Managing behaviour: An experimental study into the behaviour of Tanzanian forest users

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Master Thesis 30 credits 2013

ABSTRACT

As the dire consequences of tropical deforestation and forest degradation are recognised at both local and global levels, a myriad of measures to mitigate the problem is proposed. Despite these measures affecting local forest users in their daily interaction with forest resources, their actual impact on behaviour is not thoroughly researched. One reason might be the difficulty of assessing the measures' *ceteris paribus* effects on behaviour.

The random and controlled attributes of economic experiments allow researchers to observe causal relationships in the behaviour of the participants. A common criticism, though, refers to the generalizability (external validity) of findings in abstract laboratory experiments. A valid experimental study therefore needs to acknowledge the relevance of field context in the experimental design. Through an experimental study in Tanzania consisting of 36 field experiments with 288 participants, this thesis aims to contribute to both the advancement of experimental economics and to the forest management literature.

Measures to mitigate deforestation and forest degradation can be classified by the intended mechanism to reduce forest use. The thesis assesses the impact of three management regimes: command and control (CAC), payment for environmental systems (PES), and community forest management (CFM). The regimes are imposed as treatments in the field experiments.

The experimental study finds that the regimes' impact on the behaviour of the participants varies, and that individual and group characteristics affect the impact of the regimes; as well as general behaviour. Overall, the CFM regime reduces forest use the most, while the CAC regime reduces forest use the most among women and older participants. The PES regime reduces forest use slightly more than open access, at best.

In terms of characteristics, women are more aggressive harvesters than men are, but including more women in the group at the same time decreases the groups' aggregate harvest. Younger participants are more aggressive harvesters than older participants are, but the effect of older participants on others is negligible. Ethnic group heterogeneity has an ambiguous effect on behaviour.

The results indicate that the choice of regime matter for the impact on the behaviour of forest users, and that the impact varies with the characteristics of the forest users and their community. Therefore, the thesis argues that field experiments provide an essential method for *ex-ante* impact assessment of planned forest management.

ACKNOWLEDGEMENTS

First and foremost, thank you, Arild Angelsen, for your time and efforts. Even without the counterfactual I am certain that your supervision before, during and after my fieldwork was crucial for the presented result. Also, thank you, Lars Olav Eik and the CCIAM programme for providing the funding necessary for the empirical study. Furthermore, Professor Kessy and Professor Ngaga at Sokoine University of Agriculture, thank you for navigating me through the ocean of bureaucracy, and for staking out the course to the field.

Fellow fieldworkers, Donatha and Lucas, thank you for making the data collection possible. Donatha, your strict command and control regime along with an eye for detail proved to be the perfect measure for managing the experiments. And Lucas, *Bwana*, your communication treatment was vital for the experiments being completed at all.

Pastory, Nzunda, Charles Thomas, Nyerere, Lalika, Elyimo, and others who helped arrange for the experiments in the villages: *Asante sana*. We would literally have been lost without you.

Thank you, Zlata and Ingrid, for valuable comments. And, Kari-Anne, thank you for always caring, and for enduring months of my babbling about experiment-this and experiment-that.

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LIST OF ABBREVIATIONS

CAC	Command and control
CBFM	Community based forest management
CCIAM	Climate Change Impacts, Adaptation and Mitigation
CFM	Community forest management
CIFOR	Center for International Forestry Research
COP	Conference of the parties
CPR	Common pool resource
ES	Environmental system
FAO	The Food and Agriculture Organization
INA	The Department of Ecology and Natural Resource Management at the Norwegian university of life siences
IPCC	The Intergovernmental Panel on Climate Change
JFM	Joint forest management
NP	National park
OA	Open access
OLS	Ordinary least squares
PES	Payment for environmental systems
PFM	Participatory forest management
PREM	Poverty reduction and environmental management
REDD	Reducing emissions from deforestation and forest degradation
REDD+	Reducing emissions from deforestation and forest degradation and enhancing forest carbon stocks
SUA	Sokoine University of Agriculture
TAF	Tanzania Association of Foresters
TCMP	Tanzania Conservation and Management Project
TNRF	Tanzania Natural Resource Forum
TZS	Tanzanian shilling
UN	United Nations
URT	United Republic of Tanzania
USAID	United States Agency for International Development
USD	United States dollar
WCA	Wildlife conservation area

1 INTRODUCTION

An individual harvesting from a shared forest faces a social dilemma. On the one hand, harvesting forest products increases the individual's private welfare. On the other hand, harvesting the products decreases the potential welfare of the other beneficiaries of the forest. The dilemma thus concerns the extent in which the individual's decision-making process incorporates the negative externalities the harvest imposes on others. Consequently, understanding the behaviour of forest users, along with the factors influencing their behaviour, is necessary to understand their propensity to incorporate the externalities of their actions.

Tanzania experiences high rates of deforestation and forest degradation, creating negative externalities at both local and global levels (FAO 2011). Measures to mitigate the problem have accordingly been suggested from both the Tanzanian government and others. From focusing merely on central command and control of forest use, the Tanzanian government has in the last 20 years increasingly decentralised the responsibility to the community level (URT 2012). Furthermore, foreign donors in collaboration with NGOs have introduced a third measure: rewarding forest users for reducing their CO_2 emissions by decreasing forest use (Angelsen et al. 2012). The measures aim to increase the propensity of the forest users to incorporate the negative externalities of their actions in their decision making process. As such, forest management is also management of behaviour.

Surprisingly, research and implementation of the measures largely elude the behavioural aspect of forest management. One reason might be the difficulty of assessing their *ceteris paribus* impact on behaviour. In simple terms, cross-sectional comparisons suffer from unobservables, while longitudinal studies suffer from selection biases. Randomly imposing measures would provide valid data, but the method is costly, politically infeasible, and might be subject to moral criticism. Fortunately, advancements within experimental economics provide a reliable method to explore the issue. By simulating the social dilemma in an experiment with high internal and external validity,[1] the *ceteris paribus* effect of the participants' characteristics, and the imposed treatments, on behaviour is observable.

The social dilemma has been explored in abstract laboratory experiments. For instance, in Ostrom et al. (1994) American undergraduate students perform abstract tasks simulating a social dilemma in the laboratory. At the same time, the authors argue that the specific characteristics of the good, the community and the population of interest should be considered when analysing the dilemma. Thus, a valid experiment studying the social dilemma should include the characteristics of the particular dilemma and the population of interest.

This thesis explores the impact of the mentioned regimes on the behaviour of Tanzanian forest users, as well as the role individual and group characteristics have in determining behaviour and the effectiveness of the regimes. By incorporating field context in good, task and setting in experiments conducted with 288 rural Tanzanians, the thesis aims to contribute to both the continued development of the experimental method and to the forest management literature.

The research questions addressed in this thesis are: (1) does the experiment possess sufficient external and internal validity? (2) How does the game structure, simulating the management regimes, affect the forest use? (3) How do individual and group characteristics affect the forest

[1] Concepts explained in sub-subsection 3.3.3.

use? Based on existing literature and previous empirical studies, hypotheses testing the research question will be postulated.

The structure of the thesis is as follows: section 2 presents the forest situation in present Tanzania, along with a brief review of the forest management history of the country, summarised by a taxonomy of the undertaken measures. Section 3 stakes out the literature relevant for the thesis. The thesis is founded upon the common pool resource literature (and in particular the works of Elinor Ostrom), game theory, and the growing literature in experimental economics. This section also explains the thesis' research questions and the hypotheses postulated to answer the questions. Section 4 describes and discusses the methods applied in the thesis' empirical study, in relation to the theory of the previous section. Section 5 presents and discusses the results in the order of the hypotheses, and discusses implications of the findings. Section 6 concludes the thesis.

2 BACKGROUND

2.1 TANZANIA'S FOREST RESOURCES

Forests are an important part of rural and urban livelihoods in Tanzania. About 80% of Tanzania's population depend on agriculture and natural resources for their livelihoods (TNRF 2009). On national level, forestry gave USD 5.5 million in government revenues in 2004, and it officially contributed to 2-3% of GDP and to 10-15% of total export earnings (World Bank 2008). More importantly for this thesis, though, is the World Bank estimate that unaccounted-for services and non-industrial forestry alone contribute 10-15% to GDP. Furthermore, forests provide 75% of all building materials in Tanzania, 100% of indigenous medical plants and supplementary food, and fuels 95% of Tanzania's energy consumption (World Bank 2008).

Currently, Tanzania has 35.3 million hectares of forest land, constituting about 40% of the total land area (URT 2012). The annual loss of forest area has been estimated by URT (2012) to be 412 000 ha in the period 2000-2005, while FAO (2011) calculates the annual loss to be 403 000 ha in the period 1990-2010. Thus, on average about 1.15% of the forest land is lost every year. In absolute terms, Tanzania experienced the second highest loss of forest area in Africa in the period 2000-2010; surpassed only by Nigeria (FAO 2011).[2]

Loss of forest land, loss of forest cover, deforestation, and forest degradation are terms that often vary in their definitions and are subject to questionable use (Schoene et al. 2007). I will in this thesis refer to the loss of forest land as deforestation, and loss of forest cover as either deforestation or forest degradation. Deforestation converts forests to some other land use or reduces tree canopy cover to below a certain threshold in the long term, while degradation decreases the supply of forest products and services by negative changes *within* the forest (FAO 2001). Distinguishing between deforestation and forest degradation is not vital for this thesis.

2.2 DRIVING FORCES

Causes for the loss of forest cover can be examined at different levels, cf. Angelsen (2010). First, the characteristics and activities of the agents utilising the forest can be examined. Second, the *immediate causes* can be examined by looking at the external factors influencing the choices of the agents. Third, the *underlying causes* affecting the immediate causes can be examined.

The driving forces of forest cover loss are often complex and vary not only between countries, but also within countries (Hosonuma et al. 2012). Still, Blomley & Iddi (2009) identify the reliance on fuelwood for energy consumption as a major immediate cause for the loss of forest cover in Tanzania. This is supported by World Bank's (2008) estimate that fuelwood constitutes 95% of the energy consumption of Tanzania.

Fuelwood is mainly harvested for household use in Tanzania (Johnsen 1999). The harvesting could be by collecting dead branches and trees, or by removing living branches and trees. The focus of the thesis is on the latter. Fuelwood can furthermore be separated by firewood and charcoal, where firewood is unprocessed wood and charcoal is processed wood (Johnsen 1999).

[2] FAO's (2011) "Forest area" term does not consider forest quality and density. This might be relevant as large parts of Tanzania consist of low density *miombo* woodlands (Blomley & Iddi 2009). In terms of carbon stock, three African countries experience higher absolute loss than Tanzania (FAO 2011).

Firewood constitutes the largest share of the fuelwood consumption in Tanzania, particularly in rural areas (World Bank 2008).

Fuelwood gathering can be seen as an externality as one individual's harvesting of fuelwood removes the possibility of others benefitting from the same trees. The potential benefits can come through direct utilisation of the trees, e.g. as fuelwood and honey production (Johnsen 1999), or through environmental services, e.g. as flood protection and carbon sequestration (Angelsen & Rudel 2013). The negative externalities are thus found at both local and global levels. Subsection 3.2 further elaborates on the externality and the social dilemma it creates.

Identifying the agents for the firewood harvesting and charcoal production is perhaps even more complex than identifying the causes (World Bank 2008; FAO 2011). Local subsistence harvesting of forest products is often neglected in analysing the loss of forest cover, as identifying and quantifying the dispersed use is difficult (Blomley et al. 2011; FAO 2011). Local subsistence reliance on these forest products is, however, widely recognised (Blomley et al. 2011; Johnsen 1999; FAO 2011). Therefore, on the assumption that harvesting fuelwood creates negative externalities, subsistence use of local forest resources is a driving force for deforestation and forest degradation in Tanzania. As will be shown in sub-subsection 4.1.2, 96% of the sampled individuals for this thesis regularly harvest forest products from their local forests.

A recognised underlying cause of deforestation and forest degradation in Tanzania is the open access situation that characterises large parts of the forest land (Zahabu et al. 2005; URT 2012; Blomley & Iddi 2009). Open access refers to a situation where no person or entity has the right or the capacity to exclude others from benefitting from the resource (Ostrom 2006; FAO 2001). The situation has long been applied as a contributor for non-optimal resource use, cf. Gordon (1954).

The focus of this thesis is consequently on local forest users in Tanzania as the agents, with the harvesting of forest products for energy as the immediate cause and the open access situation as an underlying cause.

2.3 A BRIEF HISTORY OF TANZANIAN MEASURES

2.3.1 THE LAST HALF OF THE 20TH CENTURY

As a result of the process towards self-governance, the first forest policy of Tanzania, then Tanganyika, was introduced in 1953 (URT 2012). The policy sat visions for the forest management of the soon-to-be independent country, but lacked legal measures to enforce them (URT 2012). Additionally, the Forest Legislation of 1957 was imprecise in how to monitor the forests outside state ownership and were therefore also inadequate in dealing with the growing problem of deforestation (URT 2012).

Later, the Arusha Declaration of 1967 announced nationalisation of all "major means of production and exchange" (Nyerere 1968:234). The declaration specifically mentioned forests, and effectively transferred all forest land to central government control. Simultaneously, the process of "villagisation" (*ujamaa*) was undertaken which, among other things, resettled people into planned village settlements and established village councils to legally manage the interests of the villages (Zahabu et al. 2009; Blomley & Iddi 2009). The process also aimed at creating "a Tanzanian identity that cut across ethnic lines" (Stöger-Eising 2011:137).

Since the mid-1980s Tanzania has been through a process of partial decentralisation of forest management (URT 2012; Zahabu et al. 2009). The process can be seen in context with the 1985 presidential change and the subsequent change in the country's economic and social direction (USAID 2011). Recently, a more market-based forest management approach is also being tested, especially visible in Norway's international climate and forest initiative (Angelsen et al. 2012). In collaboration with government agencies, NGOs and the UN, projects are being developed to compensate forest beneficiaries for decreasing deforestation and forest degradation (UN-REDD Programme 2009; Angelsen et al. 2012).

The forest management history of Tanzania shows that a wide range of measures has been tried to combat unsustainable forest use. The results have been mixed (Zahabu et al. 2009). Furthermore, during the same time period, Tanzania's population has increased from 12.3 million in 1967 to about 45 million in 2012 (URT 2013), increasing the need for clearly defined forest management (URT 2012). To further understand the complexity and variety in Tanzanian measures to manage deforestation, the following sub-subsection gives a brief account of the current land ownership laws in Tanzania.

2.3.2 PRESENT TANZANIAN LAND AND FOREST

The Land Act No. 4 of 1999 divides Tanzanian land in three categories: reserved land, village land and general land (Blomley et al. 2011). The act concerns the management of reserved land and general land, while the Village Land Act No. 5 of 1999 deals with village land (Zahabu et al. 2009). Since some reserved land might be found within village land there is some overlap between the two acts. In addition, reserved land is part of the Forest Act, National Parks Ordinance, Wildlife Conservation Act, and Town and Country Planning Ordinance (Zahabu et al. 2009).

Reserved land is assigned with a specific purpose by the national government, one purpose could be nature conservation through nature reserves (Blomley et al. 2011). Village land on the other hand is under the direct control of village governments. The last category of general land is more complicated in terms of responsibility. The 1999 Village Land Act defines general land as all land that is not reserved land or village land, while the 1999 Land Act adds that general land also includes unoccupied and unused village land. The latter definition does not specify what is meant by "unused" or "unoccupied". Attempts to quantify the amount of land in each category therefore vary substantially in their estimates. Table 1 gives an overview of the estimates commonly found in the literature. The Tanzanian Ministry of Land's estimations for unreserved land and therefore also unreserved forests, however, are far smaller than the ones presented in Table 1; as more land is regarded under village control (Blomley et al. 2011).

TABLE 1 DISTRIBUTION OF FOREST LAND AREA IN TANZANIA BY USE AND OWNERSHIP

Ownership	Productive area[a]	Protective area[a]	Total area[a]
Declared forests			
Local authority	1 356 204	231 470	1 587 675
Central government	9 292 845	2 986 862	12 279 707
WCA and NP[b]		2 000 000	2 000 000
Private forests	20 5476	23 188	43 736
Village forest			2 345 500
Subtotal	10 669 597	5 241 521	18 256 618
Proposed forest reserves			
by local authority	64 019	102 559	166 578
by central government	352 557	443 367	795 924
Subtotal	416 576	545 926	962 502
Unreserved forests			16 037 880
Total	11 086 173	5 787 447	35 257 000

Notes: [a]areas reported in hectares. [b]Wildlife conservation areas and national parks.
Sources: Malimbwi (2002) and Zahabu et al. (2009)

Productive areas constitute about 11 million ha. In this area, controlled harvest of for instance fuelwood and timber is allowed, whereas in protective areas, constituting about 5.2 million ha, harvesting is illegal (Zahabu et al. 2005). Declared forests are areas that have formally been annexed such that ownership and responsibility are delegated by legislation, while proposed forest reserves are areas that are in process of becoming declared.

The unreserved forests and currently the proposed forest reserves constitute the general land category. According to Table 1, the categories covers about 48% of all forest land in Tanzania, making almost half of the country's forests "open access" (Zahabu et al. 2005). These forests are subject to far more deforestation than the declared forests, as mentioned in subsection 2.2. As a result, the Tanzanian National Forest Policy of 1998 aims to "ensure sustainable supply of forest products and services by maintaining sufficient forest area under effective management and enhance national capacity to manage and develop the forest sector in collaboration with other stakeholder" (URT 1998:14). "Effective management" could take a variety of forms, and produce ditto results.

2.4 A TAXONOMY OF FOREST MANAGEMENT REGIMES

The following three sub-subsections constitute a taxonomy of the forest management regimes being used as measures to limit the loss of forest cover in Tanzania. The taxonomy separates the forest management regimes by the underlying idea of the regime, revealing the intended mechanism to reduce deforestation and forest degradation.

2.4.1 COMMAND AND CONTROL

Perman et al. (2003) broadly refer to command and control (CAC) as measures exercising direct control over agents. Angelsen & Rudel (2013) describe the CAC forest management regime as a direct regulatory policy that imposes restrictions on forest use. They focus on protected areas, but according to the definition, the regime can be imposed less restrictive as well, e.g. in the productive areas referred to in Table 1.

In this thesis, I refer to CAC as a forest management regime regulating the behaviour of forest users by deterrence – i.e. where an authority defines legal and illegal forest use, monitors the use, and punishes violators. In Tanzania, CAC has historically been the main measure for decreasing the loss of forest cover, and is still today widely enforced (URT 2012; Van Beukering et al. 2007).

2.4.2 Payment for environmental systems

In reference to Angelsen & Rudel (2013:104), payment for environmental systems (PES) aims to increase the capturing of the "protective rent" of the forest by the local users or owners. By increasing the payment for the forests' carbon services, for instance, the incentive for protecting the forest is strengthened. There is no formal definition for PES (Wunder 2005). Still, five criteria may be used to describe the principle: voluntariness, well-defined environmental systems (ES), a buyer and a provider of ES, and conditions (Wunder 2005). PES accordingly involves the voluntary selling of well-defined ES from at least one provider to at least one buyer, on the condition that the ES is securely provided.

As mentioned in 2.1, forests provide vital ES at a local and national level in Tanzania. In addition, the Intergovernmental Panel on Climate Change (IPCC) estimates that deforestation and forest degradation make up 17.3% of global anthropogenic greenhouse emissions (Barker et al. 2007). Deforestation and forest degradation are therefore not only a local or national problem, but a global problem. Potential buyers of forests' ES should accordingly be found at every level.

At the global level, reducing emissions from forest degradation and deforestation (REDD) aims at rewarding individuals, communities, projects and countries that reduce their emissions of greenhouse gases in relation to forests (Angelsen et al. 2012). The term was endorsed at COP13 in 2007. A plus was later added to the term to also include enhancement of forest carbon stocks. REDD+ has generated attention and funds at global level and is seen as a relatively quick and cheap measure to limit global warming. Norway pledged USD 2.6 billion over five years at COP13 and is the most prominent REDD+ donor, with Tanzania as one of the four main recipients (Angelsen et al. 2012).

2.4.3 Community forest management

The level of obtainable forest rent does not only depend on the level of PES, but also on the forest's property right (Angelsen & Rudel 2013). In community forest management (CFM), as defined by Agarwal & Angelsen (2009), the property right is set at the community level. Thereby, the potential forest rent can be captured at the community level through increasing the incentive for sustaining the forest. The regime is thus a common-property regime, where a group of resource users share rights and duties to a resource (McKean 2000).

In Tanzania, The National Forest Policy of 1998 was part of a shift from focusing on merely centrally exercised CAC to more community based management (URT 2012). The policy argues that satisfying the needs of the people, decentralising control over resources, and securing tenure arrangements are important to achieve long term environmental protection (URT 1998). The strategy has later been termed participatory forest management (PFM) (Blomley & Ramadhani 2006).

PFM in Tanzania takes one of the two forms: community based forest management (CBFM) and joint forest management (JFM) (Blomley et al. 2011). CBFM concerns land under the Village Land Act of 1999 and therefore shifts clear ownership and management responsibilities to rural communities. In the JFM approach on the other hand, ownership and responsibilities are shared between the government and local communities.

CFM, and CBFM in particular, is generally believed to improve forest management in Tanzania (Blomley et al. 2011; Wily & Dewees 2001), but the management regime has also been criticised for having problems associated with poor governance at the community level (Zahabu et al. 2009). Brockington (2007) in closely studying the local institutions of a particular Tanzanian village found corruption as a severe stumbling block for the success of community management.

The presented taxonomy broadly classifies the measures taken in Tanzania to reduce the country's severe loss of forest cover. CAC, PES and CFM vary fundamentally in the underlying ideas and are consequently divisible, despite great variation within each forest management regime. The empirical study of the thesis, presented in section 4, utilises this taxonomy.

3 THEORETIC FOUNDATION

The focus of the thesis is forest characteristics, the behaviour of forest users, and possible measures to change the behaviour. While sub-section 2.4 presented a taxonomy of measures, the following three subsections present and discuss the other two issues. Subsection 3.1 categorises forests as a good, while subsection 3.2 presents a framework for analysing the behaviour of forest users. Subsection 3.3 discusses how to observe forest users' behaviour, before subsection 3.4 presents a review of relevant findings in the experimental literature. Finally, subsection 3.5 concludes and presents the thesis' hypotheses.

3.1 THE FOREST AS A COMMON POOL RESOURCE

3.1.1 DEFINITION AND CLASSIFICATION

Ostrom et al. (1994), in defining a common pool resource (CPR), set up a general classification of goods using two attributes: excludability and subtractability[3]. The classification relates back to Samuelson (1954:387) and his distinction between private goods and "collective consumption goods". Ostrom et al. (1994) define excludability as the ease of limiting potential beneficiaries from consuming a good. Limiting could be a purely physical measure, such as a fence, but would also normally include institutions, such as enforced property rights. The possibility to exclude a potential beneficiary from a good thus depends on both the physical attributes of the good and the existing institutions.[4]

Subtractability is defined by Ostrom et al. (1994) as the extent the use of one beneficiary affects the possible uses by others. If subtractability is high, benefits are gained by subtracting from the good, thereby decreasing the size of the good and simultaneously decreasing the potential benefit others could gain from the good. The amount subtracted naturally also depends on the level of use of the beneficiaries, meaning that low levels of use can have a small impact on others, even with high subtractability. In Figure 1, four goods are categorised based on these two attributes. Common pool resources are characterised by difficulty of exclusion and high subtractability.

		Subtractability	
		Low	High
Exclusion	Difficult	Public good	CPR
	Easy	Club good	Private good

FIGURE 1 A GENERAL CLASSIFICATION OF GOODS. SOURCE: OSTROM ET AL. (1994:7)

A forest is consequently a common pool resource. The harvesting of a tree eliminates the possibility of someone else benefitting from the same tree, and limiting potential beneficiaries from harvesting requires effort.

3.1.2 THE FOREST'S CHARACTERISTICS

Ostrom et al. (1994) separate between two problems related to a CPR: the problem of *appropriation* and the problem of *provision*. In the appropriation problem, the relationship between the yield of the CPR and its inputs are given. The problem therefore relates to the

[3] "Rivalry in consumption" is an interchangeable term. I use "subtractability" as it is consistent with Ostrom et al. (1994).

[4] North (1990) defines institutions as rules that define and constrain human interaction. In this case, the rules would define and constrain the use of a specific good. This thesis uses the terms institutions and regimes interchangeably.

allocation of the yield, which might involve excluding potential beneficiaries. In the provision problem, the focus is on creating, maintaining and improving the production capability of a CPR, or avoiding the CPR's destruction (Ostrom et al. 1994). This problem thus refers to the efforts needed to support the resource.

A forest provides a limited amount of goods in a given time period. These goods are appropriated by beneficiaries, implying an allocation of the yield. Furthermore, a forest grows with a growth rate dependent on the surrounding supporting characters, e.g. water. These inputs could be provided by the same beneficiaries. Forest can consequently experience appropriation problems or provision problems, or both at the same time. The term *appropriation* will be used in the thesis to describe the withdrawal of goods from a CPR. When specifically concerning the appropriation a forest resource, the term *harvesting* will be used.

Cardenas et al. (2011) highlight the need to understand the dynamics of natural resources to be able to understand the behaviour of the beneficiaries. The size of a resource stock in time *t* depends on both the appropriation and the provision in time *t-1*. The size of a forest subsequently depends on previous harvesting of forest products and the growth rate of the forest. These characteristics need to be taken into account in any study of the behaviour of forest users.

3.2 BEHAVIOUR AND GAME THEORY

3.2.1 THE SOCIAL DILEMMA

The two potential problems related to a CPR arise due to the social dilemma. A social dilemma, as defined by Dawes (1980:169), is a dilemma where each individual maximizes his or her own utility by making a "socially defecting choice", but where the individuals altogether are better off if they were cooperating instead. It refers to the classical opposition of what is rational for an individual and rational for a group. Cardenas (2000), building on the theory of Ostrom et al. (1994), sets up a model depicting a dilemma where the well-being of household *i* depends on three factors related to a CPR: (1) the household's self-appropriated goods, either self-consumed or sold in a market, (2) the negative group externality from aggregate appropriation, and (3) income from non-forest related activities. Equation [1] describes this dilemma.

$$U_i\left(x_i \textstyle\sum x_j\right) = k\left[\gamma x_i - \frac{\phi(x_i)^2}{2} + \left(q^0 - \frac{\left(\sum x_j\right)^2}{2}\right) + w_i(e - x_i)\right]^{\eta} \qquad [1]$$

x_i is *i*'s appropriation effort and $\sum x_j$ is the aggregated effort by the group. *w* is the prevailing wage of labour, making $w_i(e - x_i)$ the payoff of providing $(e - x_i)$ units of labour. The private benefit of appropriation is given by $g(x) = \gamma x_i - \phi(x_i)^2/2$ with $\gamma, \phi > 0$ and $g(x) > 0$ for $x_i \in \{0, e\}$. Strict concavity describes the diminishing marginal return of spending time harvesting forest products. Finally, the group externality $q^0 - \left(\sum x_j\right)^2/2$, where q^0 is the forest without any harvesting, shows that as the negative externality increases, *i*'s utility will increasingly decrease. In total, [1] shows that as x_i increases, so does *i*'s payoff. At the same time, as $\sum x_j$ increases, *i*'s payoff decreases. This creates a CPR dilemma, where an individual's short-term private interest is in conflict with the long-term interest of the group.

3.2.2 PREDICTING CHOICES

Ostrom et al. (1994) stress that the problems related to the CPR dilemma are many and complex. These problems can be depicted in numerous ways, but the thesis' focus is on the dilemma related to the appropriation of the resource, and where the forest users face the mechanisms described in Equation [1]. This dilemma can be simplified to a two-player prisoner's dilemma game, where the two players choose between cooperating and defecting. Cooperating in the case of the CPR dilemma implies choosing to maximize the group's payoff, whereas defecting implies maximizing own payoff. Figure 2 shows how the players' payoff depends on the choice of both players.

		Player 2	
		Cooperate	Defect
Player 1	Cooperate	b,b	d,a
	Defect	a,d	c,c

FIGURE 2 PRISONER'S DILEMMA

In the prisoner's dilemma a>b>c>d. Assume (b,b) to be the social optimal outcome, i.e. where the players total payoff is maximized (2b>a+d). Any deviation from this outcome is thus decreasing total payoff. Then assume that the players belong to the species *homo economicus*[5]. If player 2 cooperates, player 1 would be better off defecting. If player 2 defects, player 1 would also be better off defecting; and vice versa. Defecting is thus the dominant strategy for both players, making (c,c) the Nash equilibrium. In this situation, each player gets a lower payoff by unilaterally changing his or her strategy.

Figure 2 depicts a one-shot two-player prisoner's dilemma game. A real life CPR dilemma on the other hand could involve more than two individuals, in repeated interaction. Increasing the group size of the game in Figure 2 to an N-player game would not alter the dominant strategy of the players, as the ordering of outcomes by payoff would remain the same. If the game is repeated, the two players could achieve a cooperative equilibrium given appropriate strategies.[6] But, free riding, i.e. defecting with the hope of others cooperating, becomes a more feasible alternative as the group size increases (Barrett 1999). A possible explanation is that the social outcome depends less on the action of one player, the more players participating.

Faced with a social dilemma, feasible behaviour for a rational and selfish utility-maximizer is thus to not consider the long-term interests of the group and defect, regardless of the others' choices. The prediction is in accordance with the arguments of Olson (1971), and even supported by Ostrom (2006:151):

When the resource units produced by a common-pool resource are highly valued and institutional constraints do not restrict harvesting (an open-access situation), individuals face strong incentives to appropriate more and more resource units leading eventually to congestion, overuse, and even the destruction of the resource itself.

[5] The origin of *homo economicus* is unclear, but the term could be traced back to John Stuart Mill (Persky 1995). Even though Mill's original "economic man" had several interests, I use the term *homo economicus* as a rational and selfish actor who aims at achieving the highest possible level of private utility, given the constraints he or she faces.
[6] See for instance Axelrod (1981) who presents the reciprocal tit for tat strategy

Ostrom thus also indicates a possible way to achieve the social optimal outcome and thereby "solve" the dilemma: deviate from the open-access situation.

3.2.3 Solving the dilemma

Ostrom et al. (1994) stress a coordinated strategy undertaken by the appropriators as the solution to the dilemma. A coordinated strategy in the CPR dilemma can be defined as determining "how much, when, where and with what technology to withdraw resource units" (Ostrom et al. 1994:16). The determination process could be a natural process where the group ultimately solves the dilemma; evolution-like, or it could be a more deliberate change of the local institution. In the latter case, Ostrom et al. (1994) describe the appropriators themselves making systems of monitoring and sanctioning to regulate the use of the CPR.

Subsection 2.2 argues that the harvesting of forest products by local forest users is an important immediate cause of the loss of forest cover in Tanzania. Furthermore, the subsection presents the open access situation as an important underlying cause for the forest users' overexploitation of the resource. Consequently, on an aggregated level, the communities lack coordinated strategies. Subsection 2.3 and 2.4 subsequently present measures to solve to the social dilemma. The measures aim to deviate from the open access situation. The question is therefore: which alternative to the open access situation is more efficient in changing forest users' behaviour? Sub-subsection 3.4.2 presents some studies examining the effect of institutions on behaviour, while the next subsection discusses advancements in the methods examining behaviour.

3.3 Observing behaviour

3.3.1 The experimental method

Despite being widely applied in other sciences, experimental methods have long been met with scepticism in economics (List 2011). In recent decades, however, experimental methods have gained a foothold in economics and are now rarely met with the same scepticism as before (List 2011). The trend is also visible in the number of published papers in experimental economics, whose fraction of the total number of published papers in economics more than doubled from 2003 to 2011 (Hamermesh 2013).

One appealing attribute of the experimental method is the possibility of creating credible counterfactuals, as the researcher is able to make the variable of interest exogenous through randomization. If y_1 is outcome with some sort of treatment and y_0 is outcome without the treatment, the effect of the treatment on individual *i* can be shown as $\tau_i = y_{i1} - y_{i0}$. The problem is, however, that the researcher is normally not able to observe the individual both in the treated and untreated state, i.e. τ_i is unknown. Harrison & List (2004) claim that controlled experiments are the most convincing solution for the missing counterfactual problem. In the experiments, a control group can be created through randomization. Identifying the average treatment effect of the population is therefore possible: $\tau = y_1^* - y_0^*$, where y_1^* and y_0^* are the average outcomes with and without treatment.

Another benefit of experimental method, mentioned by Holt (2006), is the possibility of replication. With a clear research design, other researchers can repeat the same experiment numerous times to find tendencies in the population of interest. This attribute also allows researchers to test the findings of a study, or to contribute to the study by performing the same experiments.

Traditionally, experiments have been conducted in laboratories, but experimental economists are now increasingly going out in the field for their experiments (Carpenter et al. 2005). A driving factor for the trend is that field experiments are able to capture important characteristics of the real world in greater extent than laboratory experiments (Reiley & List 2008). What "field" entails and how to distinguish lab experiments from field experiments is discussed in more detail in the next sub-subsection.

3.3.2 A TAXONOMY OF EXPERIMENTS

Harrison & List (2004) propose six factors that determine the degree of field context in an experiment. All of the factors could affect the observed behaviour of the subjects[7], implying that results obtained from lab experiments and field experiments might deviate. The authors acknowledge correlation between some of the factors, but still consider them to be useful in assessing experiments. The factors are summarised in Table 2 and explained below.

TABLE 2 FACTORS DETERMINING THE FIELD CONTEXT OF AN EXPERIMENT

Factor	Description
Subject pool	Standard vs. non-standard subject pool
Information the subject brings to the task and commodity	Inherent information might affect the behaviour of the subjects differently in the lab than in the field
Nature of the commodity	Abstract goods vs. physical goods or actual services
Task or trading rules introduced	The development of subjects' heuristics of the task could be different in the lab than in the field
Stakes	The level of the stakes might affect behaviour
The environment in which the subjects operate in	The environment is by design different in the lab than in the field, which might affect behaviour

Source: Harrison & List (2004:1012)

A common criticism of lab experiments is the use of students and not "real people" as subjects. Undergraduate students, often from the U.S., form a considerable part of the total experimental subject pool (Henrich et al. 2010). Generalising from this specialised group to other populations is questionable, in particular since research has shown that the group is a possible behavioural outlier (Henrich et al. 2010).

Sampling from a pool of subjects not relevant for the task or commodity could ignore relevant information that the population of interest holds. Relevant field subjects will bring a relevant set of information to the task or the commodity, and thereby resemble reality better than other less relevant subjects. Due to more context in field experiments, the behaviour of the subjects are more likely to take real life information into account in these experiments, than in lab experiments (Reiley & List 2008).

The commodity used in the experiment could in itself affect the behaviour of the subjects. An abstract good could give less or different connotations than relevant physical goods would give. Cardenas & Ostrom (2004) note that if the nature of the commodity is not recognised its possible effect on behaviour is not accounted for.

[7] I use the terms subject and participant interchangeable in the thesis, referring to individuals participating in an experiment.

An experiment normally asks the subjects to perform a task and might also introduce a rule at some point. In this thesis, this aspect of an experiment is defined as the experiment's game structure. The game structure constitutes the rules and mechanisms of the experiment, as well as the payoff and incentive structure. The nature of the game structure could be important in the development of the subjects' heuristics of the task (Harrison & List 2004). Abstract tasks and rules might affect the heuristics differently than those that the subjects can recognise and relate to.

Different level of stakes might also affect behaviour. Carpenter et al. (2005) highlight this as one of the main criticisms of lab experiments. The real stakes in the field might be higher than the stakes set in the laboratory. Field experiments, especially in low-income countries, open up for stakes closer to reality.

Lastly, the environment creates a context for the subjects in which they create their strategies and heuristics (Harrison & List 2004). The environment in an unfamiliar laboratory will not produce the same context and might therefore affect subjects' behaviour differently than a field context would do.

Harrison & List (2004) emphasise that the introduction of e.g. a non-standard subject pool or a physical good in a lab experiment is not necessarily sufficient for the experiment to be labelled as a "pure" field experiment. Still, based on the presented factors they propose the taxonomy of experiments presented in Table 3.

TABLE 3 TAXONOMY OF FIELD EXPERIMENTS

Experiment	Description
Conventional lab experiment	Standard subject pool, abstract framing, an imposed set of rules.
Artefactual lab experiment	Same as the category above, but with a non-standard subject pool.
Framed field experiment	Same as the category above, but with field context in the commodity, task or information.
Natural field experiment	Same as the category above, but undertaken in the natural environment and subjects are unaware of their participation in the experiment.

Source: Harrison & List (2004:1014)

According to Harrison & List (2004), moving down the rows of Table 3 increases the external validity of an experiment. The following sub-subsections will define external validity, as well as internal validity, present advancements in the design of economic experiments, and finally discuss how taking these advancements into account can affect the validity of an experiment.

3.3.3 External and internal validity

External validity can be defined as the relationship between the behaviour of the subjects in the experiment and the real life behaviour of the population of interest (Lusk et al. 2006). The issue is therefore critical in arguing for the use of an experimental study. In fact, most criticism of the experimental method is related to external validity (Lusk et al. 2006). Considerable space is therefore devoted to the issue in the following sub-subsections.

Internal validity refers to the extent an experiment shows a confident cause-and-effect relationship (Loewenstein 1999). If all other variables than the variable of interest are held constant, the experiment has internal validity and the researcher can observe the *ceteris paribus* effect on behaviour, given a deliberate treatment. Threats to internal validity include the learning effect, where a subject behaves differently at later stages in the experiment due to the knowledge gained through participation (Ledyard 1995), and varying experiment conditions; due to for instance location, time of day, seasonality and subject sample.

Even though internal validity is seen as less of a problem than external validity, the categorisation has been accused for being unnatural (Carpenter et al. 2005), meaning that issues under external validity could be just as relevant to discuss under internal validity. Inadequate sampling will for instance undermine both internal and external validity. The following does consequently not specifically consider if the presented design issues affect the external or the internal validity of the experiment.

3.3.4 MONETARY REWARD

Monetary incentives are an integral part of most economic experiments today (Carpenter et al. 2005). Smith (1976) introduced the induced value theory, in which he stresses the importance of inducing monetary value on choices made by subjects in experiments. Friedman & Sunder (1994:12) summarise the theory and further emphasise its importance by stating:

The key idea of induced-value theory is that proper use of a reward medium allows an experimenter to induce pre-specified characteristics, and the subjects' innate characteristics become largely irrelevant.

Furthermore, Friedman & Sunder (1994) present Smith's three conditions for inducing characteristics: (1) Monotonicity: Subjects must prefer more reward to less, and must not be satiated. (2) Salience: The level of reward achieved in the experiment depends on the actions of the subject. (3) Dominance: The subjects' utility depends primarily on the reward, and therefore to a negligible extent other factors.

Once the three conditions are met, the experimenter has control over the subjects' characteristics and is able to explain behavioural changes by the experiment's game structure (Friedman & Sunder 1994). The induced value theory might, however, underestimate the influence of other factors than monetary reward in choices done, both in the experiment and in real life. The sub-clause in the quote from Friedman & Sunder (1994) is a strong statement and the following sub-subsection will look more closely into other factors affecting subjects' choices in an experiment.

3.3.5 FIVE OTHER FACTORS AFFECTING BEHAVIOUR

Levitt & List (2007) argue that the observed behaviour in an experiment is a result of not only monetary reward, but also at least the five other factors listed below. These factors may vary systematically between the experiment and the field.

(1) the presence of moral and ethical considerations
(2) the nature and extent of scrutiny of one's actions by others
(3) the context in which the decision is embedded
(4) selection into the experiment
(5) the stakes of the experiment

First, a subject will consider moral benefits and costs to particular action. For instance, performing an action that goes against prevailing moral codes in the society comes at a cost the subject will consider (Levitt & List 2007). The size of the cost depends on several aspects. The stakes determines the level of monetary reward the subject will receive, but also the size of the possible externality imposed on others. Imposing negative externalities on others are generally thought of having a moral cost. Therefore increasing the stakes could increase the moral cost of an action. Also, the good in question might have particular norms or legal rules attached to it and therefore the context matter. And if the action is closely scrutinized or broadcasted to others the moral cost or benefit will further increase.

Scrutiny – or close and visible observation – of subjects in an experiment might affect behaviour. In the famous Hawthorne experiments the researchers observed changing productivity of labourers simply due to participation in the experiment (Carpenter et al. 2010). This change in behaviour is generally thought to be pro-social (Levitt & List 2007), meaning that observed behaviour is likely to be less selfish the closer the scrutiny. To avoid a pro-social bias, the experiment therefore should aim at resembling the level of anonymity the subjects would experience if conducting the same actions in real life.[8]

Third, context does matter. Taken from psychology, behavioural economics presents the concept of priming. Priming is non-conscious activation of social knowledge structures (Bargh 2006), meaning that the mere sight or hearing of a concept like money or cookies affect our behaviour through orientating the mind towards that setting. Kahneman (2011) for instance reports of an experiment where subjects that saw a screensaver depicting money were less likely to help a person to pick up pencils that fell on the floor. He also reports an experiment where the mere sight of words referring to old age makes the subjects move slower.

Monetary reward does make the most convincing argument for creating incentives in an experiment to meet the conditions of Smith (1976). At the same time, priming suggests that introducing concepts related to the issue of study can activate behaviour that relates to the real life behaviour. Therefore asking subjects to fill in forms to state their behaviour might create priming effects towards for instance bureaucracy or hospitals, which might trigger a particular behaviour. Similarly, to announce the outcomes of the possible choices in terms of monetary reward instead of indirectly through a related good might create a bias towards selfish behaviour.

Fourth, the method of selecting participants to an experiment might affect the observed behaviour. As mentioned, students are commonly used as subjects and these students are normally self-selected into the experiment (Carpenter & Cardenas 2008). Levitt & List (2007) state that to generalize findings from an experiment in which the subjects systematically differ from the targeted population might create unsatisfactory results. Targeting the population of interest and avoiding self-selection into the experiment are therefore important to be able to generalise the observed behaviour.

Lastly, Levitt & List (2007) make a cautious claim that increasing the stakes in an experiment would increase the importance of monetary incentives relative to moral and ethical

[8] Ostrom et al. (1994) in their experimental study, report statements done by subjects during a session where they were allowed to communicate, indicating close scrutiny by the researchers. This is likely to create a pro-social bias according to Levitt & List (2007) and Carpenter et al. (2010).

considerations when subjects consider choices. They do, however, acknowledge that the literature is mixed in this regard: some papers have found changing behaviour due to changing stakes in several dictator games, while others have found that fairness plays an important part in trust gift exchange games both with low and high stakes. Carpenter et al. (2005) conclude that after the study of Cameron (1999) the convention in both developing and developed countries is stakes at about a day's work.

Levitt & List (2007) consequently set up the utility maximizing subject *i*'s utility function in Equation [2].

$$U_i(a, v, n, s) = M_i(a, v, n, s) + W_i(a, v) \quad [2]$$

The subject *i* must in the experiment make a choice and take the action *a*. The action taken would affect the subject's wealth, *W*, and produce a moral cost or benefit, *M*. The effect on the subject's wealth depends simply on the action and the stakes, *v*. The higher the stakes, the higher the monetary reward. The moral cost or benefit also depends on the action and the stakes, but is additionally influenced by norms and legal rules in the given society, *n*, and the level of scrutiny, *s*.

3.4 REVIEW OF FINDINGS

3.4.1 TESTING THE FIVE FACTORS

Voors et al. (2011), in testing the five factors of Levitt & List (2007), find that more pro-social behaviour in two basic public goods games are correlated with less forest-related commercial interest and interaction, and less involvement in illegal hunting. Still, some of the results from the two game variants are contradicting. This, they claim, is due to changing context; further emphasising the importance of relevant context in experiments.

Benz & Meier (2008) suggest that pro-social behaviour is more prevalent in the laboratory than in the field. They find that individuals who willingly donated to charitable funds in a laboratory experiment did not do so in real life when they did not know they were observed. Similarly, Lusk et al. (2006) conducted two types of experiments at a grocery store. One where the subjects knowingly participated in a framed field experiment, and one where they unknowingly participated in a natural experiment. They find a tendency in subjects to show slightly more pro-social behaviour in framed field experiments than in natural experiments, showing the effect of scrutiny. Still, the authors claim that the framed field experiments are "reasonably accurate" in predicting consumer behaviour (Lusk et al. 2006:290).

Overall, the literature on experimental methods highlights the importance of sound experimental design to be able to draw conclusions from the findings. Therefore, research question one specifically aims at testing this issue.

3.4.2 TESTING INSTITUTIONS

Rodriguez-Sickert et al. (2008) argue that economists often assume away the influence institutions have on behaviour, and that this assumption might have unfortunate consequences. One example is paying for blood donations, which made blood donations plummet in Great Britain (Rodriguez-Sickert et al. 2008). The following presents some of the limited literature testing relevant institutions for this thesis.

In analysing abstract social dilemmas in laboratory experiments with undergraduate students, Ostrom et al. (1994) find that face-to-face communication significantly enhances the participants propensity to reach the socially optimal outcome. They link the finding to community management's success in solving social dilemmas. Cardenas (2000) supports the finding in a similar experimental study undertaken in Colombia. He found that the outcome of a social dilemma is closer to social optimal with communication than without communication. Cardenas similarly indicates the communication treatment to be correlated with self-governance and therefore also hints that the finding makes a case for CFM in solving the social dilemma. Platteau (2004) claims the main advantage of these kinds of measures is that it utilises the local groups superior knowledge of local conditions and constraints. The main disadvantage, he continues, is the possible capture of benefits by local authorities and elites, i.e. elite capture.

Rodriguez-Sickert et al. (2008) find that imposing fines for defectors in a CPR experiment increased cooperation among participants sampled from rural Colombia. Velez et al. (2010) in their experimental study in the same country also find that regulatory schemes with fines increase the propensity to cooperate, but furthermore emphasise that such schemes also undermine organisation at the community level. They find that communication, simulating community management, also increases the propensity for a cooperative outcome, as Ostrom et al. (1994) and Cardenas (2000). Both studies indicate correlation between real life regulations and rules of the experiment.

The mentioned studies basically examine the effect of two of the three institutional arrangements presented in subsection 2.4. In comparison, little experimental research has been conducted on the third arrangements, PES. The hypotheses of research question two builds on previous findings in aiming to examine all three arrangements.

3.4.3 TESTING CHARACTERISTICS

As sub-subsection 3.1.2 discusses, local specific characteristics matter for the behaviour of forest users. It is not only the characteristics of the CPR that matter for the outcome of the CPR dilemma, but also the characteristics of the population and the local community (Ostrom 1999).

According to several scholars, heterogeneity within a group matter for the group's propensity to solve a social dilemma. Cardenas (2003) argues that unequal distribution of wealth and group member heterogeneity reduces the group's capacity to make coordinated strategies. He furthermore cites Kramer & Brewer (1984) who argue that homogenous groups are more prone to cooperate due to a greater sense of group identity. The group identity, they define, is based on what the group members are, do, or have. Furthermore, Platteau (2004) claim that heterogeneity increases the chance of elite capture.

Ostrom (1999) supports Cardenas (2003) and Kramer & Brewer (1984) and claim that members with heterogeneous cultural backgrounds might interpret rules, trust and reciprocity differently, potentially affecting the outcome of the CPR dilemma. Ostrom (2006) reports experimental studies showing that imposed heterogeneity results in less cooperative outcomes. Ledyard (1995:143) even claims the effect of group heterogeneity on group cooperation to be "strong and replicable" positive, and terms it as a stylized fact. Ostrom (1999), finally and importantly, warns that heterogeneity is a highly contested variable as groups may vary in a wide range of attributes. Cultural background, interests and endowments are three – of numerous – group characteristics that may be referred to when addressing group heterogeneity.

Individual characteristics may also affect the outcome of the CPR dilemma. Gender matters according to the influential ecofeminism literature that emerged in the 1980s (Agarwal 2010)[9]. The eco-feminists argue that women are "closer to nature than men" and therefore are better conservationists than men (Agarwal 2010:41). Shiva (1989) argues that women traditionally have been responsible for gathering forest products for the household, making them better at treating and renewing the forest's biodiversity. The view is supported by Merchant (1996:26) who summarises ecofeminism's many strands with the belief that women's close relationship to reproduction invokes their "common goal of restoring the natural environment and quality of life".

Experimental studies find ambiguous results in examining gender and cooperation (Cardenas, Dreber, et al. 2011; Schwieren & Sutter 2008; Leah-Martin 2012; Ledyard 1995). To my knowledge, little research has examined gender's role in determining the likely outcome of a CPR dilemma.

Another individual characteristic that might affect the outcome of the CPR dilemma is age. Grossmann et al. (2010) argue, and present findings indicating, that older people are better at reasoning about social dilemmas and consequently should possess key roles in decision making processes. Also Meier & Frey (2004) find that age is positively correlated with pro-social behaviour, as older participants in their experiment were significantly more inclined to donate to social funds. Carpenter et al. (2008) find that older participants in an experiment were more prone to contributing to a public good than younger participants. At the same time, Park et al. (2002), after empirical testing, warn that cognitive abilities decline with age, making older individuals less able to process situations and therefore making well-informed decisions. None of the studies does, however, analyse the CPR dilemma specifically.

3.5 Summary and the hypotheses

Ostrom et al. (1994) set a solid theoretical foundation in exploring the issue of common pool resources and the dilemmas that might arise in the use of these resources. However, when testing the theory, their methods are questionable. In the first part of the book they argue for the importance of taking local and good specific considerations into account when analysing CPR dilemmas, before they in the second part present findings from abstract laboratory experiments performed with American undergraduate students. They argue that descriptive field studies presented in part three of the book offset the abstractedness of part two. Still, if the behaviour of the population of interest depends on the attributes of the good of interest and on the area or community of interest, these attributes should be taken into account in the experimental study. The recent advancements in experimental economics show that this is necessary and possible.

Levitt and List (2007) suggest five other factors that should be considered besides monetary reward to be able to generalise the findings of an experiment. Figure 3 summarises these and other factors affecting the observed behaviour of subjects in an experiment. Even though trade-offs have to be made in designing an experiment, all the presented factors should be considered in the design. Therefore, the model is employed when considering the experimental design of the thesis in section 4.

[9] Agarwal's book is endorsed by Elinor Ostrom who highlights "…[the] difference women can make, when they are actively involved in forest governance" (Agarwal 2010:ii)

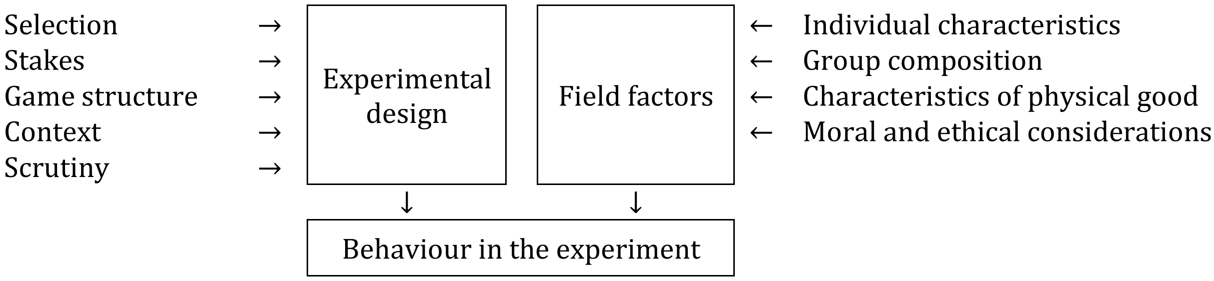

FIGURE 3 AN OVERVIEW OF FACTORS AFFECTING A PARTICIPANT'S BEHAVIOUR IN AN EXPERIMENT

By holding other factors constant, the thesis aims at testing the significance of some of the factors in Figure 3. The thesis will explore three research questions. Research question one refers to the validity of the experiment. If the experiment possesses dubious internal or external validity, other potential results are of lesser interest. Research question two examines the effect the institutions presented have on behaviour. Lastly, research question three examines the importance of some individual characteristics and group composition in determining behaviour. Each research question will be answered by testing related hypotheses, based on the review of previous studies in subsection 3.4.

RQ1 Does the experiment possess sufficient external and internal validity?
H11 The change in behaviour from the 1st part to the 2nd part of the experiment is due to the externally imposed treatments.
H12 The behaviour of the participants in the presented experiment is predictive for their real life behaviour.

RQ2 How does the game structure, simulating the management regimes, affect the forest use?
H21 The open access situation induces the highest forest use.
H22 The CAC treatment induces higher forest use than the PES treatment.
H23 The CFM treatment induces the lowest forest use.

RQ3 How do individual and group characteristics affect the forest use?
H31 Younger individuals have higher forest use than older individuals.
H32 Male individuals have higher forest use than female individuals.
H33 Heterogeneous groups have higher forest use than homogenous groups.

4 DATA AND METHODS

This section presents the methods used to gather the data presented and discussed in section 5. The methods are presented in the sequence in which they were applied, and are discussed separately in the light of the theoretical framework of section 3. Finally, I evaluate the validity of my collected data.

I conducted fieldwork in Tanzania from June 19th to August 13th 2012, with the assistance of two students at the Sokoine University of Agriculture (SUA): Lucas Jonathan Kwiyega Matatizo as enumerator and Donata Dominic Shirima as monitor[10]. The responsibilities of the two roles are described in section 4.6. "We" refers in the following to the three of us.

4.1 SAMPLING

4.1.1 VILLAGE SAMPLE

We collected data from seven villages in five districts, in three regions of Tanzania. The regions, Shinyanga, Singida and Morogoro, are located in the North-West, centre and South-East of the country, respectively. The villages' locations are given in Figure 4, while the villages and some village characteristics are presented in Table 4.

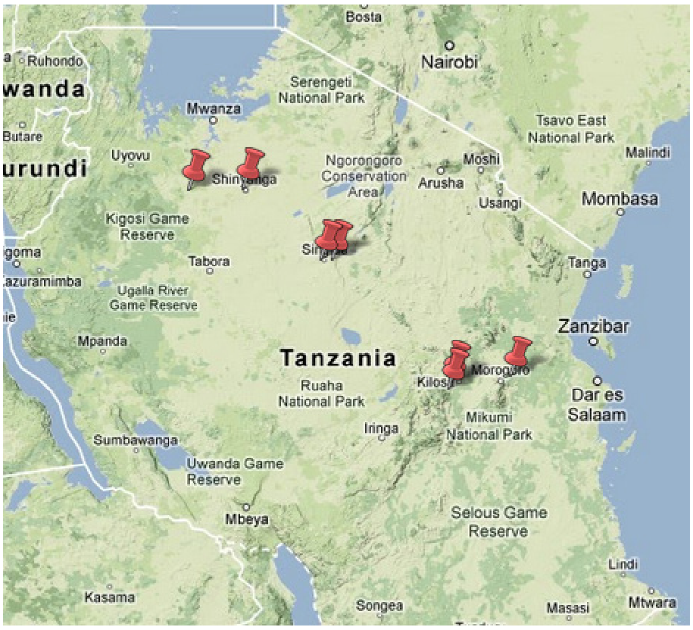

FIGURE 4 LOCATION OF THE VILLAGES IN THE SAMPLE[11]

[10] The term «monitor» is adopted from Cardenas & Ostrom (2004) and Cardenas (2003) for describing the individual observing the participants' behaviour.
[11] Source: Google Maps. Available from: http://goo.gl/maps/vZHaV [18 March 2013].

TABLE 4 VILLAGES' CHARACTERISTICS

Village	District	Region	Externally driven conservation projects	Ownership of forest	Population	Total number of livestock[a]	Public transport	Distance to nearest town[b]	Distance to forest frontier[b]
Mughunga	Singida Rural	Singida	Community conservation	Reserve and community managed	1922	1050	1/day	45	3
Nduamughanga	Singida Rural	Singida	Community conservation	Reserve and community managed	2970	1834	1/day	67	2
Busongo	Kishapu	Shinyanga	None	Ngitilis[c]	2219	1850	No	50	3
Ngulu	Kahama	Shinyanga	REDD	Ngitilis[c]	2234	1265	No	77	2
Zombo	Kilosa	Morogoro	None	Community managed	3401	15	Several times/day	18	5
Dodoma-Isanga	Kilosa	Morogoro	REDD	Community managed	1308	52	No	35	1
Muhungamkola	Morogoro Rural	Morogoro	None	Reserve and community managed	1756	1735	No	45	4

Notes: [a] cows and donkeys. [b] numbers in kilometres. [c] Swahili word for "enclosure", a traditional arrangement where private or communal areas are managed as woodland or pasture (Blomley & Iddi 2009).
Sources: The village's chairperson, the district's forest officer, or the area's forest conservation project coordinator.

The population of interest is villages where the village members utilise a nearby forest resource for household use. The criterion to be included in the village sample is thus an accessible forest within walking distance of the "village centre". Villages within the criterion were sampled to ensure variation in village characteristics; since village specific characteristics might unpredictably influence the behaviour of forest users. As depicted in Figure 4 and Table 4, the villages vary in location, forest management regime, population size, livelihoods (as indicated by the number of livestock, some villages are more pastoral than others), accessibility, and distance to forest frontier and closest city. The villages were chosen in collaboration with district authorities or local level NGOs.

The village sample is not larger due to practical reasons. Time and financial constraints prevented us from sampling more than seven villages.

4.1.2 HOUSEHOLD SAMPLE

Random sampling is the key criterion for inference, i.e. applying the results in the sample to the population. Upon arriving in the villages, we asked the village leaders to find or prepare a list of all households in the village. We then assigned a number to each household and drew 40 households, plus an additional 10 backup households in an ordered list; in case any of the first 40 households were unable to attend. The 40 households were randomly assigned to five experiments, meaning that each experiment consisted of eight participants.[12] If a household had a husband and a wife, a coin toss decided which one of the two was to be invited. We then noted personal data on the chosen participants to crosscheck the identity of the participants on the list with the individuals that appeared for the experiments.

The village leaders helped us assign two persons to do the job of inviting the participants. The inviters were strictly instructed to invite only the individuals on the given list and only to the assigned experiment at the assigned time. If anyone was unable to attend, the inviters were instructed to invite individuals from the top of the backup list, or to contact us.

Despite these strict instructions, we repeatedly discovered that the inviters brought self-selected individuals. In these cases, except one,[13] we postponed the experiment to find the missing participants or to invite others from the top of the backup list. Yet, it is likely that some self-selected (or rather inviter-selected) participants were not exposed and consequently participated in the experiments. The fraction of self-selection will vary with village, as it is highly dependent on the inviters. In some villages we perceived the inviters to be thorough in their work; they went to great length to find even the remotely located participants and were open about the process. While in other villages, the inviters were surprisingly quick to find all 40 participants and were secretive about their methods. Evidently, we revealed more attempts of self-selection in the latter than in the former case. A related issue is that the variation in inviter methods could make the invitees turn up for the experiment with different expectations. We had limited control of what the inviter actually told the participants he (it was always a he) invited. As both the self-selection bias and the "expectation-bias" are village specific, controlling for village-specific unobservables would mitigate the effect this has on the results.

[12] The same group size as Ostrom et al. (1994) and Cardenas (2003) utilised, as eight subjects "approximates some of the characteristics of larger groups or conflict-ridden small groups" (Ostrom et al. 1994:108).

[13] On one occasion we were unable to locate the sampled subjects. Annex I elaborates on the experiment and explains why it is included in the dataset.

Two other relevant biases are attrition bias and randomization bias. According to Heckman & Smith (1995) randomization bias occurs when random sampling makes the participants differ systematically from the population of interest. There could be a bias in who accepts and who declines the invitation to participate due to the random and experimental nature of the research. Harrison et al. (2009) find a bias towards risk seekers in experiments, as the risk averse would be inclined to avoid unfamiliar situations like an experiment. They also find that experiments with a guaranteed show-up fee decrease the risk seeking bias, and that the bias decreases with the fee. I did not have a stated show up fee, but rather stated that the participants receive money dependent on their harvest. As such, they could feel that some reward was guaranteed. Still, according to Harrison et al. (2009) I should expect some bias towards risk seeking in my findings.

Attrition bias is not an issue in the conducted experiments. All the participants who entered the experiments completed it. Only one participant, clearly intoxicated, went missing after completing the experiment and before being interviewed. We were not able to locate him again and therefore lack interview data for one participant.

The total sample size is 288 participants. The sample consists of four subsamples: three treatment groups of 80 participants each, and one control group of 48 participants. The sample sizes are discussed and tested in Annex II.

Table 5 depicts the distribution of the sampled participants' characteristics. The table indicates that most participants state that they collect forest products each week, supporting subsection 2.2 in that local forest use is widespread. As only 21% state to be selling forest products, a majority of the forest product harvesting is for subsistence use. Furthermore, an almost equal split in participants born in the village and not, and participants belonging to the village's largest tribe and not, indicates ethnically diverse villages. Also note the skewed distribution of stated wealth, making the variable untrustworthy.

TABLE 5 PARTICIPANTS' CHARACTERISTICS

Characteristic	Distribution
Gender	
Male	51%
Female	49%
Age	
18-24	7%
25-39	47%
40-64	41%
65-90	5%
Children in the household (below 18 years)	
No children	6%
1-2 children	27%
3-4 children	38%
5-7 children	25%
8-14 children	5%
Which wealth group the participant states to belong to	
The village's richest third	0%
The village's middle third	78%
The village's poorest third	22%
Times per week the participant visits the forest to collect forest products	
0	4%
<1	11%
1	38%
2-5	43%
>5	5%
Has the participant sold forest products in the last month?	
Yes	21%
No	79%
Has the household taken part in paid work outside the farm during the last month?	
Yes	14%
No	86%
Hectares of land the household owns	
0	1%
<1	2%
1-5	51%
6-10	28%
>10	18%
Does the participant know of any forest conservation project nearby?	
Yes	42%
No	58%
Is the participant born in the village	
Yes	58%
No	42%
Does the participant belong to the village's largest tribe?	
Yes	56%
No	44%

Notes: N=287

4.2 FROM CPR EXPERIMENTS TO A FOREST EXPERIMENT

Following the seminal work of Ostrom et al. (1994), several experimental studies have tried to study behaviour in relation to the social dilemma arising with the collective use of a common pool resource (some of which are presented in subsection 3.4). As pointed out by Cardenas, Janssen, et al. (2011), the experimental design of these studies have for the large part focused on the institutional and behavioural aspects, rather than on the characteristics and dynamics of the common pool resource of interest.

Forest as a common pool resource has particular attributes that are not considered in more abstract experiments. For instance, a forest has a growth rate which affects the decisions of forest users and therefore should affect the decisions of the participants of an experiment. Also, forests might have a priming effect that differs from for instance money. Forests might consequently activate social knowledge structures in forest users and therefore trigger different behaviour than the more abstract experiments are able to do. Most real life decisions related to harvesting forest products are made in the forest, so an experiment should try to include the priming effect in the design.

The experimental design of this thesis builds on Cardenas et al. (2011) and Slavíková et al. (2011), and is forest specific; rather than CPR general. The following subsections present and discuss the experimental design in relation to the theory of section 3, with the goal of achieving internal and external validity.

4.3 STOCK AND STAKES

4.3.1 THE STOCK

After signing the consent form presented in Appendix III, and given the first part of the instructions in Appendix I, the eight participants of an experiment are collectively endowed with a stock of 80 cardboard trees. The trees, depicted in Figure 5, are about 6 cm tall and imitate acacia trees. The participants are individually rewarded according to the number of trees they harvest from the stock. The harvesting is done in rounds where the participants, one at a time, face the stock in private from the other participants. The participant reveals his or her harvest decision by physically tipping over the chosen amount trees, as is depicted in Figure 17 in Appendix V.

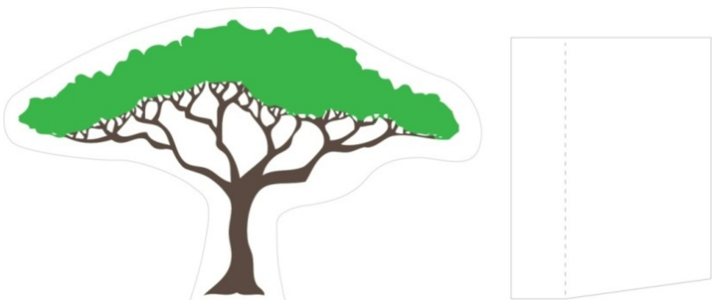

FIGURE 5 THE MODEL USED TO IMITATE TREES

At the end of a round, when all eight participants have harvested, the collective harvest is announced to the group. The remaining forest stock grows with the growth rate of two trees for every ten trees standing. The high growth rate is necessary to ensure the social dilemma.

4.3.2 The stakes – monetary reward

At the end of the experiment, and after interviewing the participants, the harvested amount of trees is converted to Tanzanian shillings. The calculation made to set the price of a tree is given in Equation [3a] and [3b]. First, the targeted mean reward for each participant at the end of an experiment was set to TZS 3 000, about USD 1.85. The stake was, in collaboration with Professor Kessy[14] at SUA, set to be high enough to create a significant incentive (about a day's work), but low enough not to be disturbing; both for the participants and for future researchers visiting the village. To find the median amount of harvested trees, the mean of two extreme outcomes presented in section 4.4.1 was used, divided by the number of participants in an experiment and multiplied with two as there are two parts of the experiment.

$$\left(\frac{\frac{88+144}{2}}{8}\right) * 2 = 29 \text{ trees} \qquad [3a]$$

The expected median amount of trees after completing the experiment is thus 29 trees for each participant. By dividing the targeted reward by the median expected harvest and rounding the number of to the nearest ten, the unit price for the trees becomes TZS 100. The median participant should consequently expect to be rewarded with TZS 2900, slightly lower than the targeted amount.

$$\begin{aligned} &29 \text{ trees} = \text{TZS } 3000 \\ &1 \text{ tree} \approx \text{TZS } 100 \end{aligned} \qquad [3b]$$

4.3.3 Assessing the payoff structure

The experiment's payoff structure meets the monotonicity condition and the salience condition of Smith (1976), that sub-subsection 3.3.4 presents. These two conditions are important for the internal validity of the experiment. If they are not met, the causal relationship should not be considered as strong as if they are met. For instance, if the participants become satiated, behaviour in later rounds could seem irrational, as the incentives are not as strong as in the first rounds. The dominance condition, however, is difficult to assess. If the participants consider other factors than the monetary reward offered and these considerations affect their choices, the condition is not met. This might be the case of the presented experiment. Moreover, as I have mentioned and will elaborate on, this might be in conflict with the intents of the experimental design. Table 6 summarises the performance of the experiment in fulfilling the conditions of Smith (1976).

[14] Professor John F. Kessy is head of the Department of forest economics at SUA.

TABLE 6 EVALUATING THE PAYOFF STRUCTURE

Condition	Explained	In the experiment	Is the condition met?
Monotonicity	Participants must prefer more reward to less, and must not be satiated.	Monetary reward was used.	Yes
Salience	The level of reward achieved in the experiment depends on the actions of the participant.	The level of reward is calculated from the number of trees the participant harvests.	Yes
Dominance	The participants' utility depends primarily on the reward, and therefore to a negligible extent other factors.	The reward was calculated to give incentives, but also to not be disturbingly high. Also, field context primes participants towards forests.	?

The dominance condition improves the researcher's control of the experiment as the participants behaviour is mainly driven by a factor that is easily observed and controlled; in this case: money. This control does, however, come at the cost of losing other relevant but unobservable factors affecting the real life behaviour, which the experiment is trying to observe. By involving these factors in the experiment, the researcher loses some control, i.e. decreases internal validity, but at the same time increases external validity, as the experiment to a greater extent resembles reality. One way to involve these factors is through adding context, which is discussed in section 4.5.

If one tries to apply the three conditions of Smith (1976) to a real life situation, one might find that they are all violated. A subsistence forest user might satiate the need for forest products, in particular if one assumes the presence of market imperfections. Therefore the situation would not meet the monotonicity condition. The salience condition might get violated if a forest-reliant individual receives forest products from others, because of for instance high status, and is therefore receiving products independent of present actions. Lastly, a forest user making harvest decisions might consider local customs and traditions as well as the material gains of the forest products, and thereby not meet the dominance condition.

Purposely violating the conditions of Smith (1976) and instead focusing on increasing external validity through a relevant reward is therefore an option. In the case of Tanzanian forest users, an alternative reward is firewood or charcoal. The approximate price for a kilogram of charcoal in Tanzania is between USD 0.02 and USD 0.12 (Kilahama 2008). The total monetary reward distributed through the experimental study was USD 445, such that each participant received on average USD 1.55. If the participants should have received the same reward in the form of charcoal we would have to distribute up to three tons of charcoal in each village! With firewood, the physical amount would be even larger. Rewarding the participants with the relevant commodity is thus an interesting area of further research, but unfortunately out of the scope of this thesis. The experimental design therefore uses monetary reward and rather aims at using context as the means to induce other considerations.

4.4 GAME STRUCTURE

The experiment consists of two parts of six rounds each. The design mainly utilises within-participant comparison. The first part is therefore the same for all participants. The part simulates the open access situation, where the only constraint the participants face is a technical limit to how much they can harvest. The technical limit for the first round is always 5 trees each, but if the forest stock decreases to below 40 trees, the technical limit also needs to decrease. This is not only necessary to avoid a negative stock, but also realistic if one assumes the cost of harvesting to increase with the scarcity of trees. The participants therefore need to follow the maximum harvest table of Table 7, which is presented to the participants in the form of Appendix IV.

TABLE 7 MAXIMUM HARVEST TABLE

Stock size	Max. harvest per participant
40-160	5
32-39	4
24-31	3
16-23	2
8-15	1
0-7	0

For the second part, one of four games[15] is played. The sequence in which the respective experiments are played is predetermined and distributed on the seven villages to make the three treatments imposed at least once in each village, and in different sequences. The three treatments are command and control ("CAC"), payment for environmental services ("PES"), and communication, simulating community forest management ("CFM"). The fourth game is the open access situation repeated. Table 8 shows the distribution of participants in the four games.

TABLE 8 DISTRIBUTION OF PARTICIPANTS BY GAME

Game	No. experiments	No. of participants
CAC	10	80
PES	10	80
CFM	10	80
Open access	6	48
TOTAL	36	288

As the experiment measures the change in behaviour from part one to part two, it utilises a within-participants design. At the same time, since a control group of participants is exposed to the open access situation in both parts, I am able to control for possible behavioural changes occurring without treatment. The experiment thus also compares between participants, and consequently possesses a mixed design.

In the CAC treatment, simulating the regime described in sub-subsection 2.4.1, a rule that the participants can legally harvest 0, 1, 2 or 3 trees is announced. They are furthermore told that it is possible to harvest more, but that the technical limit still applies. After each decision the researcher throws a die. If the die shows a five or a six, the researcher inspects the harvest decision of the participants. If the rule is broken, the participant is penalized with 10 trees; in addition to losing all of the trees harvested in that round. Even though the caught participant does not receive the attempted harvest, the trees are removed from the stock.

[15] I use the term "game" to distinguish the different game structures introduced in the second part. The term therefore includes both the three treatments and the open access control group.

In the PES treatment, the participants are paid TZS 80 for the trees they decide *not* to harvest, as well as the TZS 100 for the ones they harvest. The participants as such receive $\text{TZS } 100\text{x} + \text{TZS80}(\text{z} - \text{x})$ in monetary reward, where x is the amount of trees harvested and z is the maximum allowed harvest. Sub-subsection 2.4.2 describes the management regime the treatment aims at simulating. The payment for not harvesting is set at 80% of the reward for harvesting, as setting the payment equal to or higher than the harvest-reward should be trivial; the participants would choose to receive the payment.

In the CFM treatment, the participants are allowed to communicate collectively for three minutes at the start of each round. The instructions states that they could talk about harvesting decisions, but give no further guidelines. The participants discuss in private. After the 3 minutes session, the participants are again prohibited from communicating. The treatment aims at simulating community management, as described in sub-subsection 2.4.3.

The forest management regimes presented in the background section are complex arrangements that involve extensive regulations and mechanisms, and occur in diverse variants. Simplifying these regimes into treatments in an experiment might therefore seem infeasible. The intent of the treatment is, however, not to simulate all aspects of the regimes, but rather to extract the underlying mechanism of the regime and convert it to a treatment. In the case of CAC, the underlying mechanism transferred to the experiment is the enforcement of rules to limit forest use. In the case of PES, the idea to reward positive externalities is transferred to the experiment. And in last case, the treatments simulate the transfer of the decision making process to the local level. Therefore, the experiment does not examine behaviour in relation to specific forest management arrangements, but examine forest users' behaviour in relation to the idea which the three regimes are founded upon.

4.4.1 Strategies

In accordance with the salience condition, the payoff of a participant in the experiment is dependent on his or her decisions through the experiment, i.e. the participant's strategy. Additionally, the CPR nature of the experiment makes the participant's payoff dependent on the strategies of the seven other participants in the experiment. The strategies of all the participants are therefore relevant for each private payoff. Figure 6 depicts three possible strategies followed by all eight participants for the first part of an experiment.

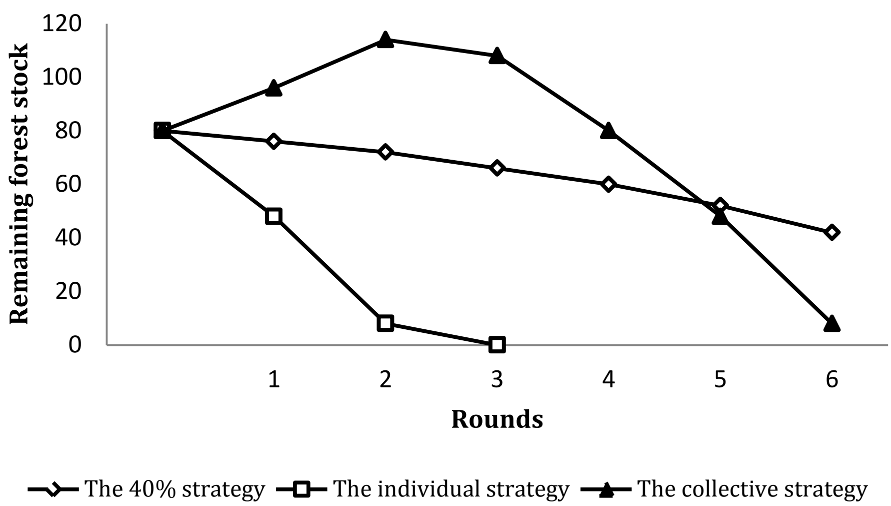

FIGURE 6 THREE AGGREGATED HARVEST STRATEGIES

If the participants harvest the maximum possible harvest each round ("the individual strategy"), i.e. five trees each in round one and two, and one tree each in the third round, the forest will be depleted after the third round and the total group reward would be 88 trees. The participants thus acquire merely one tree each from the growth rate. This is one extreme outcome.

The other extreme outcome occurs if the group follows the optimal collective harvest strategy ("the collective strategy"). Then the total group reward becomes 144 trees. This strategy maximizes the possible aggregated payoff for the participants by not harvesting for the first three rounds, then harvesting three trees each in the fourth round, and finally five trees each in the remaining three rounds (making the average harvest rate through the six rounds 60%). However, as will be discussed below, the optimal strategy is unrealistic as the participants are unaware of the number of rounds in the experiment.

If the group decreases the aggregated average harvest rate to below 80%, a more socially optimal outcome than by the first strategy can be achieved. If the average group harvest rate decreases to 20% or lower, however, even the collective strategy provides a higher group reward. There are numerous possible strategies within this space, analysing them all is therefore infeasible. A simple rule-based example is "the 40% strategy", where the group harvests 40% of maximum possible harvest each round, i.e. two trees each in each of the six rounds. In this case the total group reward becomes 96 trees.

At the individual level, Figure 7 depicts participant *i*'s payoff dependent on *i*'s strategy and on the strategy of the other participants in the group. The figure indicates that to maximise private payoff, participant *i* should follow the individual strategy, irrespective of the strategies of the other participants. This is true for all eight participants, meaning that if the participants are rational and selfish utility-maximisers, all will choose this strategy in every round. This is a not a socially optimal Nash equilibrium. The socially optimal outcome is in the lower right of the figure where all participants follow the collective strategy, but even the middle square, where all follow the 40% strategy, is a Pareto improvement from the Nash equilibrium. The treatments presented above aim to improve the outcome of the experiment.

		The rest of the group		
		Individual strategy	40% strategy	Collective strategy
	Individual strategy	11	28	27
Participant *i*	40% strategy	5	12	12
	Collective strategy	3	18	18

FIGURE 7 PARTICIPANT *I*'S HARVEST, DEPENDENT ON THE STRATEGIES OF ALL THE PARTICIPANTS IN THE GROUP

A valid criticism of the payoff structure is that the experiment deviates from real life in a principle matter: the optimal solution for the participants is, as shown, to deplete the forest stock at the end of the experiment, whereas the real life optimal solution for a group of forest users would be a sustained forest over the foreseeable future. The final goal of the participants and the final goal of forest users are thus in conflict; the former should deplete, the latter should sustain.

The experiment intents to measure how participants deal with a specific social dilemma, and even though the population and the sample deviate in their goals in the long term, they are the same in the short term: to lower the aggregate harvest rate. To achieve a socially better outcome than the Nash equilibrium, the participants would need to lower their individual harvest rates;

at least in the first few of rounds. The same is true for the real life situation, even though in this case the lowered harvest rate should be sustained in the long term. An expansion of the experiment, by for example including a collective reward, positively correlated to the remaining forest stock, would complicate the experiment and demand more of the participants' abilities to understand the mechanisms and therefore constrain them from making well-informed decisions.

Additionally, the number of rounds which the experiment consists of is unknown for the participants. This implies that the experiment resembles reality in that the participants are unaware of when "the end occurs". Therefore, the participants are unable to do backward induction. But, it also implies that the participants are unable to perform the calculations explained above. Still, there should be little doubt among the participants that the Nash equilibrium depletes the forest the fastest; and therefore reaps the gains of the growth rate the least. Any improvement from this outcome should thus be in the interest of all the participants.

4.5 Context

Priming matters, as mentioned in sub-subsection 3.3.5. A laboratory experiment performed in a "sterile" setting where participants are told to fill in forms to reveal their decisions that are directly linked to monetary rewards, might create priming effects that differ from what influences forest users. The priming effect can therefore exist in three forms: in setting, in commodity and in task. By exposing the participants to a relevant setting, commodity and task, the priming effect could make the participant include the factors mentioned in Figure 3 in their decision making process.

Instead of inviting and transporting the participants from their homes to another location for the experiments, we held the experiments in the villages. Therefore, when the participants made their experiment decisions they made them in surroundings similar to the surroundings where they make their real life decisions. Hence, by moving the experiment from a centralized location to the villages, the field context in setting increases.

Instead of using forms and money as the materials for the participants to deal with in the experiment, we used cardboard trees as depicted in Figure 5. Even though the trees are clearly not real trees, the participant recognise them as pictures of trees – which could create a priming effect towards a forest. The experiment thus has some field context in commodity. The priming effect found in the picturing of other goods, e.g. money and words (see sub-subsection 3.3.5), makes it likely that a similar effect exists in replicates of trees. The priming effect could therefore make the cardboard trees a valid substitute for real wood or charcoal. Additionally, the priming effect of irrelevant objects like forms and money make enough of an argument to shield the participants from these objects during the experiment.

Finally, instead of making the participants fill in their decisions in forms, they had to physically tip the cardboard trees to reveal their harvest decision. The task has field context as the participant fell cardboard trees to harvest, an act that demands some effort and removes the trees. Filling in forms would on the other hand reduce the trees to numbers on a sheet of paper; and would at the same time demand writing skills which is not needed for real life forest harvesting.

In the taxonomy of Harrison & List (2004), I claim the presented experiment to be a framed field experiment. The non-standard participant pool and some field context in setting, commodity and task make the experiment move down the categories of Table 2, which, according to Harrison & List (2004), increases the external validity of the experiment.

4.6 Scrutiny

The experiment is designed to allow the participants to reveal their choices anonymously from the researcher and the other participants. As depicted in Figure 8, the physical setup of the experiment is in two rooms: the game area and the waiting area. The game area is where the participants make their individual decisions and receive their collective information. The waiting area is where the participants are waiting for their turn to visit the game area. It is not possible to see the game area from the waiting area. The monitor is responsible for the game area and is therefore responsible for noting the participants' choices and for informing the group. The enumerator is responsible for the waiting area and therefore for ensuring that the participants do not communicate, or otherwise violate the rules of the experiment.[16]

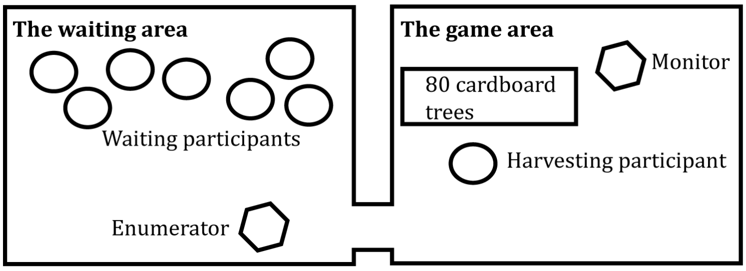

FIGURE 8 THE WAITING AREA AND GAME AREA

In this experimental design, some scrutiny is necessary as the monitor must collect the participants' harvest decisions and therefore observe the participant tipping down cardboard trees. I should therefore expect some pro-social bias in my data according to the findings of Lusk et al. (2006). My variable of interest is, however, not absolute harvest rates, but relative harvest rates. I compare harvest rates between the four games and I compare harvest rates across individual characteristics and group characteristics. Therefore, if I assume that there is no bias in the pro-social bias, i.e. that the pro-social bias is not more prevalent in certain games or among certain characteristics: the pro-social bias should not influence my conclusions.

The assumption might be strong, particularly in relation to individual characteristics, as scrutiny might systematically affect individuals differently. To mitigate the pro-social bias, the design therefore tries to convince the participants that the decisions are not linked to their identity. Each participant is given a small sticker with a number to represent his or her identity throughout the experiment (as depicted in Figure 19 of Appendix IV). Their decisions are solely linked to this number, and not to the participant's name. Also, unlike other experimental studies (Ostrom et al. 1994; Cardenas 2003), the behaviour of the participants was not recorded and the communication in the CFM treatment was not scrutinized.

[16] The role of the researcher is often an important topic in conducting fieldwork and consequently often occupies large part of qualitative studies. In the experiments conducted in Tanzania, I had merely a minor role in interacting with the participants, and even more important: the same role in the visited villages. The scrutiny effect of my presence should thus be negligible and indistinguishable in all seven villages. I therefore do not devote more space to the issue than this footnote.

Scrutiny is normally recognised at the individual level, but it might be just as important on more aggregated levels. Even though the participants' individual harvest decisions are not announced, the collective harvest of the group is announced at the end of each round. The perceived scrutiny of the group might therefore be stronger than the perceived scrutiny of the participant. This close scrutiny might create a collective pro-social bias as the total harvest is announced aloud; not only to the group, but also to the enumerator and the researcher. The group's perceived anonymity might therefore seem low. The announcement might also create a feeling of a pro-social competition among the experiment groups in a village.

In addition, scrutiny might create a bias towards pro-social behaviour at the village level. If the villagers feel ownership and pride for their village, or think they are competing against other villages to attract of some kind of forest conservation aid, they might purposely behave in a more pro-social manner to "show themselves from their best side". Little research has been done to examine these collective pro-social biases, but they might influence my findings.

A final note is that the quest to fight the scrutiny effect might create a more artificial setting, that reduces the external validity rather than increasing it. Related to forest use in small Tanzanian communities, it might be realistic to impose some scrutiny in the experiment, as real forest use will be scrutinized by your fellow villagers and even possibly by the village leaders. Therefore, pro-social bias as a result of scrutiny should be observed in real life, and therefore could be an issue the experimental design should include as well.

4.7 DATA ANALYSES

4.7.1 DEPENDENT VARIABLE

The only variable collected in the experiment itself – revealing the participants' behaviour – is the amount of trees each participant decided to harvest in each round. 288 participants completed the experiment and each experiment consists of 12 rounds, thus the total number of observations should be 3 456. But, as some experiments were cut short because of stock depletion, the total number of observations is 3 272. Furthermore, as I lack questionnaire data on one participant (the intoxicated participant who went missing), the number of observations is limited to 3 260 when this data is utilised. When presenting the results in section 5, the number of observation and the dependent variable used is stated. When analysing subgroups, the number of observations would naturally further decrease. An indication of the size of the subgroups is given in Table 5.

The observations report a participant's harvest in a given round, a number from zero to five, but do not reveal the decision in relation to the forest stock. If, for instance, a participant facing a forest stock of 10 trees decides to harvest one tree, the harvest is in relative terms higher than a participant that harvests two trees from a forest stock of 50. The latter number is higher in absolute terms, but due to the experimental design, the number is only 40% of the maximum possible harvest. The former number is the maximum possible harvest for a forest stock below 16 trees. I focus on relative harvest and consequently construct a harvest rate variable that reports the harvest decision as a number between zero and one, where one is the maximum possible harvest calculated from the harvest decision and the remaining forest stock. I consider this the more correct variable as it to a greater extent measures the participant's consideration of the negative externality imposed on others by harvesting (cf. sub-subsection 3.2.1). Considering a participant who harvests 40% of the possible harvest from an abundance of trees as more selfish than a participant who harvests as many trees as possible from a small forest could therefore produce wrong results.[17]

The dynamics of the forest stock demand some calculations when undertaking the experiment. After the participants have revealed their harvest decisions, the numbers are summarized and withdrawn from the forest stock, and then the growth rate is added. Some calculation errors were done in the field. Consequently some harvest rates are above 1, and some forest stocks become negative when I recalculate the observations. The stock declined to -3 in three cases and to -1 in one case, and there are 14 observations (of 3 272) were the harvest rate is above one. I choose to keep the observations as they are because they reflect the participants' decisions based on the information they had at the time. That the stock becomes negative or that forest users make impossible harvests is unrealistic, but irrelevant for the data analysis.

4.7.2 Confounding factors

The random nature of the experimental design should make the treatments independent from confounding factors on village, group and individual level. The participants are randomly distributed into the four games and all the three treatment games are played at least once in each village, in a random sequence. I should therefore expect that ordinary least squares regression suffices. Accordingly, the first column of the regression output tables in section 5 reports OLS estimates.

As discussed in sub-subsection 4.1.1, the village sample consists of relatively few villages with a large variation in characteristics. The characteristics, along with other unobserved characteristics, could affect the behaviour of the participants. In fact, comparing the participants' total harvest rates to the specific village in which the experiment was carried out, finds some correlation.[18] Even though the games are randomly distributed among villages and the three main groups are present at least once in each village, the distribution is not even. When the PES treatment is played twice in one village and only once in another, where for instance the CAC treatment is played twice, the village specific factors influence the treatments differently. Also, the treatments might affect the participants in different villages differently. Furthermore, sub-subsection 4.1.2 mentions self-selection and "expectation" biases that depend on the inviters, and therefore are village specific. The assumption in the paragraph above might subsequently not hold: village specific factors might influence the results.

Accordingly, the second and third columns of the section's regression output tables report estimates from two models that deal with the issue of inter-village heterogeneity: OLS with clustered standard errors and village fixed effects. The former recognises the intra-class correlation and clusters the standard errors by the seven villages. The model does not alter the estimates, but produces robust standard errors that are more appropriate if the above assumption does not hold. The latter similarly controls for village specific factors, observable and unobservable, affecting the behaviour of the participants. Recent forest discussions and conflicts in a village might for instance influence the observed behaviour in the experiments. I assume village fixed effects have the same influence on behaviour in all the four games. The models are named "OLS", "clustered by village" and "village fixed effects" throughout the thesis.

[17] Annex III displays my general finding using absolute harvest rates, and show that although the coefficients vary, the choice does not alter the conclusions drawn in sections 5 and 6.

[18] The loneway command in Stata 11 produced an ICC value of 0.15. Additionally, Table 25 in Annex I shows that the villages are significant in determining the participants' harvest rates.

4.7.3 Models

The model for analysing research question three is given in Equation [4]. The model explains participant *i*'s (*i*=1,…,288) harvest rate (*x*) in round *j* (*j*=1,…,12), participating in experiment *m* (*m*=1,…,36) by the given independent variables, and the error term and constant term. The reasoning for the independent variables is given in sub-subsection 3.4.3, and will be explained further in subsection 5.3. The variable *age* is squared to include the possibility for a quadratic relationship. The model includes the possibility of village fixed effects, γ_k, for village *k* (*k*=1,…7). The independent variables will furthermore be tested for multicollinearity.

$$x_{ij} = \alpha + \gamma_k + \beta_1 age_i + \beta_2 age_i^2 + \beta_3 gender_i + \beta_4 femalegroup_m + \beta_5 children_i + \beta_6 trust_{im} + \beta_7 tribe_m + \beta_8 native_{im} + \varepsilon_{ijm} \quad [4]$$

In analysing research question two, another model – presented in Equation [5] – is more feasible. In this model, the dependent variable (y_i) is participant *i*'s mean harvest rates in part two of the experiment minus the participant's mean harvest rate in part one. The variable consequently measures the change in the participants' behaviour from part one to part two. The independent variables of interest are the dummy treatment variables, *CAC*, *PES*, and *CFM*, reporting the effect of each of the three treatments on the dependent variable. The variables are a variable set in a categorical variable, where the fourth category is experiments with open access in both parts of the experiment. The three treatment variables thus report the effect of the treatment relative to the open access control group, consequently controlling for learning effects. The *sequence* variable controls for the possibility that the sequence in which the experiments are conducted in a village might matter. This variable is discussed at the end of subsection 5.2. As in Equation [4], the model includes the possibility for village fixed effects, γ_k.

$$y_i = \alpha + \gamma_k + \beta_1 CAC_m + \beta_2 PES_m + \beta_3 CFM_m + sequence_m + \varepsilon_{ijm} \quad [5]$$

The model to be used in testing H12 in research question one is given in Equation [6]. The dependent variable is the same as in Equation [4], whereas the independent variables report stated relative forest use (*rfuse*), stated absolute forest use (*afuse*), and stated commercial forest use (*cfuse*). The variables are further explained in sub-subsection 5.1.2.

$$x_{ij} = \alpha + \gamma_k + \beta_1 rfuse_i + \beta_2 afuse_i + \beta_3 cfuse_i + \varepsilon_{ijm} \quad [6]$$

Variations of the models will also be used to analyse the research questions, along with bivariate analysis. The variables and models will be explained in each reporting table of section 5.

4.8 Summary

In subsection 3.5, I summarised the factors influencing the behaviour of participants in an experiment with Figure 3, repeated below. The left side consists of factors making up the design of the experiment, while the right side are the factors the participants, community, and the commodity in question bring into the experiment.

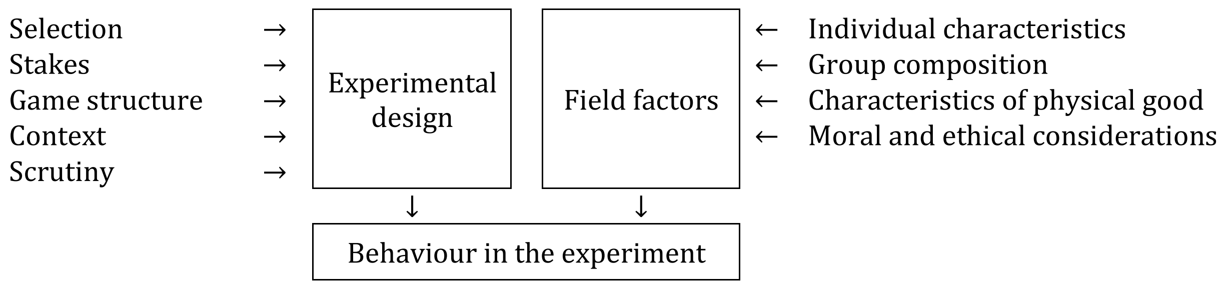

FIGURE 3 AN OVERVIEW OF FACTORS AFFECTING A PARTICIPANT'S BEHAVIOUR IN AN EXPERIMENT

In this section I have examined how the five factors on the left side of the figure affect the internal and external validity of this thesis' experimental design. If the factors ensure that the observed harvest rates are due to the deliberate actions of the participants – and they collectively make up a design that is able to observe both the cause of the action and the effect of the action – the experiment has internal validity. If the factors on the left side take into account the factors on the right side, the experiment has external validity.

The random sampling of 288 participants ensures that the individual characteristics of the participants are representative for the population in the sampled villages. The sampled villages are all forest dependent, but vary substantially in their characteristics. The random assignment into experiment groups ensures that there should be no biases in group composition.

The stakes of the experiment ensures that the participant does not become satiated and that the reward they receive are a consequence of their actions, which are vital for the internal validity of the experiment. The use of indirect monetary reward through tree imitation takes the physical appearance of the good into account.

The game structure reveals cause-and-effect-relationships between the treatments and the participants' behaviour, which enhances the internal validity. Furthermore, the stock's subtractability and exclusion characteristics along with its growth rate take the attributes of the physical good into account.

The context involves the visible attributes of the physical good, task, and the surroundings. The relevant context could therefore prime the participants in taking moral and ethical considerations into account, in addition to other considerations as culture and tradition.

Lastly, a low level of scrutiny in my experiment and an interest in relative harvest rates makes the scrutiny factor not as disturbingly as it can be. Still, the data should contain some pro-social bias, particularly at the group and village level.

Overall, I argue that the experimental design has enough external and internal validity to find a cause-and-effect relationship of the experiment's treatments, and predict the real life behaviour of the population in relation to forest use. The first two subsections of the following section will examine if the claim holds.

5 RESULTS AND DISCUSSION

This section presents the results and discusses them, organised by the research questions. Subsection 5.1 deals with internal and external validity, subsection 5.2 deals with the forest management regimes, and subsection 5.3 deals with individual and group characteristics affecting behaviour in the experiment. Finally, subsection 5.4 summarises the findings and discusses implications.

5.1 THE EXPERIMENT'S VALIDITY

RQ1 Does the experiment possess sufficient external and internal validity?

Research question one refers to the external and internal validity of the experimental design. For the findings in 5.2 and 5.3 to be valid, the experiment needs to have high internal and external validity. Section 4 argued for the validity of my experiment. This section assesses the arguments with the results from the experimental study.

5.1.1 INTERNAL VALIDITY

H11 The change in behaviour from the 1st part to the 2nd part of the experiment is due to the externally imposed treatments.

To test hypothesis H11 I need to test if the experimental design presented in section 4 is: (1) identical for all games in the first part of the experiment, and (2) identical in the first and the second parts of the experiment, except for the treatments imposed. The two conditions are depicted in the matrix of Table 9. Hypothesis H11 tests the two conditions and therefore the claim that the experiment possesses enough internal validity to establish a cause-and-effect relationship.

TABLE 9 BEHAVIORAL CHANGE WITHIN AND BETWEEN THE TWO PARTS OF THE EXPERIMENT AND THE CONSEQUENCES FOR INTERNAL VALIDIDTY

	1st part	2nd part
1st part	Little variation in behaviour across games necessary for internal validity to hold. (1)	-
2nd part	Little variation in behaviour in the open access game necessary for internal validity to hold. (2)	Internal validity not observable. The variation in behaviour should be due to the externally imposed treatments.

Due to the experimental design, testing the internal validity should be unnecessary as the participants are randomly distributed into the four games and the causal effect of the treatment is observed. Still, as the village sample is small and the distribution of treatments on villages is unbalanced, biases could arise due to village specific characteristics (as discussed in sub-subsection 4.7.2). Furthermore, as mentioned in sub-subsection 4.1.2, self-selected participants are likely to be present in the dataset. Testing the internal validity of the experiment reveals the significance of these confounding factors, and consequently strengthens or weakens the results of subsections 5.2 and 5.3.

To examine the first condition, Table 10 presents the aggregated mean harvest of the participants in the experiment's first part by the control and treatment groups. It further presents the results of a t-test examining the difference between the control group and each of the three treatment groups. The differences are negligible and insignificant in all three cases.

TABLE 10 COMPARISON OF HARVEST RATES IN PART ONE BY TREATMENT GROUPS AND CONTROL GROUP

Groups	Mean	Difference	N
Open access	0.453 (0.02)	-	272
CAC	0.462 (0.02)	-0.01 (0.03)	424
PES	0.457 (0.02)	-0.004 (0.03)	432
CFM	0.486 (0.02)	-0.033 (0.03)	440

Notes: t-test comparing the harvest rates in the three treatments with the open access game. Difference between the means are stated. Standard errors in parentheses.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

Graphically, Figure 9 depicts the changing mean harvest rates for the participants through the six rounds of the first part of the experiment. The figure shows that the mean harvest rate is declining as more rounds are played in all four groups. The slopes seem to be similar, even though the CFM treatment has a somewhat gentler slope. Due to the large 95% confidence interval, however, the functions are still insignificantly different. The functions start at a similar mean harvest rate in round one and end within the same 95% confidence interval in round six.

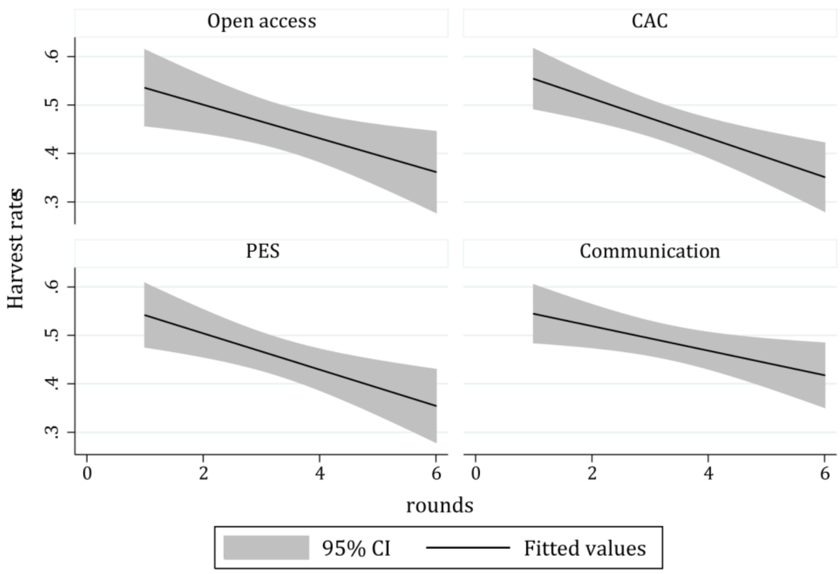

FIGURE 9 THE MEAN HARVEST RATE IN EACH ROUND, PART ONE OF THE EXPERIMENT

The findings in Table 10 and Figure 9 indicate that the behaviour in the first part of the experiment is similar, but not identical, in the four games. The difference should be due to the small subsample size, especially of the open access game. Still, since the difference is small and the mean harvest rates are not significantly different, the experiment exhibits enough internal validity to observe substantial behavioural change in the second part.

The second condition, that the first and second parts are identical except for the treatment, does not necessarily hold in my experiment. The participants could in part two behave differently merely because of the knowledge they have acquired during the first part, i.e. due to the learning effect. The experiment therefore includes the open access game where the experimental design is identical in both parts. The observed behavioural change in this game is assumed to be due to learning effects. My main interest is how the behaviour changes from part one to part two, relative to the open access game. In this way, the experiment controls for the learning effect and only examines the effect of the treatment. The second part of the open access game therefore serves as a benchmark when discussing the three treatments.

By design, the four games are identical in the first part of the experiment, and the only observed difference from the first part to the second part of the experiment is the treatment imposed in three of the four games. My results indicate that the harvest rates in the four games are insignificantly different in the first part. And the open access game played in part two controls for any learning effect or other effects that might change behaviour from part one to part two. I therefore find evidence supporting hypothesis H11.

5.1.2 External validity

H12 The behaviour of the participants in the presented experiment is predictive for their real life behaviour.

Hypothesis H12 is more difficult to prove than H11 as I have not observed the real life behaviour of the participants. I do, however, have the stated forest behaviour of the participants (cf. questions 3-5 in Appendix II). The stated answers for the questions refer to: (1) the family's perceived use of forest products relative to the other village residents (the higher the number, the lower the use relative to others in the village), (2) the participant's effort exerted in collecting forest products (the higher the number, the more times the participants collects forest products each week), and (3) if the participant uses forest products commercially (a binary variable where 1 is commercial use). I assume the stated answers to correspond with *de facto* behaviour. Therefore, correlation between behaviour in the experiment, i.e. the participants' harvest rates, and the stated forest use is an argument for the external validity of the experiment.

Since correlation should be found for all participants irrespective of game, village, age, etc. for H12 to hold, OLS estimation should be sufficient. In column one of Table 11, I therefore regress the stated answers for the three questions as variables explaining the participants' harvest rates through the experiment. In case the assumption that OLS should suffice does not hold, I cluster the standard errors by villages and controls for village fixed effects in column two and three respectively.

TABLE 11 EXPLAINING EXPERIMENTAL BEHAVIOR AS A FUNCTION OF STATED REAL LIFE BEHAVIOUR

	OLS	Clustered by villages	Village fixed effects
Relative forest use	-0.100***	-0.100***	-0.100***
	(0.01)	(0.01)	(0.01)
Absolute forest use	0.018***	0.018**	0.018***
	(0.00)	(0.01)	(0.00)
Commercial forest use (b)	0.133***	0.133**	0.108***
	(0.02)	(0.04)	(0.02)
Constant	0.558***	0.558***	0.563***
	(0.03)	(0.05)`	(0.03)
R^2	0.091	0.091	0.078
F-test (p-value)	0.000	0.000	0.000
N	3260	3260	3260

Notes: Dependent variable: Each decision's harvest rate. (b) = binary variable. Standard errors in parentheses.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

The negative coefficient in the first row reflects that the higher the stated relative forest use is, the higher the harvest rate in the experiment.[19] Similarly, the more often the participant states to be collecting forest products, the higher the harvest rate. Also, forest users that sell forest products have higher harvest rates in the experiment than those that do not. The latter finding is even stronger if I exclude participants that are not forest users. The relationships are intuitive and highly significant.

Even though the independent variables are correlated, I find no multicollinearity.[20] Examining the variables bivariately, as depicted in Figure 10, produces the same relationships as Table 11: the correlation is negative in question three and positive in the other two.

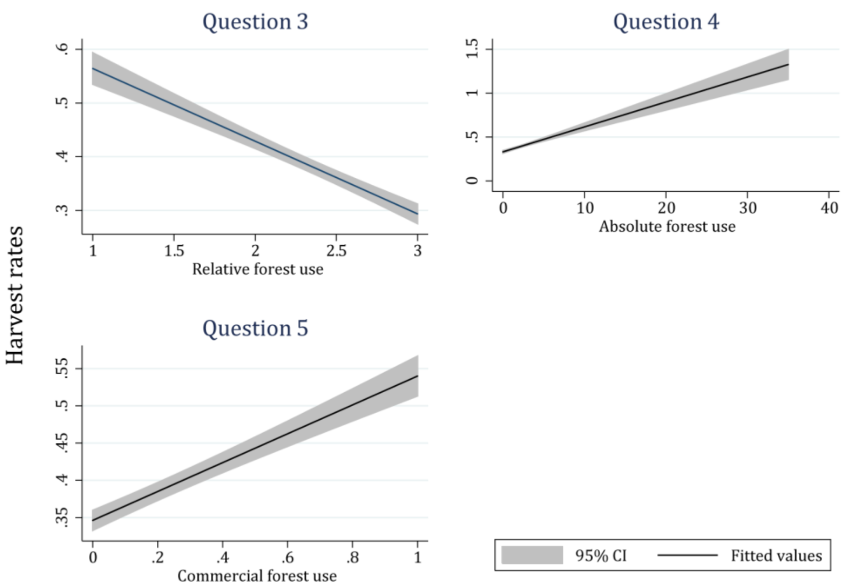

FIGURE 10 TOTAL MEAN HARVEST RATES' CORRELATION WITH THE THREE QUESTIONS

[19] The coefficient is negative due to the wording of the question (cf. question 3 in Appendix II).
[20] The vif command in Stata 11 produced VIFs between 1.09 and 1.11.

An argument against that the results indicate external validity is the possible bias in the stated forest use of the participants. As the interview is conducted after the experiment, the aggressive harvesters might feel inclined to "justify" their behaviour in the experiment by stating real life forest use that corresponds to the experimental harvest they have just revealed. Still, that most participants are able to do so consistently in all of the three questions is unlikely. Furthermore, it would also require the participants that harvest little in the experiment to state little real life harvest as well. The incentive to do so is not so clear. I consequently find the assumption that stated forest use is correlated with actual forest use a reasonable one. That the behaviour in the experiment significantly correlates with the stated real life behaviour and that the relationship is intuitive, is thus a strong argument for the external validity of the experiment. I consequently find evidence in support of hypothesis H12.

The external validity of the treatments themselves is not testable and is consequently left to be argued for. The issue is discussed in sub-subsection 5.4.1.

5.2 FOREST USE AND THE GAME STRUCTURE

RQ2 How does the game structure, simulating the management regimes, affect the forest use?

Research question two refers to the impact the treatments have on the participant's behaviour. I will first briefly present the results, before the following sub-subsections analyse and discuss the findings in accordance with the hypotheses and games.

Table 12 depicts the mean harvest rates in the two parts. Column one depicts the same means as in Table 10, while the third column depicts the change in mean harvest rate from part one to part two. In all four cases the rate decreases, but the change is not significant in the open access game.

TABLE 12 MEAN HARVEST RATES IN PART ONE AND PART TWO, BY GAME

Game	Harvest rate in 1st part	Harvest rate in 2nd part	Difference (2nd part–1st part)	N (1st part/ N 2nd part)
Open access	0.453	0.41	-0.043	272/280
	(0.02)	(0.02)	(0.32)	
CAC	0.462	0.29	-0.177***	424/480
	(0.02)	(0.01)	(0.22)	
PES	0.457	0.362	-0.095***	432/464
	(0.02)	(0.02)	(0.26)	
CFM	0.486	0.238	-0.248***	440/480
	(0.02)	(0.01)	(0.02)	

Notes: t-test comparing the mean harvest in the two parts of the experiment, by game. Difference between the means is stated. Standard errors in parentheses.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

Figure 11 shows the dynamics of mean harvest rates through the rounds. The graphs on the left side of the figure are harvest rates for the first part of the experiment in the four games. The graphs on the right side are mean harvest rates for the second part.

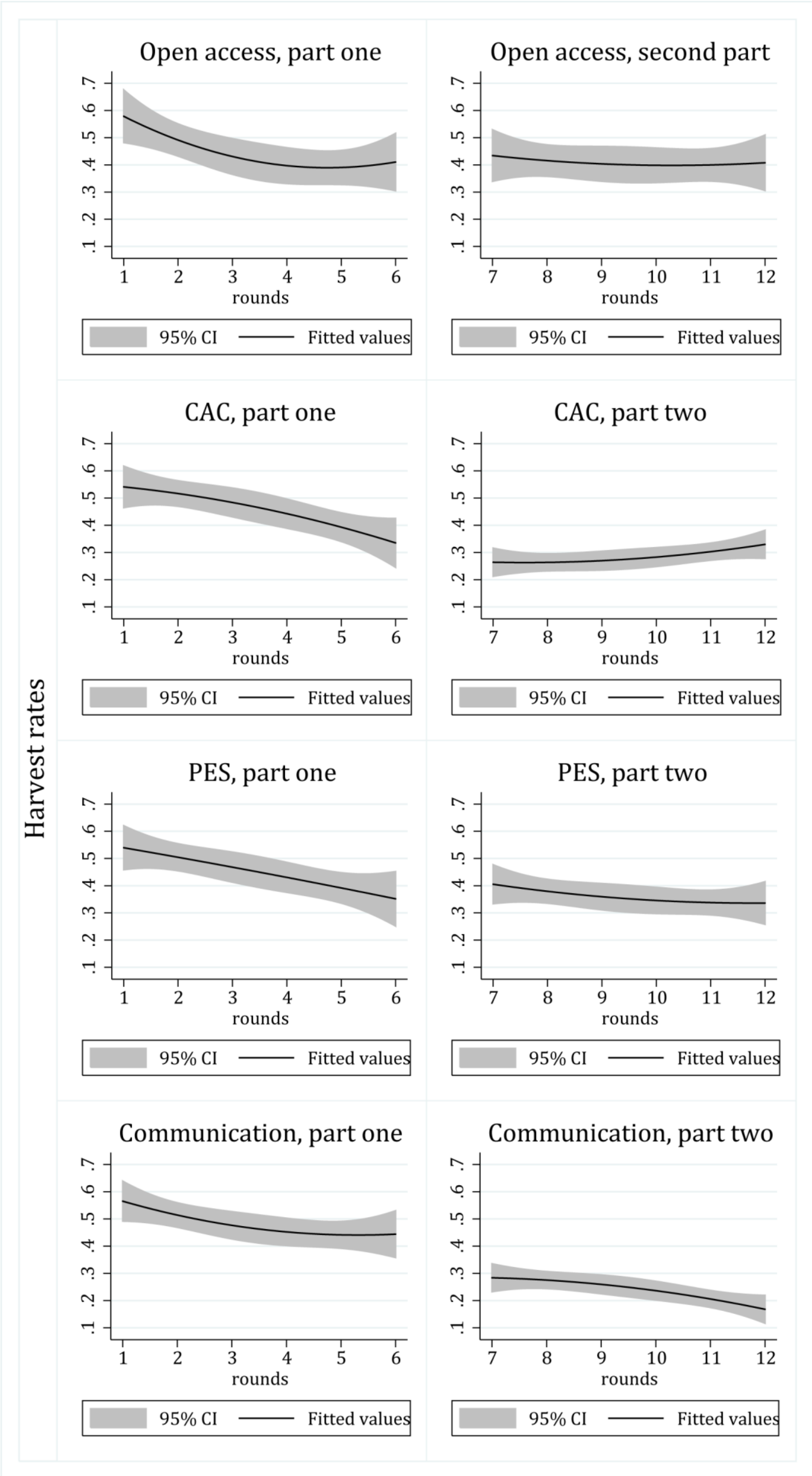

FIGURE 11 THE CHANGE IN M HARVEST RATES, BY GAME AND PART

Table 13 takes the learning effect into account and assumes the behaviour in the games to be the same in part one of the experiments. A t-test considers the difference in the mean harvest rates of the three treatments in part two to the mean harvest rate of the open access game in the same part. The table shows that the CFM treatment imposes the largest change in harvest rates, followed by the CAC treatment, and the PES treatment (significant at the 10% level).

TABLE 13 COMPARISON OF THE MEAN HARVEST RATES IN PART TWO BY GAME

Game	Mean	Difference	N
Open access	0.41 (0.02)	-	280
CAC	0.286 (0.01)	-0.124*** (0.02)	480
PES	0.362 (0.02)	-0.0484* (0.02)	464
CFM	0.238 (0.01)	-0.172*** (0.02)	480

Notes: t-test comparing the respective means of the main games with the open access game. Difference between the means is stated. Standard errors in parentheses.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

Table 14 estimates the model presented in Equation [5]. The dependent variable is the change in mean harvest rate from part one to part two of the experiment. The independent variables are the relative change in the three treatments, using the change in the open access game as the baseline, together with *sequence.*

TABLE 14 REGRESSING CHANGE IN HARVEST RATES ON TREATMENTS, RELATIVE TO THE OPEN ACCESS GAME

	OLS	Clustered by villages	Village fixed effects
CAC	-0.172***	-0.172**	-0.227***
	(0.04)	(0.07)	(0.05)
PES	-0.068	-0.068	-0.124***
	(0.04)	(0.07)	(0.05)
CFM	-0.222***	-0.222***	-0.268***
	(0.04)	(0.04)	(0.04)
Sequence	-0.014	-0.014*	-0.015*
	(0.01)	(0.01)	(0.01)
Constant	-0.004	-0.004	0.043
	(0.05)	(0.03)	(0.05)
R^2	0.115	0.115	0.143
F-test (p-value)	0.000	0.008	0.000

Notes: Dependent variable: difference in harvest rates between part one and two. Standard errors in parentheses. N=288.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

I will discuss the estimates for the games separately in the next four sub-subsections. The last independent variable, *sequence*, is the sequence the experiments were conducted in a village. In the variable, the first experiment conducted in a village is numbered 1, the next is numbered 2, etc. The variable is included to control for the sequence effect on the experiments, which is not village specific and might affect the games differently. The coefficient is significantly (at the 10% level) negative in two of three models.

5.2.1 THE OPEN ACCESS GAME

H21 The open access game induces the highest forest use.

Hypothesis H21 states that, *ceteris paribus*, harvest decisions made in the open access game are higher than harvest decisions made under the three treatments. The variable of interest is then either the mean harvest rate in part two, or the difference in harvest rates from part one to part two. Table 12 shows that the open access game has the highest mean harvest rate in the second part. Figure 11 depicts the same finding graphically, while Table 13 and Table 14 show that none of the three treatments have higher mean harvest rate than the open access game. In Table 14, the treatments all have significantly lower harvest rates than the open access game with village fixed effects. In the OLS and clustered models, the PES treatment is just insignificantly different from the open access games. The CAC and CFM treatments have significantly lower mean harvest rate than the open access game also in these models. Taken together, the results are supportive of hypothesis H21.

Table 12 find that the mean harvest rate for the open access game is lower in part two than in part one, but the finding is not significant. This could be explained by Figure 11, where the mean harvest rate seems to decline through the rounds of part one and stabilise at about 0.4. In part two, the harvest rate continues at the 0.4 harvest rate. The initial decline – only observed in the first part – could be explained by the learning effects.

In subsection 3.2, the predicted outcome (the Nash equilibrium) of the social dilemma in the open access situation is to maximize private payoff, as depicted in sub-subsection 4.4.1. The results presented above show that even though the open access game arguably induces the highest forest use, the behaviour is not as selfish as the predictions forecast. The finding could be due to the pro-social bias discussed in subsection 4.6. But, that *homo economicus* behaviour is not observed, even in the absence of institutions, is supported by Ostrom (2006), Cecchi & Bulte (2013) and Henrich et al. (2010), among others. Also, the results could be supportive of the model by Levitt & List (2007) in sub-subsection 3.3.5 that highlights the importance of other considerations than the strict monetary ones. The open access situation is thus the inferior game in this experiment, but still performs better than the game theoretical prediction, which is in accordance to the literature.

5.2.2 THE CAC TREATMENT

H22 The CAC treatment induces higher forest use than the PES treatment.

Hypothesis H22 sets the CAC and PES treatments up against each other and states that harvest decisions made in the CAC treatment are *ceteris paribus* higher than harvest decisions made in the PES treatment. Table 12 shows that the CAC treatment has lower mean harvest rate than the PES treatment in part two. There is a significant decrease in the mean harvest rate in both treatments, but the decrease is stronger in the CAC treatment. Figure 11 confirm that the immediate change is stronger in the CAC treatment, but at the same time, the figure depicts an increasing mean harvest rate through round 7 to 12 in the CAC treatment. The mean harvest rate in part two of the PES treatment is decreasing.

In Table 14 the CAC treatment lowers the harvest rate by 17-23% relative to the open access game. The coefficient is significant at least at the 5% level, even with clustered standard errors. The PES treatment, on the other hand, lowers the harvest rate by 7-12%, relative to the open access game and the coefficient is only significant with village fixed effects. The finding is supported by Table 13. In short, I find evidence against hypothesis H22. The remaining sub-subsection discusses the findings regarding the CAC treatment, while sub-subsection 5.2.3 discusses the PES treatment.

In the CAC treatment the participants can legally harvest three trees each round, implying that a harvest rate at 0.6 is allowed if the stock is above 40 trees. If the stock is below 40 trees an even higher harvest rate is allowed. Therefore, a mean harvest rate after treatment of merely 0.3 is surprising (cf. Table 13). There could be numerous intertwined explanations for this finding, the following highlights three possible ones. First, the introduction of the rule could remind the participants that a low harvest rate is preferable for the group. That some measure is taken with the clear aim of decreasing harvest rates could simply invoke moral and ethical considerations that the participants did not take into account without any forest conservation measures. In the explanation, the treatment thus has a crowding in effect of morality.

Second, the introduction of the rule could decrease the belief that the stock will deplete. If the participants really want to maximize the group's payoff, but lack the belief that the others share the same goal, a credible measure to decrease the harvest rates could increase the participants' belief in reaching the goal. If sufficiently many share the new belief, the group might reach the equilibrium where the group's payoff is maximized. The treatment thus changes the participants' expectations of the behaviour of others, which again changes the participants' own behaviour.

A third possible explanation is that the mentioning of a punishment frightens the participants into lowering their harvest rates dramatically. Being punished is so unappealing that the participants prefer to "be on the safe side" and consequently lower their harvest rates below the set limit. The effect could be further strengthened if the participants do not perfectly understand the rule, or are not sure that they did. These three explanations are not a complete list of explanations, but they are feasible explanations, both in the experiment and importantly: also in real life.

The increasing harvest rates with the rounds in part two, as depicted in Figure 11, is also interesting. The increase appears to be significant and does not seem to be levelling out. CAC is the only game to have this trend, which could be because the discussed effects are just temporary. At some point a participant might "forget" that the low harvest rate is preferable for the group, lose faith in the continued existence of the stock, or understand that a higher harvest rate is allowed. The temporariness of the effects could be self-enforcing, as the more participants forget, lose faith or understand, the more participants will do the same. Another possible explanation is that the participants' perceived risk of being caught decreases when they observe that most (c. two-thirds) are not inspected. Further research, preferably through experiments with more rounds, is needed to reveal if the effects are temporary.

5.2.3 THE PES TREATMENT

The high harvest rates in the PES treatment might be the most puzzling finding in examining research question two. The treatment is the only one to introduce economic incentives in that it compensates 80% of each tree not harvested. The compensation at individual level could be the additional incentive needed for the participants to maximize the group's payoff. Also, the effects discussed as the two first explanations for the performance of the CAC treatment, could be applied in this treatment as well. Still, the change in harvest rate is – if significant – merely about half of what the CAC treatment induces. Additionally, as shown in Annex II, the subsample size of the PES treatment is the only treatment group not large enough to find a significant change in behaviour, further emphasising the small impact of the treatment.

A possible explanation for the poor performance of the treatment is that it crowds out moral considerations in the decision making process of the participants. While the monetary incentives are easy to understand in the other games, the PES treatment requires the participants to do more complicated calculations to foresee the payoff from their potential decisions. These calculations could transform the commodity from tree figures to money. The possible priming towards forests is then substituted by priming towards money.

The finding is not necessarily limited to the experiment. If PES is carried out the same way in real life, i.e. making private monetary incentives for forest users not to harvest trees, the same effect is relevant here as well. A harvested tree already has a price on the local market, but a living tree does not necessarily have a monetary price. A commodification of living trees could make forest users take economic considerations in greater extent into account and thereby crowd out other considerations; e.g. moral considerations. Crowding out of morality is found when economic incentives have been introduced in other potential markets as well (see for instance Gneezy & Rustichini (2000a), Gneezy & Rustichini (2000b) and Rodriguez-Sickert et al. (2008)).

Furthermore, the decision could now be perceived as not to be how many trees to harvest, but how many trees *not* to harvest. A participant that decides to not harvest one tree takes a cost of TZS 20 and could thus feel that with the now visible self-inflicted cost, the participant does enough to limit the harvest. Deciding not to harvest another tree would after all double the participant's cost.

To decrease the harvest rate in the PES treatment, the payoff structure could be altered. PES could be distributed as a collective reward, e.g. at the village level. A forest user choosing not to harvest a tree is thus not earning private money, but contributes to the community's total welfare. This could force the forest user's calculation process from merely considering private gains to also consider the welfare of the community. Additionally, the reward could be non-monetary, e.g. in the form of subsidised alternatives to forest products. In this way the "commodification" of the forest is less clear and therefore the crowding out of morality effect could be limited.

Another solution is to ensure proper incentives. In the experiment, the direct private payoff for not harvesting a tree is mere 80% of the private payoff received for harvesting a tree.[21] The dominant strategy presented in sub-subsections 3.2.2 and 4.4.1 is therefore not altered. Proper

[21] This does not include the increased potential payoff in future rounds achieved by decreased harvesting in the present round.

PES should after all compensate properly, as reflected by the title of Gneezy's & Rustichini's (2000b) paper: “Pay enough or don’t pay at all”. To create a viable incentive, the compensation for not harvesting trees should therefore be at least as high as the economic reward a forest user receives for the harvested trees. In this way, the predicted dominant strategy of the open access situation can be altered.

The PES treatment has two potential effects: (1) it reduces the cost of decreasing forest use, i.e. increases the incentive for deviating from the Nash equilibrium, and (2) it crowds out moral considerations that encourage pro-social behaviour. The experiments reveal the net effect of the treatment. If the former is strongest, the treatment should significantly lower the participants’ harvest rate. If the latter is strongest, the treatment should increase the participants’ harvest rates. Since the treatment’s impact on behaviour is barely stronger than the open access game and I am unable to distinguish the two (and other possible) effects, I am unable to make a clear conclusion on the treatment. Still, that PES performs the worst of the treatments is an argument against partly rewarding forest users for decreasing forest use by private monetary transfers.

5.2.4 THE CFM TREATMENT

H23 The CFM treatment induces the lowest forest use.

The hypothesis states that the CFM treatment induces *ceteris paribus* lower harvest rates than the three other games. In Table 12, the CFM treatment depicts both the lowest mean harvest rate of the second round and the greatest decline in mean harvest rate between the rounds. The change is significant. Figure 11 graphically depicts the strong decline in mean harvest rate, and additionally shows that the rate is further declining with the rounds.

Lastly, Table 14 shows that in all three models, the CFM treatment decreases the harvest rate the most. Imposing the treatment significantly lowers the harvest rate by 22-27%, relative to the open access game. Table 13 supports the finding. I consequently find evidence in support of hypothesis H23. In the following, possible explanations are discussed.

The three minutes of communication does not only allow the participants to agree on a common strategy, but it could also increase trust and belief in that the other group members will maximize the group’s payoff. A possible explanation for the well-performing treatment is therefore that the participants feel accountable to each other. Each round’s total harvest is announced, revealing the aggregated harvesting strategy of the other group members, making each participant able to evaluate if the rest of the group is cooperating, and if there is a tendency for free riding. If the participants foresee this aspect, they might be less inclined to free ride

Furthermore, allowing the participants to communicate could create a stronger feeling of ownership towards the forest stock. They could feel that they, as a group, are now responsible for the cardboard trees, thereby clearly showing that the conservation of the forest stock relies on them only. In the other two treatments, external measures attempt to reduce the harvesting, which could make the participants feel less needed in the process and therefore less influential in conserving the forest.

It is worth noting that the performance of the CFM treatment is dependent on the dynamics and trust within the group, likely more so than the other games (as will be discussed in sub-subsection 5.3.4). In visiting one village, for instance, an individual with a leading position in the village was selected as a participant in a CFM experiment. For the second part, the monitor got a clear perception that the leader took charge when communication was allowed, as might be expected. When observing the harvest decisions she saw a pattern in that most participants harvested low and at similar harvest rates. One participant stood out, though: the mentioned individual, who harvested the maximum possible harvest. The leader consequently received one of the highest payments throughout the experimental study. The finding is similar to the previously mentioned anthropological study of Brockington (2007), and could be explained by the elite capture also observed by Platteau (2004).

The CFM treatment lowers the harvest rates more than the other treatments. Still, the anecdote tells of a potential risk in transferring power to the local level in that local leaders might take advantage of their positions to maximize their own payoff at the cost of the group's payoff. All the possible explanations above could just as well apply in real life as in the experiment.

5.3 INDIVIDUAL AND GROUP CHARACTERISTICS

RQ3 How do individual and group characteristics affect the forest use?

Research question three refers to the characteristics the participants bring into the experiments, and to the group characteristics that is created by the random sampling. Understanding how these characteristics affect the participants' behaviour in the experiment is interesting as they could be taken into account to design well-performing forest management regimes. Therefore, also understanding how these characteristics affect the effectiveness of the three treatments is relevant.

The effect of individual characteristics on forest use will be examined collectively as the model depicted in Equation [4] of sub-subsection 4.7.3, and as bivariate analyses. This sub-section first presents the results from the model in Table 15 and briefly explains the findings below. The following three sub-subsections discuss the findings separately under the headings of the related hypotheses, while the last sub-subsection briefly discusses how the characteristics affect the effectiveness of the treatments.

TABLE 15 HARVEST RATES EXPLAINED BY INDIVIDUAL AND GROUP CHARACTERISTICS

	OLS	Clustered by villages	Village fixed effects
Gender (b)	0.083***	0.083	0.082***
	(0.01)	(0.05)	(0.01)
Femgroup	-0.011***	-0.011	-0.017***
	(0.00)	(0.00)	(0.00)
Age	-0.014***	-0.014***	-0.015***
	(0.00)	(0.00)	(0.00)
Age²	0.000***	0.000***	0.000***
	(0.00)	(0.00)	(0.00)
Distrust	-0.022***	-0.022	-0.008
	(0.01)	(0.01)	(0.01)
Children	-0.010***	-0.010**	-0.004
	(0.00)	(0.00)	(0.00)
Tribe	-0.001	-0.001	-0.022***
	(0.00)	(0.01)	(0.01)
Native	0.003	0.003	0.013***
	(0.00)	(0.01)	(0.00)
Constant	0.726***	0.726***	0.792***
	(0.06)	(0.19)	(0.06)
F-test (p-value)	0.000	-	0.000
R^2	0.033	0.035	0.035

Notes: Dependent variable: harvest rates in both parts of the experiment. (b) = binary variable. Standard errors in parentheses. N=3260.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

Table 15 summarises how four individual characteristics and two group characteristics affect the harvest rates of the participants. First, a significantly positive coefficient on the binary *gender* variable (0=male, 1=female), at least in the OLS and village fixed effects models, imply that women *ceteris paribus* harvest more than men. The related *femgroup* variable summarises the number of females in each experiment. The negative coefficient indicates that the higher proportion of females relative to men in the experimental group, the lower the harvest rate. This somewhat contradicts the first finding.

Secondly, the significantly negative coefficient on *age* indicates that older participants harvest less than younger ones. The *age²* variable is also significant and therefore necessary for including a quadratic relationship, even though the coefficient is negligible.

The *distrust* and *children* variables are taken from question seven and sixteen in the questionnaire, depicted in Appendix II. *Distrust* is the stated trust on a Likert scale the participant has for the village council. The lower the number, the higher the participant trusts that the village council is working for the village's interests. The negative coefficient indicates that the lower the trust, the lower the harvest rate. The finding is only significant in the OLS model, which should be expected. The variable, if valid, should vary by villages because of its dependence on village leadership. Consequently, the effect of interest is controlled for in the models in column two and three. A significantly negative coefficient on the *children* variable (except with village fixed effects) implies that with more children (<16 years), the participant harvests less. The variables are not directly relevant for my research questions, and are merely included to control for confounding factors. The findings will subsequently not be discussed in detail.

Tribe is a number between zero and eight, and states the number of participants in an experiment belonging to the village's major tribe[22]. *Native* is similarly a number between zero and eight stating the number of participants in an experiment who are born in the village. The two variables therefore concern the heterogeneity of the group. That the former is negative and the latter is positive is a puzzling result. The more participants in an experiment belonging to the same major tribe participating in the same experiment, the lower the harvest rate. And the more participants in an experiment born in the same village (the village where the experiment is conducted in), the higher the harvest rate. The coefficients are, however, only significant with village fixed effects.

The characteristics discussed are possibly correlated. A young participant might for instance be more likely to have children than an older participant. Therefore, examining the explanatory variables' correlation among themselves is important for the analysis. The Pearson correlations are depicted in Table 16.

TABLE 16 CORRELATION MATRIX OF THE INDEPENDENT VARIABLES

	Gender	Age	Children	Distrust	Tribe	Native
Gender	1					
Age	-0.046***	1				
Children	-0.035**	-0.083***	1			
Distrust	0.083***	0.043**	-0.086***	1		
Tribe	0.076***	-0.109***	0.139***	-0.109***	1	
Native	-0.036**	-0.322***	0.025	-0.114***	0.132***	1

Notes: N=287
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

All, except one pair of variables, are significantly correlated with each other. The presence of correlation among the explanatory variables could affect my results if the variables are highly correlated, making a case for including *children* and *distrust* as control variables. However, the coefficients in Table 16 are small, and I find no presence of multicollinearity when testing for this.[23] Accordingly, I trust the findings, but will be careful when discussing the size of the coefficients, particularly when performing bivariate analyses in the coming sub-subsections.

5.3.1 Age

H31 Younger individuals have higher forest use than older individuals.

Hypothesis H31 implies that younger participants should have higher harvest rates than older participants. The findings in Table 15 clearly support the hypothesis. The coefficient is negative, indicating a negative relationship between age and harvest rates. Figure 12 shows the bivariate

[22] The participants were asked if he or she belonged to the village's largest tribe. The questionnaire consequently allowed the participants themselves to consider what the village's largest tribe is. The use of the term "tribe" in Tanzania fits in with Whitten's & Hunter's (1990:311) definition of tribe as "a relatively small group of people (small society) who share a culture, speak a common language or dialect, and share a perception of their common history and uniqueness".

[23] The vif command in Stata 11 produced VIFs in the range 1.02 to 1.29.

relationship between the two variables. The figure shows that there is a negative relationship between age and harvest rates, but only in the age range 18-50. Above c. 50 years, the relationship is positive: older participants harvest more than younger participants.

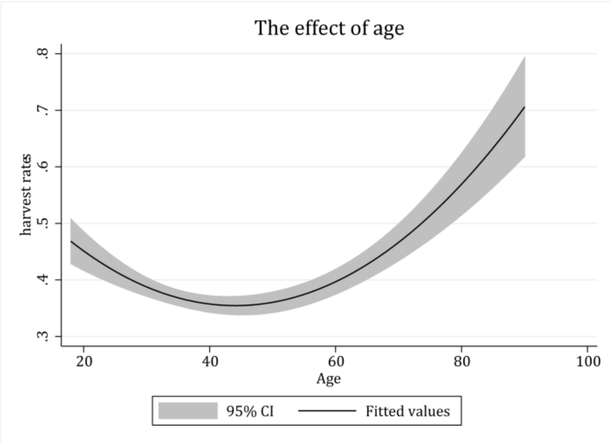

FIGURE 12 THE EFFECT OF AGE ON HARVEST RATES

The confidence interval expands to the right of the figure, indicating that there are relatively few old participants. In fact, as shown in Table 5, merely 5% of the participants are 65 years old or older.[24] Therefore, when controlling for the quadratic relationship, Table 15 still indicates a negative relationship. Figure 12 is, however, a warning that the finding does not necessary apply for the oldest individuals. Overall, I find evidence in support of hypothesis H31.

The finding is consistent with the literature presented in sub-subsection 3.4.3. Grossmann's et al. (2010) experimental study investigated how participants understand and foresee the development of a presented social conflict. They find that " with age comes wisdom" (Grossmann et al. 2010:7249), as the older participants were to a greater extent able to recognise uncertainty, others viewpoints, and limits to own knowledge. They therefore conclude that key decision making roles should be possessed by older individuals. Park et al. (2002) on the other hand, focus on the decline in cognitive abilities with age. Through experiments they found that older participants have slower processing speed and working memory than younger participants. This thesis' experiments require some cognitive abilities to understand the mechanisms and foresee potential outcomes. The two studies combined could explain the quadratic relationship found in Figure 12: older participants comprehend the CPR situation better, but declining cognitive abilities make the oldest participants incapable of making well-founded decisions. Wisdom and cognitive abilities are also needed when reasoning about CPR dilemmas in real life.

5.3.2 GENDER

H32 Male individuals have higher forest use than female individuals.

The vast literature on ecofeminism (some of which are referred to in sub-subsection 3.4.3) suggests that women are inherently better forest conservers than men. Table 15, however, suggests otherwise. According to the findings, women harvest about 8% more than men, although the coefficient is not significant with clustered standard errors.

[24] The age is based on stated age by the participants themselves. Some measurement error should therefore be expected.

A bivariate comparison of the mean harvest rates for men and women in both parts of the experiment, shown in Table 17, supports the finding. The t-test shows that the mean female harvest rate is significantly higher than the mean male harvest rate.

TABLE 17 DIFFERENCE IN HARVEST RATES BY GENDER

Gender	Mean harvest rate	N
Male	0.358 (0.01)	1675
Female	0.416 (0.01)	1597
Difference (male-female)	-0.058*** (0.01)	3272

Notes: t-test comparing the total mean harvest rate of males and females. The difference between the means is stated in the 3rd row. Standard errors in parentheses.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

Moreover, Figure 13 presents the distribution of the mean harvest rate in the first and second parts of the experiment by gender. The left graph shows that male harvest rates are distributed further to the left than their female counterparts, implying that more men than women have lower harvest rates for the experiment's first part. In the graph on the right side depicting harvest rates for part two of the experiment, the difference is not so clear. Still, there is a tendency for the male harvest rates to be lower than the female harvest rates. Again, however, the bivariate analysis does not take the correlation among the explanatory variables into account.

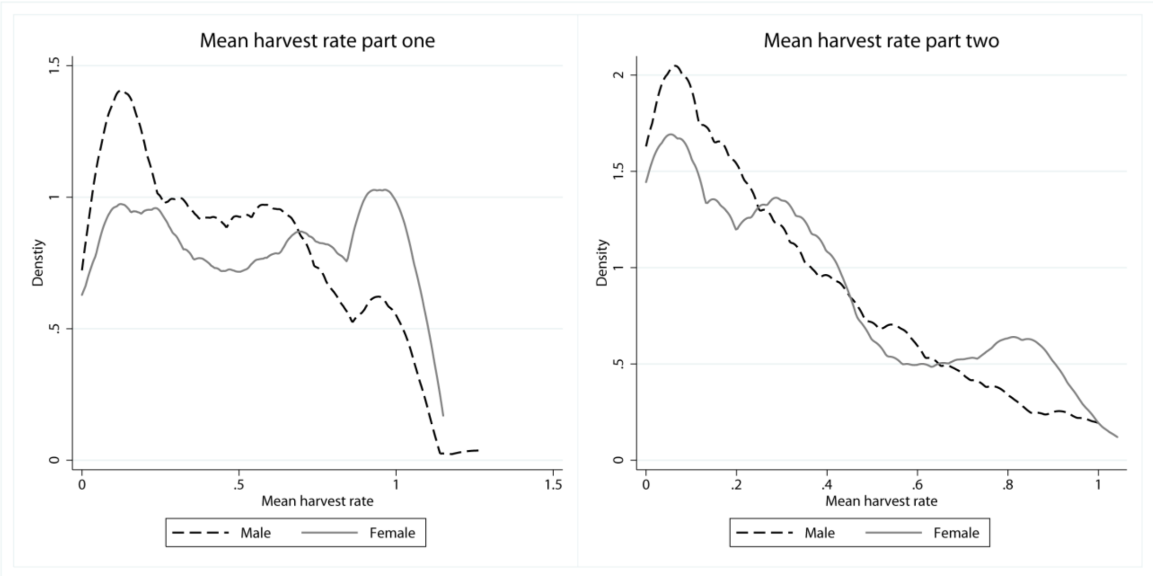

FIGURE 13 DISTRIBUTION OF MEAN HARVEST RATES IN THE TWO PARTS, BY GENDER

In looking at the group composition of gender, *femgroup* has a negative coefficients indicating that participants in experiment groups with more women harvest less than participants in groups where the majority is men. Table 18 depicts a t-test comparing mean harvest rates in feminine and masculine groups.[25] The table supports the finding in Table 15.

TABLE 18 DIFFERENCE IN HARVEST RATES BY GENDER COMPOSITION OF GROUP

Group	Mean harvest rate
Masculine group	0.341
	(0.01)
Feminine group	0.275
	(0.01)
Difference	0.066***
(Masc.-fem.)	(0.02)

Notes: t-test comparing the total mean harvest rates of feminine and masculine groups. The difference between the means is stated in the 3rd row. Standard errors in parentheses.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

The finding is interesting and opens up for further investigation on the individual effect of gender versus the effect of gender composition. In relation to hypothesis H32, however, *femgroup* is of lesser importance, as the focus is on the individual effect of gender. Therefore, on the basis of the findings in Table 15, Table 17 and Figure 13, I find evidence against hypothesis H32. Sub-subsection 5.3.4 further examines the effect of gender on the performance of the three treatments, while sub-subsection 5.4.2 elaborates on the possible implications of the two findings.

5.3.3 Group heterogeneity

H33 Heterogeneous groups have higher forest use than homogenous groups.

In Ostrom's (1999:10) discussion on heterogeneity she cautiously argues that groups with "diverse cultural backgrounds" may find it more challenging to organize themselves in managing forest resources. Other studies mentioned in sub-subsection 3.4.3 support the theory. The hypothesis thus indicates that groups with greater heterogeneity have higher harvest rates than others.

The assumption in this thesis is that the more participants in an experiment belonging to the village's major tribe or born in the village, the more homogenous is the group. Participants not belonging to the major tribe could belong to any of Tanzania's numerous tribes, and participants not born in the village where the experiment is conducted could have been born in any other village.

According to the findings in Table 15, the effect of the two heterogeneity variables is contradicting. The variables' coefficients are, however, only significant with village fixed effects. In a bivariate examination, Table 19 depicts a t-test of the difference in total mean harvest rate

[25] Masculine groups have less than five female participants. Feminine groups, subsequently, have more than four female participants.

between heterogeneous groups and homogenous groups in terms of *tribe* and *native*.[26] The test shows that the groups differ significantly at the 1% level, but with opposite signs than indicated in Table 15.

TABLE 19 DIFFERENCE IN HARVEST RATES BY TRIBE AND NATIVE

Category	Tribe	Native
Heterogeneous	0.403	0.364
groups	(0.01)	(0.01)
Homogenous	0.368	0.403
groups	(0.01)	(0.01)
Difference	0.035***	-0.039***
(heter.-homo.)	(0.01)	(0.01)
N	3272	3272

Notes: t-test comparing the total mean harvest rates by group heterogeneity. The difference between the means is stated in the 3rd row. Standard errors in parentheses.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

The double contradictory finding, along with insignificant coefficients for both variables in the OLS model with and without clustered standard errors, leaves me unable to neither oppose nor support hypothesis H33.

The ambiguity of this "ethnic heterogeneity" in determining participants' behaviour could be illuminated by the modern history of Tanzania, briefly presented in subsection 2.3. The *ujamaa* policies aimed at creating a Tanzanian identity by composing new villages and imposing the language of Kiswahili. The policies thus undermined the importance of local ethnicity. If the *ujamaa* policies were successful, individuals' place and tribe of origin should be irrelevant for them to cooperate as a group. This could explain the lack of clear results in the support of the existing literature on heterogeneity.

Group heterogeneity is not only measurable in ethnicity, as Ostrom (1999) emphasises in sub-subsection 3.4.3. Olson (1971) focuses on income or wealth differences within a group. I would therefore also be interested in examining this kind of group heterogeneity. Question eight tries to separate the participants by wealth. But, as shown in Table 5, about 22% of the participants stated to belong to the poorest third of the village, 78% stated to belong to the middle third, while 0.3% (one participant) constitutes the villages' stated richest third. I therefore lack a reliable variable on wealth, in addition to a valid proxy or instrument. Testing for heterogeneity in wealth is accordingly infeasible.

5.3.4 CHARACTERISTICS AND THE FOUR GAMES

The research questions' hypotheses refer to the total participant sample. Still, examining subsamples is interesting to see if the treatments systematically affect participants differently. If individual and group characteristics affect the performance of the treatments, it might have

[26] Heterogeneous groups have less than five participants from the village's largest tribe (*tribe*) and who are born in the village (*native*). Homogenous groups, subsequently, have more than four native or major tribesmen participating.

consequences for the implementation of the forest management regimes simulated in the experiments.

The CFM treatment relies more on the participants' effort than the two other treatments. Therefore, individual and group characteristics should be more correlated with the participants' behaviour in the second part of the CFM game than in the other three games. This is not necessarily the case. In Table 20 the sample is split by the subsamples of the four games, and the games are regressed on the individual and group characteristics.

TABLE 20 INDIVIDUAL AND GROUP CHARACTERISTICS EXPLAINING HARVEST RATES, BY GAME

	Open access	CAC	PES	CFM
Gender (b)	0.176***	0.087***	0.085***	-0.019
	(0.03)	(0.02)	(0.03)	(0.02)
Femgroup	0.098***	0.004	-0.045***	-0.066***
	(0.02)	(0.01)	(0.01)	(0.01)
Age	-0.014**	-0.008**	-0.029***	0.004
	(0.01)	(0.00)	(0.01)	(0.00)
Age^2	0.000***	0.000***	0.000***	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)
Distrust	-0.050***	-0.023*	-0.019	0.026*
	(0.02)	(0.01)	(0.01)	(0.01)
Children	-0.013	0.001	-0.030***	-0.012***
	(0.01)	(0.00)	(0.01)	(0.00)
Tribe	0.001	0.025***	-0.046***	0.026***
	(0.01)	(0.01)	(0.01)	(0.01)
Native	-0.017*	0.028***	0.009	-0.038***
	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.508***	0.185	1.556***	0.635***
	(0.15)	(0.12)	(0.15)	(0.11)
F-test (p-value)	0.000	0.000	0.000	0.000
R^2	0.283	0.09	0.138	0.09
N	540	904	896	920

Notes: Dependent variable: harvest rates in part two of the experiment. Model: OLS. (b) = binary variable. Standard errors in parentheses.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

Table 21 and Table 22 will examine the *gender* and *age* variables more closely. The coefficients on the *distrust* variable of Table 20 are negative in the open access and CAC games, and positive in the CFM treatment. Hence, the finding is consistent with Table 15 for these games, but reversed in the CFM treatment. The latter finding makes intuitively more sense than the former, even though the coefficient is merely significant at the 10% level. That increased trust in the village leadership is correlated with lower harvest rates, could support the argument that local characteristics are more important in the CFM treatment than in the other games. Also, it could indicate that participants who are less afraid of elite capture cooperate better. In row six, the *children* variable does not change sign from Table 15, but becomes insignificant in the open access and CAC games.

The *tribe* and *native* variables are as puzzling here as in the previous sub-subsection. In Table 15 *tribe* is negative; while in Table 20, the same variable is negative in the PES treatment and positive in the CAC and CFM treatments. The coefficient is, however, larger in the PES treatment than the two others. The *native* variable is significantly positive in the CAC treatment, significantly negative in the CFM treatment (and at the 10% level, significantly negative in the open access game). In Table 15, the same variable seems to be positive. The finding supports the claim that the total ethnic group heterogeneity effect on behaviour in the experiment is ambiguous.

In examining the effect of gender on the treatments, Table 21 splits the results of Table 14 by male and female participants. The results indicate that for women the CAC treatment induces the lowest harvest rates, whereas for men, the CFM treatment still induces the lowest harvest rates. However, the CFM treatment influences women to about the same degree as men. Women are, in fact, altogether more influenced by the imposed treatments than men, as is also visible in Figure 13.

TABLE 21 GENDER EXPLAINING THE PERFORMANCE OF THE THREE TREATMENTS RELATIVE TO THE OPEN ACCESS GAME

	Ordinary least squares		Clustered by villages		Village fixed effects	
	Male	Female	Male	Female	Male	Female
CAC	-0.094	-0.243***	-0.094	-0.243***	-0.170***	-0.299***
	(0.06)	(0.06)	(0.09)	(0.06)	(0.06)	(0.06)
PES	-0.052	-0.085	-0.052	-0.085	-0.127*	-0.132*
	(0.06)	(0.06)	(0.07)	(0.09)	(0.06)	(0.07)
CFM	-0.203***	-0.237***	-0.203***	-0.237**	-0.291***	-0.281***
	(0.06)	(0.06)	(0.03)	(0.07)	(0.06)	(0.06)
F-test (p-value)	0.004	0.000	0.000	0.022	0.000	0.000
R^2	0.088	0.15	0.088	0.15	0.143	0.192
N	147	141	147	141	147	141

Notes: Dependent variable: difference in harvest rates between part one and part two. Standard errors in parentheses.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

The finding indicates that gender composition of a village and the gender traditions of the community affect the effectiveness of forest management regimes differently. In a village with more women than men, e.g. due to many men being migrant workers, the CAC regime could produce better results than where the gender composition is more equal. Similarly, where women traditionally are responsible for harvesting forest products, the CAC regime could be more appropriate than the CFM.[27]

Table 22 examines harvest rates by participants below and over 40 years of age. For young participants, the effect of the treatments is in general lower than for old participants. The PES treatment is insignificant and the CAC treatment is only significant with village fixed effects, while the CFM treatment influences behaviour. For old participants, the CAC treatment induces the largest change in behaviour, while the CFM treatment still induces a larger change than in the young participants. Also the PES treatment induces a lower harvest rate than the open access game among old participants.

[27] In my data, the stated absolute forest use among women is higher than among men. Controlling for this does not alter the tendencies.

TABLE 22 AGE EXPLAINING THE PERFORMANCE OF THE THREE TREATMENTS RELATIVE TO THE OPEN ACCESS GAME

	Ordinary least squares		Clustered by villages		Village fixed effects	
	Young	Old	Young	Old	Young	Old
CAC	-0.075	-0.276***	-0.075	-0.276***	-0.127**	-0.317***
	(0.06)	(0.06)	(0.09)	(0.05)	(0.06)	(0.07)
PES	0.012	-0.161**	0.012	-0.161*	-0.021	-0.213***
	(0.06)	(0.06)	(0.07)	(0.08)	(0.06)	(0.07)
CFM	-0.180***	-0.251***	-0.180**	-0.251***	-0.227***	-0.281***
	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)
F-test (p-value)	0.001	0.000	0.062	0.001	0.000	0.000
R^2	0.106	0.175	0.106	0.175	0.139	0.191
N	154	117	154	117	154	117

Notes: Dependent variable: difference in harvest rates between part one and part two. Young=age<40. Old=age>40.[28] Standard errors in parentheses
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

Old participants are consequently, as women, more influenced by the treatments in general, and are most influenced by the CAC treatment. The finding indicates that a planned forest management intervention also should take into account the age composition of the community. A CFM regime might have better effect in a "young community", whereas a CAC regime might have the better effect in an ageing community. The results are, however, not necessarily in support of the argument of Grossmann et al. (2010) that key positions should be held by older individuals. As depicted in Table 20, age is of lesser importance in determining behaviour in the CFM treatment than in the other three games. If the argument of Grossmann et al. (2010) was to hold, the age coefficient should be strongest in this treatment, where the older participants have the tool of communication to influence the other participants. This is not the case.

5.4 SUMMARY AND GENERAL DISCUSSION

Table 23 summarises the results in examining the eight hypotheses. The result column to the right indicates if the results support, oppose, or neither support nor oppose the hypotheses. The column is based on the general findings. I therefore do not consider the findings in 5.3.4 in the table.

[28] The subjects are split at the age of 40 as the mean age of the sample is 40.7 years. Splitting at the age of 30 or 50 produces the same tendency.

TABLE 23 SUMMARY OF THE RESULTS, BY HYPOTHESES

Hypothesis	Described	Result
H11	The change in behaviour from the 1st part to the 2nd part of the experiment is due to the externally imposed treatment	+
H12	The behaviour of the participants in the presented experiment is predictive for their real life behaviour	+
H21	The open access game induces the highest forest use	+
H22	The CAC game induces higher forest use than the PES game	-
H23	The CFM game induces the lowest forest use	+
H31	Younger individuals have higher forest use than older individuals	+
H32	Male individuals have higher forest use than female individuals	-
H33	Heterogeneous groups have higher forest use than homogenous groups	?

+ Results support the hypothesis
- Results oppose the hypothesis
? Ambiguous results

The results oppose the hypotheses H22 and H32, while the results in hypothesis H33 are ambiguous. The remaining hypotheses are supported by the results from the experiments. The following three sub-subsections further elaborate on the findings of Table 23, and discuss the implications in relation to the existing literature.

5.4.1 INSTITUTIONS MATTER!

Two of the three treatments of the experiment have clearly significant effects on behaviour. The CAC and CFM treatments induce significant changes in the behaviour of the participants, while the effect of the PES treatments is not a clear improvement from the open access situation. That the communication treatment performs well is consistent with relevant experimental studies (Cardenas 2000; Ostrom et al. 1994; Ostrom 2006). The finding that the CAC treatment changes behaviour, even though less researched, is also consistent with relevant experimental studies (Rodriguez-Sickert et al. 2008; Velez et al. 2010).

In general, the results make an argument for the continued expansion of CFM in Tanzania, even though the anecdote mentioned in sub-subsection 5.2.4 is a reminder of elite capture as a potential problem. The results also make for a slightly weaker argument for CAC as a forest management regime, in particular in some subgroups; as will be discussed in the next sub-subsection. Lastly, the results make an argument against PES regimes where the private environmental payment is lower than the private payoff for harvesting, possibly due to crowding out of moral.

An important caveat is that generalisation of the institutional findings from the experiment to the real life hinges upon the external validity of the treatments. I am unable to test the correlation between the treatments and the institutions they simulate, and have throughout the thesis consequently been left with arguing for their relevance. In the CAC treatment, the scrutiny is close and the deterrence is strong, making only clear risk seekers interested in violating the stated rule. This is on purpose as institutions aiming to deter individuals from acting anti-socially should be strong enough to deter risk neutrals. Still, a $^1/_3$ chance of being caught is perhaps overly optimistic, making the deterrence in the experiment stronger than in real life Tanzania.

The PES treatment rewards participants individually for not harvesting trees, but the reward for not harvesting is lower than the reward for harvesting. The dominant strategy presented in sub-

subsection 4.4.1 is thus not changed. According to the predictions of subsection 3.2, there should subsequently be no change in behaviour. The treatment merely adds a private reward for not harvesting to the future collective reward provided by the growth rate, as discussed in sub-subsection 5.2.3. That the dominant strategy is unchanged is however true for the third treatment as well, where there is a substantial change in behaviour.

The CFM treatment is essentially communication; as in the experiments of Cardenas (2000) and Ostrom et al. (1994). Allowing the participants to communicate and imposing no other guidelines, could resemble CFM in that the local users themselves are given the responsibility to handle the issue. Thereby, the participants are given a tool and a responsibility, similar to the implentation of CFM. At the same time, communication is a severe simplification of the institution that is CFM, perhaps more so than the simplification in the other two treatments. This excludes many aspects of the institutions that could have an effect on participants' behaviour. Another issue, is the economic incentives for elite capture being weaker in the experiment than in real life. A village's elite can potentially in real life situations benefit greatly from elite capture, but in the experiment the benefit is severly limited. Furthermore, the scrutinty by "outsiders" is most likely closer in the experiment than in real life, further decreasing the incentives for elite capture.

There are consequently arguments against the external validity of the experiment's treatments. These should not be disregarded as they are valid and concern an aspect of the experiment not possible to test. Still, I argue for the treatments external validity based on the treatments likeness to the idea they simulate, as presented in subsections 2.4, 4.4, and 5.2. Also, that most findings are in accordance with the literature indicates the validity of the experiment.

Given the assumption that the games possess enough external validity to predict institutions' effect on real life behaviour, the experiment finds that institutions matter, and that their effects on behaviour vary. In general, CFM induces the greatest change in behaviour, while the PES treatment induces the least change in behaviour.[29] Understanding potential institutions and their effect on behaviour is thus central in deviating from the open access situation.

5.4.2 INDIVIDUAL CHARACTERISTICS MATTER!

In addition to institutions, the experimental findings indicate that individual characteristics affect behaviour. Even though the relationship is quadratic, age is significantly and negatively correlated with harvest rates, implying that younger participants are more selfish in facing the CPR dilemma than older participants. The finding is consistent with the literature presented in sub-subsection 3.4.3 and mentioned in sub-subsection 5.3.1 (Grossmann et al. 2010; Carpenter et al. 2008; Meier & Frey 2004). But, the findings that the oldest participants are the most aggressive harvesters and that age is insignificant in the CFM treatment, refrain me from supporting the argument of Grossmann et al. (2010); that key positions should be held by older individuals.

Also significant in determining participants' behaviour in the experiment is gender. Contradictory to what the literature in sub-subsection 3.4.3 (Shiva 1989; Agarwal 2010;

[29] I have differentiated the regimes by the idea the regimes are founded upon. This does not, however, suggest that the regimes are incompatibles. For instance, involving community management in a PES regime is feasible. As is involving community management in designing command and control measures. An interesting area for further study is therefore the effect that combinations of these regimes have on forest users' behaviour.

Merchant 1996) suggests, female participants are more aggressive harvesters than male participants. More supportive of the literature is, however, the finding that feminine groups harvest less than masculine groups. The contradictory findings imply that including women in a group reduces the group's total harvest, while women at the same time individually harvests more than men.

When examining the gender variables by the subgroups of the four games (Table 20), the CFM treatment stands out. While the *gender* coefficient is significantly positive in the other three games, it is in insignificant in the CFM treatment. Furthermore, the *femgroup* coefficient is strongly and significantly negative in the CFM treatment. As mentioned, the CFM treatment is the treatment most reliant on the participants themselves. That women perform better under this treatment than in the other games therefore might indicate women's positive influence in making a coordinated strategy. Altogether, the results on the gender variables create an argument against the eco-feminists claim that women are more environmentally friendly than men. Importantly, though, the results also indicate that including women in the decision making process provides a more socially optimal outcome.

The finding that ethnic group heterogeneity's effect on the behaviour in the experiment is ambiguous, also somewhat contradicts the literature of sub-subsection 3.4.3 (Cardenas 2003; Kramer & Brewer 1984; Ostrom 2006), including the stylized fact of Ledyard (1995). The finding could have historical reasons as the principal *ujamaa* policies initiated almost 50 years ago affect the Tanzanian society even today (Stöger-Eising 2011). My variables of heterogeneity are birth place and tribe identity. Even though these variables are unclear in explaining behaviour in the experiment, other types of heterogeneity might be important. Testing the effect of a reliable endowment heterogeneity variable would particularly be interesting. The effect of heterogeneity in general is therefore unclear in the population of interest.

Lastly, village specific factors not observed affect my results, as mentioned in sub-subsection 4.7.2. This implies that other local specific characteristics than tested for here are important in determining forest users' behaviour. These factors could be other types of heterogeneity than tested for (which might vary between villages), the size of the forest or size of the village, accessibility to markets buying forest products, and a plethora of other possible factors.

The results of sub-subsection 5.3.4 show that individual characteristics not only matter in directly determining behaviour of the participants, but also matter in the performance of the treatments. Even though the CFM treatment's effect on participants' behaviour is strongest in general, the CAC treatment is stronger for old and female participants. Furthermore, these groups are overall more strongly influenced by the treatments. The individual characteristics' effect on behaviour thus varies with the institution in which they exist. Taking these characteristics into account when implementing and designing forest management regimes, should therefore increase the likelihood of a successful institution.

5.4.3 Experiments matter!

Gender, gender composition, age, and other unobserved village specific factors affect the behaviour of the participants and the performance of the treatments. Ethnic heterogeneity is not significant in determining behaviour, but the finding might be Tanzania specific and other types of heterogeneity might have an effect. These characteristics could vary spatially, and their importance could vary culturally. A cost-effective method to analyse the effect of local specific characteristics on the behaviour of the population of interest, and the effect of institutions in specific communities, is therefore interesting for the design of efficient forest management regimes. This thesis argues that experimental economics is this method.

The surge in published papers utilising experiments seen in the last ten years reflect the acknowledgment of experimental economics as a valuable method for gathering empirical data. Later advancements within the branch furthermore improve the methods; which is visible in for instance the shift towards field experiments. As local characteristics matter, the experiments need to take them into account. Abstract laboratory experimental studies using standard subject pools thus lack the specificity needed for the external validity to hold. In examining CPR dilemmas, this includes specifying the CPR, as there for instance are intrinsic differences between an irrigation system and a forest.

I have argued for, and tested, the external validity of this thesis' experimental design. By incorporating context in good, task and surroundings, and sampling from a relevant population, the experiment takes the effect of the characteristics of forests, the population and the community into account. The results indicate that doing so is necessary for making predictions about real life behaviour.

Undertaking an experimental study on a specific CPR dilemma in a specific area is a cost-effective method to analyse the behaviour of the population of interest in relation to the CPR of interest. The study can be undertaken with modest research inputs, and in low-income countries even relatively low monetary rewards can create strong enough incentives for the experiment to be valid. The experimental study can then create the foundation for piloting and possibly scaling up the measures. The experimental method is thus a tool for *ex-ante* impact assessment, and should consequently matter in implementing measures against deforestation and forest degradation.

6 CONCLUSION

The rate of forest cover loss is dependent on the behaviour of local forest users. To predict the impact of policy reforms through changes in forest management regimes, one needs to understand the factors that influence the behaviour of the forest users. These factors could be inherent in individual and community characteristics, or be imposed as institutions.

Through an experimental field study in seven Tanzanian villages, involving 288 participants, this thesis have examined the effect of relevant institutions and characteristics on the behaviour of Tanzanian forest users. The study utilises the random and controlled attributes of the experimental method, in addition to acknowledging the importance of field context in the experiments and in the sample.

The experiment possesses internal and external validity by design. Furthermore, tests support the validity; as the observed change in behaviour is due to the imposed treatments, and the participants' behaviour in the experiment is correlated with stated real life behaviour. A weak spot is the external validity of the treatments, simulating the institutions of interest. These are not testable and can subsequently merely be argued for.

The results of the experiments indicate that the institutions' impact on the behaviour of the forest users vary by institution, and by group and individual characteristics. The absence of regulatory institutions, i.e. the open access situation, performs the worst in the experiment; albeit the harvest rates are not as high as game theoretical predictions suggest. The command and control (CAC) regime induces a significantly positive change in behaviour towards the social optimal outcome. Even though the change is not clearly significant among men and young individuals, this regime induces the most change among women and older individuals. Partly compensating forest users for not harvesting trees as a payment for environmental systems (PES), decreases harvest rates somewhat among older individuals, but the regime in total does not perform significantly better than the open access situation. The third regime, community forest management (CFM) induces in total the most change in behaviour toward the socially optimal outcome, with the change being strong for both genders and both age groups.

Lastly, in analysing the direct effect of individual and group characteristics on behaviour, the thesis primarily examines the effect of age, gender and group heterogeneity. Younger individuals harvest more than older individuals, but when given the chance, older individuals are not able to influence the group towards a more socially optimal outcome. Women harvests more than men, but when given the chance, they are positively affecting the group; making the social optimal outcome more likely. Ethnic group heterogeneity has an ambiguous effect on behaviour, with other types of heterogeneity being untested.

Even though age and gender are relatively stable characteristics among communities, the composition might for numerous reasons vary, and traditions might make the forest specific tasks vary by the characteristics. The characteristics are thus important for understanding the behaviour of a group of forest users and the effect of potential institutions.

The results make an argument for the continued expansion of CFM in Tanzania. However, the results also indicate that in communities where women are in majority or are in greater extent responsible for harvesting forest products, and in ageing communities, the CAC regime is more effective than the CFM regime. Lastly, the PES regime's lack of effect in changing behaviour could be due to the crowding out of moral. The findings are thus an argument for "pay enough or don't pay at all", or for rewarding through other channels than private monetary payments.

As there are numerous institutional measures to decrease the loss of forest cover, there are aspects the thesis has failed to explore. Variants and combinations of the tested forest management regimes is one. Private property right is another. Thirdly, the cost of implementing the three management regimes has not been considered. Also, the effect of larger deforestation agents, e.g. governments and multinational companies, has not been discussed. These agents' effect on the loss of forest cover, and on the effect of the three regimes, could be substantial. Lastly, the list of local characteristics potentially affecting behaviour in a CPR dilemma is far longer than the ones tested for here. The presented experimental study is thus not meant to be a complete test of the impact of forest management.

Overall, this experimental study of Tanzanian forest users' behaviour shows that forest management regimes have different effects on behaviour. Moreover, these effects vary by the characteristics of the communities. To understand a regime's effectiveness in reducing deforestation and forest degradation in a specific area, the behaviour and local characteristics of the population thus need to be understood. Experimental method is a cost-effective tool to analyse the behaviour of forest users, and to make a preliminary assessment of forest management regimes' impact on behaviour, *ex-ante*. The method should thus become an integral part of the toolkit in any forest management assessment.

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8 Annexes

Annex I: An elaboration on experiment five

Experiment five in my dataset consists of four self-selected/inviter-selected participants and four randomly selected participants. The self-selected participants were exposed before the experiment started, but because of a popular market day in a nearby village we were unable to locate the "proper" participants. The experiment was furthermore the last experiment in the village and we had to be in another village the next day. Therefore I decided to still conduct the experiment and do an *ex-post* evaluation if the experiment should be included in the dataset or not.

The self-selected participants could behave systematically differently than the randomly selected participants. In deciding to include or exclude the experiment I therefore have to decide if experiment five is an outlier in terms of harvest rates. If it is, the experiment could affect my results merely because of the sampling.

Table 24 presents a t-test examining the difference in harvest rates between experiment five and the rest. According to the table, experiment five has a significantly higher mean harvest rate than the mean for the remaining 35 experiments. Table 24 is therefore an argument for excluding the experiment. The table does, however, not control for confounding factors. Experiment five was the last experiment in a specific village, played as an open access game. These factors affect the harvest rates and might be the reason for the observed difference in harvest rates.

TABLE 24 DIFFERENCE IN HARVEST RATES BY EXPERIMENT

Experiment	Mean harvest rate	N
All except five	0.381 (0.01)	3200
Five	0.63 (0.05)	72
Difference	-0.248*** (0.04)	3272

Notes: t-test comparing the total mean harvest rate of experiment five and the remaining experiments. The difference between the means is stated in the 3rd row. Standard errors in parentheses.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

Table 25 controls for confounding factors in estimating the effect of experiment five. The variable of interest is *five*, a binary variable estimating the effect of experiment five.[30] I control for the sequence effect, the effect of the three treatments, and village specific effects.

[30] In Stata terms: five=1 if experiment==5, five=0 if experiment !=5.

TABLE 25 EXPLAINING HARVEST RATES DEPENDENT ON EXPERIMENT FIVE AND CONTROL VARIABLES

Variables	Ordinary least squares	Clustered by villages	Village fixed effects
Five (b)	-0.005	-0.005	-0.005
	(0.05)	(0.04)	(0.05)
Sequence	0.029***	0.029*	0.029***
	(0.00)	(0.01)	(0.00)
CAC	-0.086***	-0.086	-0.086***
	(0.02)	(0.06)	(0.02)
PES	-0.037*	-0.037	-0.037*
	(0.02)	(0.04)	(0.02)
CFM	-0.077***	-0.077*	-0.077***
	(0.02)	(0.04)	(0.02)
Village 2	-0.124***	-0.124***	
	(0.02)	(0.02)	
Village 3	-0.051**	-0.051***	
	(0.03)	(0.01)	
Village 4	-0.153***	-0.153***	
	(0.03)	(0.01)	
Village 5	-0.047*	-0.047***	
	(0.02)	(0.01)	
Village 6	-0.271***	-0.271***	
	(0.02)	(0.01)	
Village 7	-0.279***	-0.279***	
	(0.02)	(0.01)	
Constant	0.485***	0.485***	0.349***
	(0.05)	(0.07)	(0.05)

Notes: Dependent variable: harvest rates in both parts of the experiment. The game and village variables are relative to the open access game and village 1 respectively. N=3272. (b) = binary variable. Standard errors in parentheses.

*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

When controlling for confounding factors, experiment five has a negligible and insignificant impact on the total harvest rates of my data. The impact of one experiment on the total harvest rate of the study should in any case be small, but if the experiment is an outlier due to the sampling of the participants it should have a significant impact. Table 25 indicates that the experiment does not have this impact. I consequently choose to include the experiment in my dataset.

ANNEX II: TESTING THE SAMPLE SIZE

The sample size is important to be able to observe significant effects. I was not able to undertake a *post-hoc* estimation of the required sample size, but the following presents a post-fieldwork test of the sample size required with the observed mean behaviour and standard deviations. The total participant sample is 288, which can be split in four subsamples. The control group, i.e. the open access game, consists of 48 participants and is consequently the smallest subsample. The three treatment games, CAC, PES, and CFM, consist of 80 participants each. The games are explained in subsection 4.4.

The variable of interest when examining the subsamples is the change in mean harvest rate from part one of the experiment to part two, as explained in subsection 4.7. The change is examined by treatment, and relative to the open access control group to control for learning effects. Table 26 summarises the change in mean harvest rates by the games, and the accompanying standard deviations.

TABLE 26 CHANGE IN HARVEST RATES FROM PART 1 TO PART 2 OF EXPERIMENT BY GROUP

Game	Change in mean	Standard deviation	N	Minimum required N
Open access	-.052	.15	48	24/172/13
CAC	-.220	.25	80	24
PES	-.117	.26	80	172
CFM	-.268	.22	80	13

Note: Required N is estimated by the sampsi command in Stata 11. The statistical power of the test is 80%.

With the given means and standard deviations, the required sample sizes with 80% power is given in the far right column.[31] The required subsample sizes are 24, 172, and 13 participants, dependent on the game to be analysed. The change in behaviour by the CAC and the CFM treatments are large enough to be significantly different from the open access game in the study's sample sizes. The subsample size of the PES treatment game is, however, not large enough to observe a significant effect of the treatment. The latter result is in itself a finding within research question two.

[31] Calculated with the sampsi command in STATA 11.

ANNEX III: RELATIVE VS. ABSOLUTE HARVEST

In discussing the dependent variables in sub-subsection 4.7.1, I argued for the use of relative harvest rates, as absolute harvest might be misleading. Because my choice can have consequences for the thesis' results, Table 27 reports the results using the participants' absolute harvest decisions, a number from 0 to 5. The table shows that using absolute harvest slightly changes some results, but do not alter the conclusions.

TABLE 27 THE INDEPENDENT VARIABLES EXPLAINED BY ABSOLUTE HARVEST

Variables	Ordinary least squares	Clustered by villages	Village fixed effects
CAC	-0.216**	-0.216	-0.216**
	(0.09)	(0.22)	(0.10)
PES	-0.017	-0.017	-0.034
	(0.09)	(0.17)	(0.09)
CFM	-0.273***	-0.273	-0.282***
	(0.09)	(0.19)	(0.09)
Sequence	0.117***	0.117**	0.117***
	(0.02)	(0.03)	(0.02)
Gender (b)	0.299***	0.299	0.299***
	(0.06)	(0.19)	(0.06)
Femgroup	-0.004	-0.004	-0.024
	(0.02)	(0.10)	(0.02)
Age	-0.049***	-0.049**	-0.053***
	(0.01)	(0.02)	(0.01)
Age^2	0.001***	0.001***	0.001***
	(0.00)	(0.00)	(0.00)
Distrust	-0.049	-0.049	-0.011
	(0.03)	(0.05)	(0.03)
Children	-0.038***	-0.038**	-0.018
	(0.01)	(0.01)	(0.01)
Tribe	0.004	0.004	-0.014
	(0.01)	(0.04)	(0.03)
Native	-0.008	-0.008	0.015
	(0.02)	(0.04)	(0.02)
Relative forest use	-0.439***	-0.439***	-0.433***
	(0.05)	(0.08)	(0.05)
Absolute forest use	0.079***	0.079**	0.075***
	(0.01)	(0.02)	(0.01)
Commercial forest use (b)	0.629***	0.629**	0.514***
	(0.07)	(0.19)	(0.07)
Constant	3.194***	3.194***	3.256***
	(0.31)	(0.59)	(0.32)

Notes: Dependent variable: absolute harvest in each round. N=3272. (b) = binary variable. Standard errors in parentheses.
*** significant at the 1% level
** significant at the 5% level
* significant at the 10% level

9 APPENDICES

APPENDIX I: INSTRUCTIONS[32]

«Thank you everyone for accepting this invitation. We will spend about three hours explaining the activity, playing and conducting a short survey at the end. Let's start.

The following exercise is a different and entertaining way to actively participate in a project about forests. Besides participating in this exercise and earning money, you will answer a few questions afterwards. The funds to cover the expenses have been donated by a scientific body.

This exercise is intended to recreate a situation in which a group must make decisions about the use of a forest. You have been selected and asked to participate in a random draw from a list of all families in this village. This is done to make sure that all have the same chance of participating.

This exercise is different than experiments in which other persons in this community or others may have played already. Therefore, comments you have heard from other persons do not apply necessarily to this exercise.

You will play several rounds equivalent, for example, to years or wood harvest seasons. Let's pretend this group has an area of forest with initially 80 trees. Each round you have to make a decision about how many trees you want to harvest. You can harvest a maximum of 5 trees and minimum of 0 trees from the forest.

[Visual explanation: we have a number of paper trees here which represent the forest units. The MONITOR shows what happens if a number of units are harvested]

Between the rounds the forest is regrowing. For each ten trees of the existing resource, 2 new trees are added for the next round.

[Visual explanation: the MONITOR shows with the trees that for each row of 10 trees 2 new are added to the forest]. The forest cannot grow to more than 160 trees.

Each participant makes a harvest decision. Each harvested tree is equivalent to 100 shillings, which will be paid to you after the experiment.

When the size of the forest is less than 40 trees, the maximum harvest is less than 5 trees. How much is given by the maximum harvest table. [MONITOR shows a poster of the table]. I will announce the maximum quantity of units you can harvest according to the size of the resource at the beginning of the round and post it here.

In order to make decisions in each round you will one at a time approach the forest and tip down the chosen amount of trees [MONITOR demonstrates]. The decision is done by you and is done anonymously. I will note your choice and put up the trees again so next participant faces the fully available forest for each round.

[32] The instructions were translated to Swahili by the enumerator and monitor, and proof-read by a third-party.

Let us explain this with an example [Use visual explanation]. Suppose the current size of the forest is 68. Each of you decides to harvest 3 trees, and thus a total of 24 trees. The forest reduces to 44 (68-24) and then 8 trees are added, which leads to 56 trees. Thus 24 trees are harvested, and the size of the resource, after regrowth, is reduced with 12 trees. Each participant earned 300 shillings during this round.

So the forest grows. For each 10 trees of the forest 2 trees are added. If there are not 10 trees, we do not increase the forest. This means that if there are less than 10 trees we do not add more. If the resource is less than 8 units, no units can be harvested any more. Now let's continue with the next round. Now the current size of the resource is 44 units. It means that the maximum harvest allowed remains 5 units according to the maximum harvest table.

To start the first round we will all have to leave the game area, and then randomly choose the first participant to decide harvested amount of trees. Also keep in mind that from now on no conversation or statements should be made by you during the game, unless you are allowed to.

We will first have a few rounds of practice that will NOT count for the real earnings; it is just for practicing the game.

[Up to three practice rounds are performed and questions are addressed during the practice].

The initial size of the resource is 80 trees.

[After the practice rounds: announce that the initial size of the resource is again 80 trees and that the decisions are now real and affect the earnings.]

[After six rounds, present one of the following statements determined by researcher:]

[If OA game is played:] First part of this experiment is now finished. Now the forest returns to its original size of 80 trees and we start the next part.

[If OA game is not played:] First part of this experiment is now finished. For the next part a new rule is introduced [announce the rule decided by researcher]

Rule 1: Each of you can harvest legally 0, 1, 2 or 3 units per round. If a participant tips a higher amount than 3 trees, he or she risks being caught and has to pay a penalty. After every decision the researcher throws a die. When the die shows a six or a five, the researcher will inspect the choice done. If the participant tipped more than 3 trees, no payment will be given to the person that round and we subtract an extra 10 trees from his/her total. I [the monitor] have not the power to punish you.

Rule 2: In this rule, you will receive payment for the trees you decide not to harvest as well. For each tree you decide not to harvest, you receive 80 shillings. So if you for instance decide to harvest two trees you will receive 200 shillings for those, and another 240 shillings for the ones you did not harvest

Rule 3: Before your harvest decision in each round you are allowed to talk to each other for 3 minutes about how many trees to take out of the forest. You are only allowed to talk when all participants are present. Your decision is still anonymous and given in the same way, and the total harvested amount will be announced as before».

APPENDIX II: QUESTIONNAIRE[33]

Participant no	
Participant name	
Village name	
Age	
Gender (0=male, 1=female)	

1	Are you born in this village? 1=yes, 0=no	
2	How important is the forest to you? 1=essential, 2=important, 3=not important	
3	How much forest products do you use compared to other families in the village? 1=more, 2=about the same, 3=less	
4	How many times per week do you go to the forest to collect forest products?	
5	Have you sold any forest products during the last month? 1=yes, 0=no	
6	Hunting is common. How many times do you hunt in a month?	
7	Claim: The village council is working for the best of the village. 1=strongly agree, 2=agree, 3=neither agree or disagree, 4=disagree, 5=strongly disagree	
8	Which group in the village do you belong to? 1=the richest ⅓, 2=the middle ⅓, 3=the poorest ⅓	
9	How many hectares of land do your family own?	
10	Has anyone in your family had paid work outside the farm during the last month?	
11	How many minutes does it take to walk from your home to the village centre?	
12	How many minutes does it take to walk from your home to the forest frontier?	
13	Do you belong to the largest tribe in the village? 1=yes, 0=no	
14	Do you know of any forest conservation projects in the area? If yes, please name the project.	
15	How many adults (over 18 years old) are there in your household, including yourself?	
16	How many children (under 18 years old) are there in your household?	

[33] The questionnaire was translated to Swahili by the enumerator and monitor, and proof-read by a third-party.

APPENDIX III: THE CONSENT FORM[34]

Participant No.: ____________

Place and Date: ____________________________ Time of experiment: ____:____

You have been invited to participate in an experiment that is part of a research about management of natural resources. Your participation is very important for this research. The experiment and the following interview will give important information for all of us.

This research does not imply experiments with human beings, animals or vegetable material. For that reason your participation will not have any risk for your health.

At the end of the experiment, you will receive an amount of cash depending on your earnings during the exercise. After the experiment is over, you need to answer some questions about you and your experience as a user of natural resources. What you earned in the exercise and your answers in the survey will be confidential. **This information will be used for academic purposes only.**

Your participation in the experiment is completely voluntary. You may leave the experiment at any time. However, if you decide to leave before the experiment is over you will not receive what you earned. The amount of money that you earn during the experiment will be given to you, after you finish answering the questions of the survey.

If you want a copy of this consent form, please ask us for it.

Agreement

I, ____ state that I understand the information given above and my rights and commitments during the experiment. I also understand that I can leave the experiment at any time declining to receive the money earned in the exercise.

Signature: ____ date:_______________

I, Øyvind Nystad Handberg, student at the Norwegian University of Life Sciences, certify that this information will be used in a confidential manner and only for academic purposes. I also certify that I will pay to each participant the amount of money earned during the experiment.

Signature: ____ date:_______________

[34] The consent form was translated to Swahili by the enumerator and monitor, and proof-read by a third-party.

APPENDIX IV: THE MAXIMUM HARVEST TABLE[35]

Resource level	Max. harvest level per participant
40-160	5
32-39	4
24-31	3
16-23	2
8-15	1
0-7	0

[35] The table was translated to Swahili by the enumerator and monitor, and proof-read by a third-party.

APPENDIX V: ILLUSTRATIVE PHOTOS FROM THE EXPERIMENTS

FIGURE 14 THE SET UP OF THE EXPERIMENT, BUSONGO

FIGURE 15 PARTICIPANTS RECEIVE INSTRUCTIONS IN MUGHUNGA

FIGURE 16 A WOMAN ENTERS THE GAME AREA AS ANOTHER LEAVES. THE ENUMERATOR TO THE RIGHT ENSURES NO COMMUNICATION, DODOMA-ISANGA

FIGURE 17 ILLUSTRATION OF A HARVEST. IN THIS CASE THREE TREES ARE HARVESTED

FIGURE 18 RANDOMLY SELECTING PARTICIPANTS FROM VILLAGE FILES AND INSTRUCTING INVITERS, ZOMBO

FIGURE 19 THE SIMULTANEOUS IDENTIFICATION AND ANONYMISATION OF PARTICIPANTS