5] Interestingly, Cohen et al. (2010) report a similar finding of extreme risk aversion for a French non-student population sample.

$$\frac{\text{certainty equivalent risky prospect} - \text{certainty equivalent ambiguous prospect}}{\text{certainty equivalent risky prospect} + \text{certainty equivalent ambiguous prospect}}.$$

That is, ambiguity aversion is defined as the difference between the subject's certainty equivalent of the risky prospect and her certainty equivalent of the ambiguous prospect, normalized by the sum of the two certainty equivalents. This measure ranges from -1 (ambiguity loving) to 0 (ambiguity neutrality) to 1 (ambiguity averse). The larger the difference between the two certainty equivalents is, the stronger the ambiguity attitude. The normalization controls for the fact that a difference of ETB 2 weighs more heavily for a subject who is very risk averse (e.g., certainty equivalent risky prospect of ETB 4) than for a subject who is relatively risk neutral (e.g., certainty equivalent risky prospect of ETB 9).

Because of the strong risk aversion in the Ethiopian sample, we have 41 participants who revealed the lowest feasible certainty equivalent for the risky prospect. For these participants we cannot distinguish between ambiguity neutrality and aversion, and therefore, we excluded them from the analysis. Table 4 in the Appendix shows that the ambiguity attitudes of the excluded subject were very similar to those of the included subjects. Ambiguity attitudes did not differ between the Ethiopian farmers and the Dutch students (Mann–Whitney U tests, $z = 1.535$, P value > 0.1). In both samples, we found clear ambiguity aversion (Wilcoxon tests, P values < 0.01). Table 2 shows the distribution of ambiguity attitudes in the Ethiopian and the Dutch samples, based on certainty equivalents, and in three comparison studies. Roca et al. (2006) gave British university students a direct choice between betting on the color in the risky or the ambiguous Ellsberg two-color urn. The distribution of ambiguity aversion in their basic experiment replicates standard findings in the literature and is similar to our results in Ethiopia.

The two other studies illustrate the effect of two design features on ambiguity attitude. The differences caused by these design variations are much stronger than the differences between the different samples of participants in the first three rows of the table. Chesson and Viscusi (2003) studied ambiguity attitude for loss prospects among business owners in the U.S. Clearly, there is more ambiguity seeking in their study compared to the current study, consistent with findings for losses in the literature (Cohen et al. 1985; Hogarth and Kunreuther 1985; Kahn and Sarin 1988). Keren and Gerritsen (1999) elicited Dutch university students' willingness-to-pay (WTP) for the risky and the ambiguous Ellsberg two-color urn. They found clear ambiguity aversion, and almost none of the subjects were willing to pay more for the ambiguous option. It is clear from the table that studies of non-student and non-Western subject pools should either apply established procedures, or include an explicit student control group before claims about the generalizability of preferences (or the lack thereof) can convincingly be made.

3.2 Effects of demographic variables

Before the experiment was conducted, the Ethiopian participants were interviewed on a number of socio-economic background variables. In the econometric analysis,

Table 2 Ambiguity attitudes among Ethiopian peasants versus comparison samples

	Ambiguity seeking (%)	Ambiguity neutral (%)	Ambiguity averse (%)	Elicitation Method
Ethiopian farmers ($n = 51$)	20	24	57	CE, gains, real incentives
Dutch students ($n = 79$), Trautmann et al. (2011)	15	43	42	CE, gains, real incentives
British students ($n = 72$), Roca et al. (2006)	39	n.a.	61	Choice, gains, hypothetical
Business owners ($n = 130$), Chesson and Viscusi (2003)	56	n.a.	44	Choice, losses, hypothetical
Dutch students ($n = 39$), Keren and Gerritsen (1999)	3	46	51	WTP, gains, hypothetical

Notes: Identical tasks in Ethiopia and the Netherlands. Roca et al. (2006), Table 1, control; Chesson and Viscusi (2003), Table III, panel B; Keren and Gerritsen (1999), Table 4, panel b

we regress the risk and ambiguity attitudes on this set of explanatory variables. The background variables include personal information and family background, but also measures of economic well-being. Wealth is approximated by land size, while income is measured by consumption. Consumption is used because it fluctuates much less than direct measures of income which vary a lot due to harvesting periods.

For risk attitude we avoid dependence on expected utility assumptions by using the pure certainty equivalent multiplied by -1 as an index or risk aversion.[6] In the regressions, we control for censoring of our measure because a sizable fraction of participants revealed the lowest possible certainty equivalent. Thus, we used a Tobit model for our analysis of risk attitude. We also tested whether socio-economic variables explain the presence of extreme risk attitudes by including a Probit regression for dummy variable that assumes the value of 1 if the certainty equivalent is censored at 1, and 0 otherwise. For ambiguity attitude, we apply OLS regressions for the measure described above because there is no censoring of ambiguity attitude. Regression results are shown in Table 3. Positive parameter values in the regressions imply increasing risk or ambiguity aversion, or increasing likelihood to show extreme level of risk aversion, respectively. Marginal effects are reported for the probit regression.

The regression results show that poor health is related both to stronger risk aversion and stronger ambiguity aversion. In particular, for risk, the subjects with poor health status demonstrate extreme risk aversion. Apart from health effects, we find that household size increases risk aversion, while being married is correlated with reduced ambiguity aversion. No other socio-economic variables had an influence on uncertainty attitudes in our data.

[6] The higher the certainty equivalent, the lower the risk aversion.

Table 3 Regression analysis for risk and ambiguity aversion for the Ethiopian sample

Explanatory variable	Dependent variable		
	Risk aversion (Tobit)	Extreme risk aversion (Probit)	Ambiguity aversion (OLS)
Age	−0.562 (.573)	−0.046 (0.031)	−0.030 (0.026)
$Age^2/100$	0.419 (0.574)	0.041 (0.031)	0.029 (0.023)
Female	−2.941 (2.557)	−0.089 (0.142)	−0.129 (0.146)
Poor Health	5.265* (2.822)	0.344** (0.133)	0.339** (0.154)
Married	−3. 887 (3.430)	−0.174 (0.183)	−0.433** (0.167)
Household size	1.574* (0.836)	0.102** (0.044)	0.045 (0.062)
Number of dependent children	−0.886 (1.006)	−0.029 (0.053)	0.029 (0.045)
Land size	−0.192 (1.203)	−0.007 (0.069)	0.092 (0.073)
Consumption (100 ETB)	0.016 (0.245)	0.006 (0.013)	0.014 (0.010)
Number of observations	84	84	45

Notes. **, * denote significance at the 5 and 10% level, standard errors in parenthesis (robust standard errors for OLS). Marginal effects are reported for the Probit regression

4 Discussion and conclusions

There has been much interest in cross country differences in attitudes towards uncertainty, and numerous studies have measured attitudes toward prospects with objectively known payoff distributions in developing countries and small scale societies (Binswanger 1980; Bohnet et al. 2008; Kuznar 2001; Yesuf and Bluffstone 2009; Harrison et al. 2010; Dillon and Scandizzo 1978; Elamin and Rogers 1992). Most of these studies found a similar degree of risk aversion as in typical student samples from developed countries. Henrich and McElreath (2002) showed that there can be significant differences between culturally diverse farming societies, however. These authors also suggested the importance of cross cultural comparison of attitudes toward ambiguous prospects, when probabilities are unknown. In the real world ambiguity is ubiquitous; ambiguity-driven preferences between traditional technologies with well-known payoff distributions and new technologies and crops with unknown risks would therefore be relevant to innovation and development. Henrich and McElreath report, however, that ambiguity aversion is not prevalent in their farmers. They suggest that ambiguity aversion may be restricted to Western student populations.

We measure ambiguity aversion in a sample of Ethiopian small scale farmers, using real incentives and concrete visual representations of prospects in terms of differently colored balls in urns. We compared the Ethiopian data to data from an experiment among Western university students using exactly the same decision task. We have shown that holding design features constant between groups is necessary to draw conclusions regarding cross cultural differences (see also Bohnet et al. 2008; Kocher et al. 2008; Roth et al. 1991).

We find both risk aversion and ambiguity aversion for Ethiopian farmers. Risk aversion was stronger for the farmers than for the comparison student samples,

and this effect is driven by extreme risk attitudes for a significant minority of the farmers. Comparing the distribution of risk attitudes with other findings from Western student populations shows, however, that variation is well within the range of the variation expected across different experiments. In any case, the data support the view that strong risk aversion predominates among the farmers. Ambiguity aversion did not differ between Ethiopian peasants and Dutch university students, and both groups show ambiguity aversion. Ambiguity attitudes in the samples considered in our study are also comparable to other findings reported in the literature.

Attitudes toward uncertainty are important factors in the analysis of economic problems and policy in developing countries. Risk-sharing, crop selection, and precautionary saving influence welfare in risky agricultural environments and are influenced by economic actors' attitudes toward risk (Dercon 1996; Jalan and Ravallion 2001; Kochar 1999; Pan 2009; Udry 1994). On the other hand, ambiguity aversion has been widely observed among student samples, and has been proposed as an explanation for various market phenomena (e.g., Mukerji and Tallon 2001; Peijnenburg 2011; Zeckhauser 2006). We find that Ethiopian small scale farmers exhibit ambiguity aversion. Our result shows that empirical studies on farming decisions in developmental context may benefit from the inclusion of ambiguity attitudes.

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Appendix: ambiguity attitudes of excluded subjects

Table 4 Ambiguity attitudes in the included sample and the excluded sample

	All	Included (CE risk > 1)	Excluded (CE risk $= 1$)
Ambiguity seeking	18 (19%)	10 (20%)	8 (19%)
Ambiguity neutral	46 (50%)	12 (24%)	34 (81%)
Ambiguity averse	29 (31%)	29 (56%)	

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Paper VI

Preferences toward Efficiency and Pro-Sociality: A Comparison across Subject Pools*

Peter Martinsson[1] Haileselassie Medhin[1]

[1]University of Gothenburg

Abstract

Mixed experimental results on how social preference motivations fare against efficiency concerns have raised important issues related to representativeness of different subject pools, especially with the divergence of results between economics students and others. This paper extends the experimental investigation to non-Western subjects. We perform experiments that were conducted with Western subjects in earlier studies, but we use Ethiopian university students and small-scale farmers as subjects. Our results show that Ethiopian farmers behave similarly to non-economics students and non-students in Western countries, while Ethiopian economics students behave similarly to economics students in Western countries.

Key words: *Social preferences, subject pool, Ethiopia, experiment.*

JEL Classifications: C91, D63, D64.

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1 Introduction

Since the late 1990s, there has been an increased interest in understanding social preferences. This has resulted in the development of theoretical models of social preferences (e.g., Rabin, 1993; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Falk and Fischbacher, 2006). Experimental research has further shown that social preference motivations are important determinants of individual behavior beyond selfish concerns for oneself (e.g., Charness and Rabin, 2002; Engelmann and Strobel, 2004; 2007; Fehr et al., 2006; for a recent overview of empirical results, see, e.g., Cooper and Kagel, 2013). However, an outstanding question is to what extent results from laboratory experiments can be generalized, not the least because most are based on a subject pool of undergraduate students from Western countries.[1] The issue of generalization is vital if laboratory experiments are to inform economic theory and policy (e.g., see Levitt and List, 2007; Falk and Heckman, 2009; Henrich et al., 2010). Levitt and List (2007) identify *population* as one of the key factors that limit the external validity of results from laboratory experiments.[2] The problem of generalization relates to issues of population and can be divided into two layers: (i) the concern that experimental subjects may not be representative of their own population; and (ii) the concern that experimental subjects may not be representative of people outside their own population. While the first issue is mainly related to the concern of self-selection among experiment participants, the second issue is far more complex and is related to cultural, economic and other differences between different populations.

Two stylized facts can be stated from the recent literature on the self-selection layer of the generalization problem. First, there indeed seems to be self-selection of subjects in experiments with university student subjects. Slonim et al. (2013) find that students who responded to a lab experiment invitation had lower income, more leisure time availability, and stronger interest in economics, and spent more time volunteering. Second, and perhaps more important, there is less reason to be concerned about the effects of self-selection into laboratory experiments, at least as far as attitudes toward pro-sociality concerned. Cleave et al. (2013) present experimental evidence showing that social and risk preferences of self-selected students using common recruitment

[1] For example, Falk et al. (*in press*) find that almost 90% of all experimental papers published in the top five field journals used students as subjects.

[2] The other factors indentified by Levitt and List (2007) are the level of scrutiny, anonymity, context and stakes.

procedures were not significantly different from classroom experiments with students, where the latter case avoided selection problems. Anderson et al. (2013) make a similar comparison among non-student adults, and find that self-selection does not create bias in observed behavior toward pro-sociality. Using a different approach, Abler and Nosenzo (2013) show that, while the mention of 'making money' in invitations created a big difference in participation, observed behavior on pro-sociality was similar among groups who responded to invitations that stressed making money, helping researchers, or both. By combining real-world donation behavior of experiment participants and non-participants at the University of Zurich between 1998 and 2005, Falk et al. (in press) also show that there is no selection of more pro-social students into laboratory experiments. Overall, there does not seem to be a problem with generalizing from lab participants in Western universities to their own population.[3]

The second layer of the generalization problem, which is also the focus of our study, has been at the center of a heated debate for more than a decade. The issue is raised in the context of generalizing patterns of behavior from subjects of a certain population to other populations. This is particularly important given the fact that the bulk of experimental research is based on undergraduate university students from Western countries. Henrich et al. (2010) argue that Western university students are too unique a 'sub-subpopulation' to be representative of general human behavior. Gächter (2010), however, presents arguments for why university students might be an appealing subject pool for testing economic theories such as social preferences. The implicit assumption of generality in economic theories, Gächter (2010) argues, means that university students are as representative as other subject pools, but perhaps more attractive because of their more than average cognitive sophistication and availability. Camerer (2011) also points out that the issue of external validity is less of a concern for experiments that aim to understand general principles.[4] These contrasting views on the generalizability of experimental results should not be limited to university students. Indeed, subjects from any given population will be different for the very reason

[3]Of course, there can always be a debate about what costitutes a given subject's 'own population'. The classification is therefore bound to be very subjective. For example, a given student can be thought of as to belonging to the 'student' population, 'economics student' population or 'male economics student' population. Given this subjectivity, the phrase 'their population' is better understood as 'the population that the subjects are *randomly* drawn from.

[4]In this vein, Falk and Heckman (2009) argue that the key for experiments, laboratory or field, is to be able to isolate the causal effect of interest.

that they are from a different population. The question is whether this common trait systematically affects experimental results. That is, it is not enough to question the generalizability of experimental results from a specific population just because they are unique, but rather the focus should be whether their uniqueness has a systematic effect on the direction and magnitude of an experimental test. For example, if Western university students have a common trait that would make them behave in a certain way in social preference experiments, these results cannot be generalizable to other populations.[5]

But, while it may be easy to point out that the behavior of subjects of a certain population is not representative of other populations, it is not very clear whose behavior is generalizable across populations. Indeed, the search for the perfectly representative subject pool may be futile. A better way to improve the generalizability of experimental results is to take advantage of one of the key features of laboratory experiments: replicability. Replication of economics experiments on different populations improves the generalizability of results by allowing for better understanding of systematic biases that might arise from differences in culture and other socio-economic characteristics of populations. There is now a growing literature on social preferences along this line of research. Studies have shown that there is variation in pro-sociality, both within different segments in culturally similar societies and across different cultures. For example, the study by Anderson et al. (2013) shows that self-selected students are less pro-social than non-student adults in the US. Bellemare et al. (2008) also find heterogeneities in inequality aversion behavior across different segments of Dutch society, with the young and highly educated being the least inequality averse. On the other hand, cross-country experimental studies have shown that there seem to be more pro-social concerns in less developed countries than in developed countries (e.g., Henrich et al., 2001, 2005). We aim to supplement this line of research by presenting comparative experimental research from Ethiopia on a specific thread of the research on social preferences: preferences toward efficiency vs. equality, based on the experimental design by Engelmann and Strobel (2004). By gathering experimental evidence based on student and non-student subject pools from a non-Western society, our aim is to

[5]There are, however, situations where studying the systematic effect of group characteristics on behavior is interesting in its own right. For example, Fehr and List (2004) compare CEOs and students in trust games and show that CEOs manifest higher efficiency than students. The unique studies by Henrich et al. (2001) and Henrich et al. (2005) are also examples of how economic and social characteristics of different populations can be used to explain behavioral differences observed in experiments.

contribute both to the debates on the representativeness of university students and non-university students, and to the discussion on the role of cultural differences.

The limited experimental evidence on the relative importance of social preference and efficiency motivations has been mixed, triggering a debate on the representativeness of different subject pools. Engelmann and Strobel (2004) (ES hereafter) implemented a series of allocation experiments on German economics students that involved trade-offs between efficiency, inequality aversion and the Rawlsian Maximin principle. Their main result was that efficiency concerns are more important. In a follow-up study, Fehr et al. (2006) (FNS hereafter) selected two of the allocation games from ES and conducted experiments using different subject pools. By using university students from a mix of academic disciplines, as well as non-students in German-speaking countries, FNS find a larger proportion of subjects concerned with inequality aversion than in the original ES study, which only used economics students. A reply by Engelmann and Strobel (2006) to FNS and a new study by Engelmann and Strobel (2007) using an internet experiment further underline the relevance of discussing subject pool issues.[6]

This paper uses rural farmers and economics students from Ethiopia in a careful replication of selected allocation experiments from ES and FNS, with the objective of extending the discussion of generalizing experimental results beyond Western subjects. We believe that extending the tests to a poor, agrarian subject pool is an important step toward exploring homogeneity in preferences and hence toward the possibility of generalizing results across cultural and economic differences. By including results from Ethiopian economics students, we also explore whether the effects of economics training persist in a non-Western subject pool.

The paper is organized as follows. Section 2 presents the experimental design and procedure, Section 3 presents the experimental results, and Section 4 concludes.

[6]Engelmann and Strobel (2007) also provides an excellent review of inequality aversion models and the relatively scarce experimental evidence on allocation decisions in dictator games with more than two players, similar to what is used in this study.

2 Experimental Design and Procedure

Our choice of experimental design is motivated by our pursuit of extending the subject pool discussion in ES and FNS. We therefore use the same two allocation games as in FNS, who in turn selected them from the larger design used in ES. These two games, denoted as *Ey* and *P* (see Table 1), belong to a group of games that ES labeled the "Rich and Poor games". In each of the two games, the decision is made by a subject denoted "Person 2" (i.e., the dictator), who belongs to a group consisting of three members, including herself. The decision task is to choose one out of three alternative payoff allocations, labeled Left, Middle and Right. In each of the alternatives, the distribution of the payoff between the three people differs, except for the payoff of the dictator. The payoffs become more equal as one moves from left to right in each game, while the total pie decreases. Because the pay-off for the decision-maker is fixed in all three alternatives, the game is focused purely on how to distribute the money between the other two group members, hence the name "Rich and Poor game." Purely selfish players are therefore indifferent among the three alternatives in each game. The key difference between the two games is that the dictator has the middle payoff in the *Ey* game, while the dictator has the lowest payoff in the *P* game. As a result, a choice of Right in the *Ey* game could be motivated by either inequality aversion or Maximin preferences, while none of the three alternatives in the *P* game is preferable from a Maximin perspective. A choice of Left in both games shows a preference for efficiency. Each subject in our experiment played only one of the two games.

Table 1. Experimental Design (payoffs in Ethiopian Birr[7])

	Treatment *Ey*			Treatment *P*		
	Left	*Middle*	*Right*	*Left*	*Middle*	*Right*
Person 1 payoff	21	17	13	14	11	8
Person 2 payoff (D)	9	9	9	4	4	4
Person 3 payoff	3	4	5	5	6	7
Total payoff	33	30	27	23	21	19
Efficiency prediction	*Left*			*Left*		
Maximin prediction			*Right*	*Cannot be tested*		
Fehr and Schmidt (1999)			*Right*			*Right*
Bolton and Ockenfels (2000)			*Right*			*Right*

We use two different ways to assign roles in the experiment: (i) role-uncertainty and (ii) role-certainty. This gives us a clean comparison with previous studies, because role-uncertainty was used in ES and role-certainty in FNS. In the role-certainty treatment, the roles as Person 1, Person 2 and Person 3 are randomly determined before the decision-making. Thus, only the player assigned as Person 2 chooses her preferred allocation, while the other two members of the group just wait for their payoffs. In the role-uncertainty treatment, all three subjects make choices as Person 2. After they have finished their choices, they are randomly assigned to be Person 1, Person 2 and Person 3, where the choice by Person 2 is the income-relevant choice.[8]

The experiments were conducted with Ethiopian farmers and university students in economics. The experiment with farmers took place in the district of Tenta, in the South Wollo Zone of the Amhara Region in the Ethiopian highlands. Rain-fed subsistence agriculture is the main livelihood in the area. Our subjects were randomly

[7]The official exchange rate at the time of the experiment was 1 USD, equal to 9.86 Birr. For comparison, a daily laborer on a farm earned between 15-20 Birr per day depending on his/her duties in this region during the time of our experiment. All subjects were paid a show-up fee of 5 Birr.

[8]The predicted effect of different role assignments on actual choice is a bit unclear. Even though they did not find statistically significant differences between the two treatments, Engelmann and Strobel (2004) speculate that role-uncertainty could increase the concern for the well-being of the other two group members. But it is difficult to say more about the direction of the effect as we do not know the exact manner in which the decision maker perceives the well-being of the others. This design difference is one of the points raised in Engelmann and Strobel (2006)'s reply to FNS.

selected household heads from lists provided by the district administration.[9] Nothing was mentioned about the details of the experiment, except that they would be given a show up fee of 5 Ethiopian Birr and would participate in an economics experiment in which they might earn more money. To make sure that no one knew about the details of the experiment beforehand, the experiment was planned so that it could be done in a single day by executing different treatments in parallel and over-lapping sessions. The experiments were conducted in a local school, and the subjects were seated far away from each other in the lecture rooms to guarantee privacy. We also conducted the experiment with university students at Addis Ababa University, where we recruited students using posters. In both experimental locations, Amharic is the local language. The experimental instructions were translated from English to Amharic by one translator, and another translator conducted the reverse translation to ensure equivalence. To solve differences in translation, the two translators discussed the script with one of the authors who speaks both languages fluently.

Because a large fraction of the farmers could not read and write, the instructions were read aloud to them, as in, e.g., Henrich et al. (2001). We stapled currency to posters to illustrate the possible allocations of money in each of the three alternatives. Before the answer sheet was delivered, a subject could privately ask the experimenters any questions. Then each subject received the answer sheet and indicated her choice by highlighting the preferred box on the sheet handed out to them. Next to the boxes on the answer sheet, there was also a pictorial illustration of the allocations, similar to the one shown on the poster. The experiment was conducted with identical procedures, except for treatment differences. After the experiment was completed, the payoff was calculated and paid privately in sealed envelopes upon departure.

[9] 500 people were randomly selected from the list and invited in person a few days prior to the experiment day. A total of 352 people (78%) showed up. About one fourth of our subjects are female and the average age is 46. Note that we needed three times the eventual number of subjects for the role-certainty treatments, as 1 person out of each group of three was randomly selected as dictator. We believe this is quite a high turnout for an experiment like this. The experiment day was set to be on a holiday to minimize self-selection problems (even if, as pointed in the introduction, there is not too much to worry about the implications of self-selection on observed behavior).

3 Results

The results of our experiments are summarized in Table 2, together with previous findings by ES and FNS. It should be remembered that role-uncertainty was used in ES, while role-certainty was used in FNS. Our results show that the farmers chose allocations that support inequality aversion or Maximin preferences, or both, rather than efficiency. On the other hand, the efficiency outcome was preferred by the economics students. It is worth noticing that that this divergence between economists and non-economists has been observed in previous studies with a completely different national or cultural population.

In our findings, in the *Ey* treatment, the alternative consistent with Rawlsian Maximin and inequality aversion is the most frequently chosen by farmers. In the role-certainty version of the *Ey* treatment, Rawlsian Maximin and/or inequality aversion is chosen 44.7% of the time, while in role-uncertainty it is chosen 76.3% of the time. On the other hand, the efficient outcome is chosen by only 36.8% in the role-certainty experiment of the *Ey* treatment, while it is chosen by 15.3% in the role-uncertainty scenario.

In the *P* treatment with role certainty, 60% of the farmers selected the alternative predicted by inequality aversion, and 20% selected the most efficient outcome. For role uncertainty in the *P* treatment, 57.6% chose inequality aversion and 28.8% chose efficiency. Note that subjects with Maximin preferences should be indifferent between all three alternatives.

In contrast to farmers, in the *Ey* treatment (with role-uncertainty), a higher proportion of the Ethiopian economics students chose the efficient outcome, corresponding to 47.9% of the students. Moreover, only 39.6% of the students chose the outcome supporting Maximin preferences and inequality aversion. The pattern is similar in the *P* treatment (with role-uncertainty), where only 20.8% of the students appear to be inequality-averse.

Table 2. Experimental Results

Allocation		Treatment *Ey*			Treatment *P*		
		A	B	C	A	B	C
Person 1 payoff		21	17	13	14	11	8
Person 2 payoff		9	9	9	4	4	4
Person 3 payoff		23	4	5	5	6	7
Efficiency prediction		A	-	-	A	-	-
Inequality aversion prediction		-	-	C	-	-	C
Rawlasian maximin prediction		-	-	C	A	B	C
Role-certainty	Choices (absolute)	14	7	17	8	8	24
(Farmers, Ethiopia)	Choices (percent)	36.8	18.4	44.7	20	20	60
Role-uncertainty	Choices (absolute)	9	5	45	17	8	34
(Farmers, Ethiopia)	Choices (percent)	15.3	8.5	76.3	28.8	13.6	57.6
Role-uncertainty	Choices (absolute)	23	6	19	17	21	10
(Economists, Ethiopia)	Choices (percent)	49.9	12.6	39.6	35.4	43.8	20.8
ES	Choices (absolute)	12	7	11	18	2	10
(Economists, Berlin)	Choices (percent)	40	23.3	36.7	60	6.7	33.3
FNS	Choices (absolute)	72	12	25	63	16	30
(Economists, Munich)	Choices (percent)	66.1	11	22.9	57.8	14.7	27.3
FNS	Choices (absolute)	22	13	48	21	17	45
(Non-economists, Munich)	Choices (percent)	26.5	15.7	57.8	25.3	20.5	54.2
FNS	Choices (absolute)	31	9	18	31	9	18
(Economists, Zurich)	Choices (percent)	53.5	15.5	31	53.5	15.5	31
FNS	Choices (absolute)				8	8	20
(Non-economists, Zurich)	Choices (percent)				22.2	22.2	55.6
FNS (Non-economists inc.	Choices (absolute)	61	23	78	53	25	84
employeers, Zurich)	Choices (percent)	37.7	14.2	48.1	32.7	15.4	51.9

We conducted several statistical tests using the Fisher exact test to compare our treatments with each other and with the results from previous studies. The results of our tests are summarized in Table 3. Note that, in our comparisons with the two previous studies, we account for design differences related to certainty in the assignment of roles among the farmers. Under role uncertainty, we find a significant difference in behavior between Ethiopian farmers and students, both for the *Ey* treatment and the

P treatment.[10] This finding further establishes the economist-non-economist divide. If we compare Ethiopian farmers with the findings by ES and FNS, our results are generally closer to FNS. Farmers who participated in treatments *Ey* and *P* behaved significantly differently than the Berlin economists in ES and the Munich economists in FNS. On the other hand, we find that there is no statistical difference between the proportion of choices of Ethiopian farmers and Munich non-economists in FNS. We find a similar pattern when we compare results from our sample with that of Zurich economists and non-economists in FNS. While Ethiopian economics students behave significantly differently from Ethiopian farmers in both treatments, there is no significant difference between Ethiopian and European economics students in the *Ey* treatments. Overall, our results suggest that there is something about economics students that pulls them toward efficiency, while efficiency is not the main driver of non-economists, whether they are European city dwellers or poor Ethiopian farmers.

[10]We also find a statistically significant difference between the role-uncertainty and role-certainty treatments for the Ey treatment among Ethiopian farmers, where efficiency is a stronger motivation under role-certainty, while inequality aversion is stronger under role-uncertainty (p-value = 0.007). However, we do not find any significant difference between the two different assignments of roles among farmers for the P treatment (p-value = 0.484). As pointed out earlier, our main objective in executing our experiments using both approaches is not to explain potential differences, but to make our results comparable to the results of ES and FNS.

Table 3. Fisher exact test comparison results (*p-values*)

Sample	Role-certainty (Farmers, Ethiopia)	Role-uncertainty (Farmers, Ethiopia)	Role-uncertainty (Economists, Ethiopia)
Role-certainty (Farmers, Ethiopia)	-	*Ey:0.007; P:0.484*	-
Role-uncertainty (Economists, Ethiopia)	-	*Ey:0.0003; P:0.0001*	-
ES with role uncertainty (economists, Berlin)	-	*Ey:0.014; P:0.019*	*Ey:0.4365; P:0.0011*
FNS with role certainty (economists, Munich)	*Ey:0.006; P:0.0001*	-	-
FNS with role certainty (non-economists, Munich)	*Ey:0.411; P:0.810*	-	-
FNS with role certainty (economists, Zurich)	*Ey:0.263; P:0.002*	-	-
FNS with role certainty (non-economist students, Zurich)	*Ey:NA; P:0.905*	-	-
FNS with role certainty (non-economists inc. employees, Zurich)	*Ey:0.804; P:0.289*	-	-

4 Conclusion

Generalizability of experimental results about social preferences is attracting an increasing amount of research in experimental economics. Recent studies show that the effect of self-selection does not seem to be much of a concern. There is, however, a difference in the behavior of subjects from different populations. For example, university students, and especially economics students, have been found to be less pro-social than adult non-students. This indicates that sound generalizations should be based on

continued scrutiny of behavioral patterns among different populations. For example, Slonim et al. (2013) discuss how to do this carefully. Among other things, there is a need for more experimental evidence from non-Western populations, because Western populations are just a small fraction of the total population of the world. This discussion is partly related to the longstanding debate on the relationship between culture and economic outcomes. The equality vs. efficiency debate points to similar questions. What factors affect individual's relative preferences toward equity and efficiency? Which of these factors are individual-specific (e.g., explained by gender, income level, age etc.) and which are social (based, e.g., on culture, nationality, etc.)? We contribute to these debates by testing the role of social preferences in a subject pool of subsistence farmers and university students in Ethiopia. By replicating the ES and FNS experiments on the relative importance of equality and efficiency, we compare our results with that of different subject pools in Germany and Switzerland.

Our results show that inequality aversion or Maximin preferences, or both, dominate efficiency motives, more so among farmers. While we found statistically significant differences between the behaviors of Ethiopian farmers and economics students from Germany, Ethiopian farmers behave strikingly similarly to non-economics students and non-students from Berlin, Zurich and Munich. Considering the huge economic and cultural differences, these results indicate that the average person is more likely to be motivated by inequality aversion than by efficiency concerns. Moreover, our results further indicate that subjects with economics training are poor representatives of average behavior, at least when it comes to the trade-off between efficiency and inequality aversion. An important direction for future research on homogeneity of social preferences, and hence on the possibilities of generalizing findings, is to better understand whether the differences observed between locations are mainly driven by population-specific factors, as suggested in this paper, rather than by social or cultural factors that persist even after controlling for population-specific factors.

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Paper VII

Cooperative Preferences in Teams*

Peter Martinsson[1] Haileselassie Medhin[1]
Ferdinand M. Vieider[2]
[1]University of Gothenburg [2]Social Science Research Center Berlin (WZB)

Abstract

This paper experimentally examines the effect of team decision-making on cooperative behavior by using a public goods experiment. We find that teams are more likely than individuals to be free-riders, in line with other empirical findings showing that teams are more selfish. We also find that individuals learn free-riding from their team decision experience.

Keywords: Public goods, conditional cooperation, teams.
JEL Classification: C91, D03, D64.

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1 Introduction

During the last decade, experimental evidence has provided substantial insights into how individuals make voluntary contributions to public goods (e.g., Chaudhuri, 2011). In many situations, however, decisions on how much to contribute to a public good are not made by individuals, but rather by teams. Examples of this include households' decisions on transportation and recycling, or work teams and advisory boards making suggestions on environmental issues. While there has been long-standing interest in comparing individual and team decisions in social psychology[1] (e.g., Levine and Moreland, 1998), the issue is relatively new in economics. In general, experimental results have indicated that teams are more rational agents than single individuals, i.e., decisions by teams are closer to the behaviour predicted by *homo economicus* than decisions made by individuals (for an overview, see, e.g., Charness and Sutter, 2012; Kugler et al., 2012). To provide a better understanding of why teams make more rational decisions than individuals, Charness and Sutter (2012) provide three potential explanations: (i) multiple brains enable teams to attain better cognitive sophistication and make fewer mistakes than individuals, (ii) communication and discussion during team decision-making increase strategic thinking, and (iii) team decision-making creates in-group and out-group effects, which tend to make teams less concerned with the well-being of people outside their own team. These explanations would then suggest that teams are less likely to contribute to public goods, compared to individuals.

The objective of this paper is to systematically compare individual and team contributions to a public good. We present experimental results from a within-subject design where each subject participates in two one-shot public goods experiments, one where the individual decides alone and another where the individual decides as a team member. By explicitly testing for order effects, we also investigate how prior exposure to team decisions affects individual cooperative preferences and vice versa. We apply the design of public goods experiments developed by Fischbacher et al. (2001), allowing for the classification of subjects into different cooperation types, including free-riders and conditional co-operators.

[1] The results from this strand of literature are mixed. Findings in social psychology also indicate that teams are more competitive and selfish when interacting with other teams (e.g., Wildschut et al., 2003).

2 Experimental Design

In our public goods experiment, we have two different *provision units*: individuals and teams. In the case when the individual is the provision unit, each individual is part of a group of three people, but decides individually how much to contribute to a public good. This is the standard case in public goods experiments. In the other treatment, the provision unit is a team. A team consists of three people, and has to make a joint decision on how much to contribute to the public good in a situation where the group consists of three teams.[2] Each subject in our experiment made decisions both individually and as member of a team, with some subjects making the individual decision first, while others made the team decision first. We used stranger matching, with no feedback on decisions during the experiments, which was clearly stated in the instructions.

Each provision unit – individual or team – received an initial endowment of 20 tokens. In the team decision, each team sat down together in a face-to-face group meeting and agreed on a common decision after a discussion.[3] We choose the meeting format because this is the commonly used approach when teams make decisions in the real world (compared to anonymous decisions, for example). The exchange rate from tokens to the local currency was adjusted to keep stakes constant between individual and team decisions. Specifically, each token in the individual decision gave X units of local currency, while each token in the team decision gave 3X units of local currency to be shared equally among team members. Thus, the only difference between the two treatments was that team members had to agree on the same decision, while the stake size for each individual remained constant.

Our experiment builds on the design by Fischbacher et al. (2001). Each provision unit makes both a conditional and an unconditional contribution to the public goods. In the *unconditional contribution*, each provision unit i decides how much to invest in a public good, c_i, replicating a standard public good experiment. The contribution to

[2]While close synonyms in daily use, the terms 'group' and 'team' have distinct uses in our paper. 'Team' indicates a provision unit which consists of three individuals who make one common decision together, while 'group' indicates a collection of provision units (in our case, either three individuals or three teams) who can contribute to a public good.

[3]For a discussion on team construction and team identity, see, for example, Sutter (2009), who finds that even very weak constructions such as anonymously created teams in a laboratory evoke feelings of team identity.

the public good was an integer number that satisfies $0 \leq c_i \leq 20$ and the marginal per capita return equals 0.5. Thus, the pay-off function for provision unit i is

$$\pi i = 20 - c_i + 0.5 \sum_{j=1}^{3} c_j.$$

From the perspective of a provision unit, free-riding, i.e., zero contribution, is a dominant strategy. However, from a social perspective, it is optimal that each provision unit contributes its full endowment.

To elicit the conditional contributions, the strategy method is used. Each provision unit is requested to fill in a *conditional contribution table* stating how much they would like to contribute to a public good, conditional on each of the possible average contribution levels of the other members of their group (rounded to the nearest integer). The conditional contribution table is then used to classify provision units into broad classes of contribution types using the same criterion as in Fischbacher et al. (2001): *free-riders* (with a 0 contribution independently of others' average contribution), *conditional cooperators* (contribution increasing with others' average contribution), *hump-shaped* (contribution increasing with others' average contribution up to some point and then falling), or *others* (those who cannot be categorized into any of the three categories).[4]

Both the unconditional and conditional decisions are incentive compatible. In each group, two of the three provision units are randomly selected to be paid based on their unconditional contribution. For the third provision unit, a conditional contribution table is used to determine the payoff by taking the average of the unconditional contributions of the other two provision units, and matching this figure with the stated conditional contribution for the third provision unit.

We conducted a paper-and-pencil experiment among 180 students in Addis Ababa University, Ethiopia, where 84 participated in the individual-team treatment and the rest participated in the team-individual treatment. Each subject was paid a show-up

[4]To identify conditional cooperators, the conditional contribution data of each provision was mapped against the range of possible integer contributions from 0 to 20. A given provision unit is classified as a conditional cooperator if conditional contribution increases monotonically. Only conditional contributions with Spearman's rank correlation coefficient significant at 1% qualified as conditional cooperators. Conditional contributions with inverted U shape plots were classified as hump-shaped. 'Others' are those that couldn't be classified as either type.

fee of 24 Ethiopian Birr. One token gave 2 Birr in the individual decision, while the corresponding figure in the team decision was 6 Birr, i.e., 2 Birr for each team member.[5]

3 Results

Table 1 presents the classification of conditional contribution types from our experiment, together with results from some previous experiments using the same design with individual provision units. Looking only at our individual-decision results, we generally find a lower fraction of conditional cooperators and a higher fraction of free-riders compared to previous studies. The proportion of free-riders is higher in teams than among individuals, both for the first decision (40.6% vs. 33.3%) and for the second decision (64.3% vs. 40.6%), but the difference in proportions is only significant in the latter case (*two-sample proportion test p-values are 0.231 and 0.013, respectively*). The proportion of conditional cooperators is similar between provision units and order of decisions, while the proportion of 'others' is very low, except when an individual decision is made first. All in all, the results in Table 1 show that team decisions are more rational than individual decisions. Moreover, individual decisions made after team decisions result in much less frequent occurrence of the type 'other' (20.2% vs. 6.3%).

Table 1. Type Classification

		Free-riders	Conditional co-operators	Hump-shaped	Others
Current study, first decisions	Ind.	33.3%	29.9%	16.7%	20.2%
	Team	40.6%	28.1%	28.1%	3.1%
Current study second decisions	Ind.	40.6%	29.2%	24.0%	6.3%
	Team	64.3%	28.7%	7.1%	0.0%
Fischbacher et al. (2001)	Switzerland	29.6%	50.0%	13.6%	6.8%
Kocher et al. (2008)	US	8.3%	80.3%	0.0%	11.1%
	Austria	22.2%	44.4%	11.1%	22.2%
	Japan	36.1%	41.7%	11.1%	11.1%
Herrmann and Thöni (2009)	Russia	6.3%	55.6%	7.5%	30.6%

[5]The PPP exchange rate at the time of the experiment was 1 USD= 5.4 Birr.

In Table 2, we present the results from the unconditional contributions. On average, unconditional contributions are generally low. In detail, the results are consistent with those in Table 1 showing a higher proportion of free-riders among teams than individuals. But the difference is statistically significant only when both decisions are made first (*two-sample proportion test, p-value=0.055*), pointing to the fact that the team-decision experience narrows the gap between individual decisions and team decisions. This is also manifested by the finding that there is a significantly higher proportion of free-riding among individuals with team-decision experience compared to situations without team decision experience (54.2% vs. 42.9%) *(two-sample proportion test, p-value=0.065*). Thus, subjects seem to learn to contribute less than they would have if they were not exposed to team decisions first. However, the average contributions among non-free-riders are not significantly different between teams and individuals, and do not differ significantly based on the order of decisions.

Table 2. Unconditional Contributions

Order	Provision unit	Proportion contributing zero (i.e., free-riders)	Average contribution of non-free-riders	Overall average contribution
First decisions	Individual	42.9%	5.92	3.38
	Team	59.4%	5.54	2.25
Second decisions	Individual	54.2%	5.11	2.34
	Team	64.3%	3.90	1.39

Because we have a within-subject design, we can compare how subjects are classified in terms of contribution types between individual and team decisions. For example, from the team-individual decision order, we have information on how the team decision affected the decisions that individuals made on their own, and vice versa for the individual-team decision order. Looking into such data in detail could shed some light onto how the team decision experience affects individual behavior, and vice versa. For example, we can compare the relative 'bargaining power' of individuals of different types in determining team type. One way to look at this is to construct a transition matrix that maps the distribution of types in the first and second decision. Figure 1 and Figure 2 illustrate the result of such mapping for the individual-team and team-individual orders, respectively.

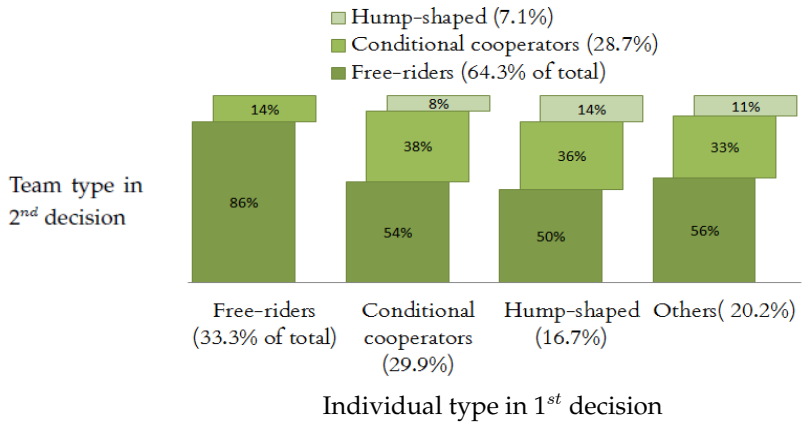

Figure 1. From individual type to team type

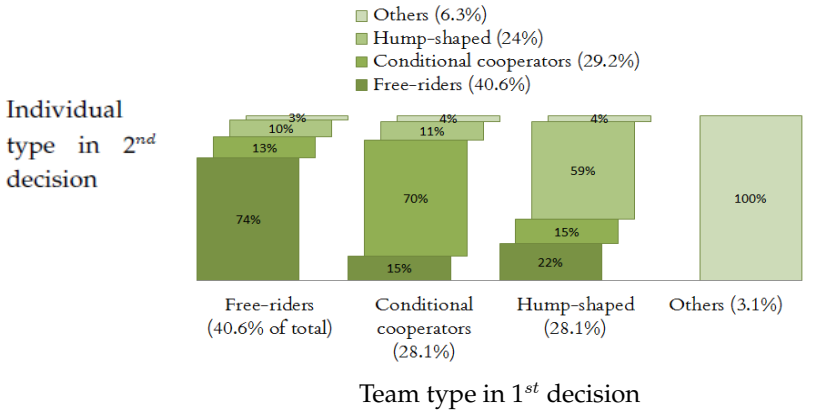

Figure 2. From team type to individual type

As can be seen from Figure 1, 85.9% of the subjects who were free-riders in individual decisions end up being members of teams which decide to free-ride. On the contrary, only 38% of conditional cooperators end up in a team that is also a conditional cooperator, with 54% of them happening to be members of a free-riding team.

Similarly, 50% hump-shaped become members of a free-riding team. That is, conditional cooperators are far more likely to be classified as free-rider types in team decisions, rather than free-riders becoming members of a conditional cooperating team. To explore further the result that free-riding individuals seem to dominate in team decisions, we also looked at the composition of each team vis-à-vis the team type. All teams with more than one free-rider individual in the first decision end up being free-rider teams. Moreover, of the teams composed of one free-rider and two non-free-riders, 64% end up as a free-riding team. These results affirm that free-riders are more influential in shaping team type than conditional cooperator or hump-shaped type individuals. Free-rider individuals are more likely to 'convert' conditional cooperators than vice versa.

Figure 2 illustrates the mapping of types when the team decision is made first. Three out of four individuals who were on a free-riding team also become free-riders, confirming the persistence of free-riding. Hence, not only are free-riders more influential in team decisions, their influence also persists in the individual behavior of their team members. Figure 2 also shows that individuals who come from a conditional cooperator and hump-shaped team are likely to maintain their type. This could be because individuals generally maintain their team type. Given the insights from Figure 1, it could also be the case that non-free-rider teams were dominated by non-free riders.

4 Conclusion

We have experimentally compared individuals to teams as provision units for contributions to a public good. We find that contributions by teams are closer to free-riding behavior than contributions made by individuals, which confirms previous findings that teams are more selfish (e.g., Charness and Sutter, 2012). Our results show that there are important interactions among the two decisions, both in how different contribution types affect each other in team decisions, and in what individuals learn from the team decision experience. Free-riding individuals seem more influential than conditional co-operators in team decisions. A possible explanation for the team-individual difference could be that the strategic thinking of free-riders gets more attention during team discussions. The finding that individual free-riding increases after the team

decision experience further strengthens the argument that the 'promotion' of strategic thinking could be the main driver in team rationality. But of course other factors (e.g., in-group, out-group effects) could also have played a role in creating the differences in behavior. A more complex design is needed to disentangle these effects.

By and large, our results show that we should be careful when generalizing findings on individuals' contributions to public goods in a team setting, which is the situation where a vast majority of contributions decisions are made. This may have important implications for understanding many real-world decisions, from decisions by business teams to climate change negotiations, and also how individuals' decisions are affected by advice and peer effects. These issues should be addressed in future research.

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