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A Framed Field Experiment on Collective Enforcement Mechanisms with Ethiopian Farmers

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A Framed Field Experiment on Collective Enforcement Mechanisms with Ethiopian Farmers

by Anke Reichhuber, Eva Camacho and Till Requate

Christian-Albrechts-Universität Kiel

Department of Economics

Economics Working Paper

No 2008-11

A Framed Field Experiment on Collective Enforcement Mechanisms with Ethiopian Farmers

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Abstract

We present the results of a framed field experiment with Ethiopian farmers that use the mountain rain forest as a common pool resource. Harvesting honey causes damage to the forest, and open access leads to overharvesting. We test different mechanisms for mitigating excessive harvesting: a collective tax with low and high tax rates, and a tax/subsidy system. We find that the high-tax scheme works best in inducing the desired level of harvesting while the tax-subsidy scheme may trigger tacit collusion. Via a panel data analysis we further investigate which variables influence the subjects' decisions during the treatments.

Keywords: common pool resources, collective tax, framed field experiment.

1 Introduction

In this paper we present results from a framed field experiment with Ethiopian farmers who experience the exploitation of common pool resources in their daily life. In Ethiopia, a long period of institutional change ranging from feudalism and centralized socialism to democracy and decentralization was combined with large-scale resettlement and incoherent environmental policies to create many quasi-open-access areas. The formerly abundant mountain rain forests in South-West Ethiopia represent one such area. The absence of an efficient system for regulating access and use has resulted in high rates of deforestation, overharvesting of non-timber products, and biodiversity loss (FAO, 2003).

To regulate this kind of overuse is difficult, largely because the individual behavior of the subjects involved can only be monitored at a prohibitively high cost. By contrast, the total result of forest use is readily observable. This state of affairs is reminiscent of a non-point source pollution problem where only the ambient pollution level can be identified, but not the individual emissions.

The particular aim of our study is to apply mechanisms known from the theory of non-point source pollution[1] to the problem of harvesting non-timber products in the rain forests. The idea behind these mechanisms is that observation of a deviation from the total harvesting levels considered to be socially optimal will trigger some punishment or reward. Segerson (1988) has proposed a mechanism where a fixed penalty and a collective tax (subsidy) proportional to the difference between the actual and the targeted ambient pollution level is charged (paid) when the actual pollution lies above (below) the targeted level. Transferred to our problem, the tax (subsidy) would be charged (paid) if the aggregate harvest level of forest products

[1] A non-point source pollution problem is characterized by the feature that only the ambient pollution level rather than the polluters' individual emissions can be observed.

exceeds (falls short of) the aggregate standard set by the regulator.

We then want to test whether a collective tax mechanism with a high (theoretically incentive compatible) tax rate, a mechanism with a low (more symbolic) tax rate, and finally a mechanism combining a tax with a subsidy can solve the problem of overexploiting forest resources. The mechanisms are designed in such a way that the social optimum is the outcome of a symmetric Nash equilibrium for both the *high* collective tax and the tax-subsidy mechanism while with the *low* collective tax the socially optimal outcome does not constitute a Nash equilibrium of the one-shot game.[2]

In addition to the aggregate efficiency rates we include also an analysis of compliance at the individual level. In order to do so we use the framework proposed by Cárdenas and Ostrom (2004) regarding the different layers of information subjects use when deciding about cooperation within a group context: subjects' individual characteristics, material incentives, and group behavior. Since we conduct a field experiment, subjects are already familiar with the task and social context in which the decision is made. Therefore the social environment may influence the subjects' behavior under certain control mechanisms. This effect can be even more important in a setting like ours where subjects interact during several periods being able to learn about the group behavior under the different mechanisms.

We find that the subjects' decision in the first period determines to a great deal the behavior in the following periods. Moreover, subjects who initially harvest more than the desired amount tend to do so even after punishment and reward, while those subjects initially complying tend to continue doing so after reactions by the others and reactions through the mechanisms. Thus there appear to be some features in

[2]If individuals collude, also the low tax rate can trigger compliance. Indeed in some groups collusive outcomes could be observed.

the subjects' individual characteristics that drive their decisions independently of which mechanism is applied.

Economists have studied the effectiveness of this and similar mechanisms[3] in a variety of economic laboratory experiments.[4] In all these studies the socially optimal outcome has been implemented by Nash equilibrium. A number of studies have established that a collective tax as suggested by Segerson (1988) is effective for small groups of homogeneous agents. The relative efficiency of the mechanisms studied varies for the different experiments. In this context, the existence of multiple equilibria has been a common concern since the effectiveness of mechanisms is reduced if subjects coordinate on suboptimal equilibria.[5]

Following the taxonomy proposed by Harrison and List (2004) our experiment can be considered as a *framed field experiment* as our subject pool consists of Ethiopian farmers and we drew upon the field context when explaining our experiments to the subjects. There are very few similar studies up to this point. Ostrom (1990) provides a body of empirical data to explore conditions under which common pool resource problems have been solved in a satisfactory or in an unsatisfactory way.

Vélez et al. (2005) conduct a framed-field experiment in three regions of Colombia to test several models of pure Nash strategies when subjects are motivated by a mix of self-interest and altruism, reciprocity, inequity aversion, or conformism.

[3]Xepapadeas (1991) suggests a mechanism where a collective or random lump sum punishment combined with a subsidy is triggered, when the actual pollution level exceeds the desired pollution level.

[4]See Camacho and Requate (2004), Spraggon (2002), Cochard et al. (2005a), and Vossler et al. (2006).

[5]Closely related to this is another problem. Even if compliance is achieved on the aggregate level, individual compliance is not guaranteed. Players may coordinate on asymmetric equilibria in which some subjects face high penalties, although they comply, while others free-ride on excess compliance of others. See similar findings by Spraggon (2002) and Cochard et al. (2005a).

Alpízar et al. (2004) compare the collective and the random fining mechanism using a non-budget-balancing version of the mechanisms proposed by Xepapadeas (1991). Running the experiments with two different subject pools, Costa Rican students and CEO's of Costa Rican coffee mills, they find that both fining schemes perform relatively well in achieving the optimal pollution level when applied to groups of two.

So far, experiments on collective enforcement mechanisms have been framed in terms of the control of non-point pollution from industrial or agricultural sources. The framework of our experiments is biodiversity conservation and the harvesting of honey from the forest. Apiculture is an important livelihood factor in the forest areas of south-western Ethiopia. Households usually own between 10 and 100 traditional beehives. In the areas studied here lianas are the major raw material for making traditional beehives. As farmers have a preference for certain species of liana, over-exploitation of these species has resulted in a severe reduction of liana diversity (Senbeta et al., 2005).

The imposition of a collective punishment mechanism in such an environment is not as far-fetched as it might seem. For example, several forests areas in developing countries are currently undergoing a process of certification for the production and sale of biodiversity-friendly forest products, such as honey. Prices for certified products are higher than those paid for conventional commodities. These certificates contain provisions on maximum harvesting. In the case of over-harvesting, the certificate can be withdrawn, which immediately reduces returns for all farmers, irrespective of their individual harvests. This withdrawal option can be regarded as a collective enforcement mechanism.

Another real world example for a group reward or group punishment mechanism is a region in Costa Rica where farmers participating in the *Payments for Environ-*

mental Services program receive cash from the state if they agree to protect the forests in their vicinity. Originally, farmers were punished collectively if deforestation was observed in an area covered by such a contract. However, this mechanism was perceived as unfair by farmers penalized on the basis of these contracts. As a consequence collective punishment was abolished in 2003.[6]

Both examples indicate that collective punishment may be difficult to implement, especially in developing countries where farmers are more vulnerable to financial risks. Accordingly, we further tested whether the desired outcome can also be induced by a tax that is lower than theoretically necessary to implement the social optimum by Nash equilibrium. As mentioned this has not been the case. By contrast, the effectiveness of the theoretically incentive compatible enforcement mechanisms was remarkable. We find that with a relative efficiency of around 99% the high collective tax and the tax-subsidy mechanism perform surprisingly well. The low collective tax performs much worse. But as 40 - 50% of the decisions comply with the socially desired outcome the low tax is still remarkably effective although its efficiency further decreases with repetition. The results show that the Segerson mechanism, although often criticized for being impracticable but also unfair, may be reconsidered as a viable regulatory alternative if observation of individual behavior is difficult. We also believe that the field character of our study enriches experimental research on collective enforcement mechanisms and also provides a new perspective on the applicability of such mechanisms.

The remainder of the paper is organized as follows: In the next section we describe the environment of the experiment. In section 3 we present the theoretical predictions we use as benchmark. In section 4 we describe the subject pool and

[6]See Pagiola (2002) for a description of the Payments for Environmental Services Program in Costa Rica, and Ortiz et al. (2003) for a description of the group contracts.

experimental design, before reporting on our main findings in section 5. Finally, in section 6 we draw some conclusions.

2 The Environment of the Experiment

Our experimental sessions were conducted in the villages of Kayakela, Yehebito, and Ermo, located near Bonga, the capital of the region of *Kaffa* in the south-west of Ethiopia. In these villages the main source of income for the inhabitants is subsistence farming. On average, the farmers own 1 hectare of land on which they produce staple crops like maize and ensete.[7] One important feature is that all three villages have access to the forest. The forest is used by the communities for harvesting forest-coffee and other non-timber forest products, such as honey, but also for apiculture and as a source of fire-wood. As no property rights have been defined for the forest resources, the forest can be considered an open-access system.[8]

The four main pillars of the social infrastructure in the villages are the family, an informal organization called *idir*, ethnicity, and religion. One village comprises between 10 and 20 idirs, and each idir is made up of some 30 households that are usually close neighbors and often related to one another. These organizations are governed by the elderly, and they play important social and economic roles in the villages.[9]

As mentioned above, one of the resources taken from the forest is honey. Farmers

[7] In Ethiopia 1 ha is the average-size plot of land per household, as it is regarded as the absolute minimum required to provide sufficient food for one household (Berhanu et al., 2002, p.58).

[8] For a detailed description of the institutional background see Stellmacher (2005).

[9] The social role consists in preventing and/or resolving conflicts in regions where no formal institutions exist for that purpose, as is the case in our study areas. Compliance with certain rules is achieved via social sanctions, the most serious of which are malediction and dismissal. See Stellmacher (2005) for an analysis of idirs in Yehebito.

put up their beehives twice a year and harvest the honey about 6 months later. This harvesting activity, however, causes non-negligible damage to the forest, and as the farmers earn part of their living from the forest, it also causes a negative social externality. For instance, lianas are cut down to obtain raw material for the beehives (bark and stems). Moreover, farmers light fires next to the beehives in order to drive the bees out of them when it is time to harvest the honey. As a result, scientists have observed a serious reduction of liana diversity.[10]

A difficult problem in controlling and reducing harvesting activity consists in the fact that it is extremely costly to identify the owner of a particular beehive found in the forest. By contrast, it is relatively easy to count the total number of beehives put up in the forest by all the members of the villages nearest to the forest. As a consequence, controlling total harvesting activity displays typical features of a non-point pollution problem (see Segerson, 1988).

3 Theoretical Benchmark

In this section we describe the theoretical basis for the experiments we have conducted. The predictions from the theoretical model serve as a benchmark to evaluate the farmers' behavior observed in the experimental treatments.

Consider a group of n identical farmers who put up $b_i \in [0, \bar{b}]$ beehives, where $\bar{b}$ denotes the number of beehives owned by each farmer. The honey harvested is proportional (for simplicity identical) to the number of beehives put up in the forest. The farmers sell the honey at the local market earning a market price of p per unit. For simplicity we assume that harvesting incurs no cost to the farmers. In the absence of regulation, a typical farmer's profit is then equal to the revenue obtained by selling the honey, and he wants to operate at full capacity, i.e. he chooses $b_i = \bar{b}$.

[10] According to Senbeta et al. (2005), lianas support the stability of the forest ecosystem.

The negative externality caused by the harvesting activity is denoted by $D(B)$, where $B = \sum_{i=1}^{n} b_i$ is the total number of beehives harvested by the whole community. We assume that $D(B)$ is increasing and convex, i.e. $D'(B) > 0$, and $D''(B) > 0$.

In this partial model the social planner's objective is to maximize social welfare (SW) defined as the sum of the farmers' profits minus the social damage resulting from the degradation of the rain forest due to harvesting activity: $SW = p \cdot B - D(B)$.

The socially optimal allocation is then simply characterized by the first-order condition $p = D'(B)$, where we use B^* to denote the socially optimal level. If the conservation effort is distributed equally among the farmers, the optimal number of beehives to be put up and harvested by each farmer is $b^* = B^*/n$.

Further, we assume that monitoring the harvesting activities of each individual farmer is prohibitively costly, whereas the total number of harvested beehives, B, can easily be observed.

To induce farmers to comply with the socially optimal aggregate standard, a regulator can employ different economic incentive schemes. In the following we describe the control instruments that are the subject of our experimental investigation: (a) a collective tax and (b) a tax-subsidy scheme.

Under the *collective tax mechanism* the farmers will be charged a tax $t > 0$ if the aggregate number of beehives harvested exceeds the number considered to be socially optimal. For each farmer the tax bill is proportional to the total harvest exceeding the socially optimal level. Under the *tax-subsidy mechanism* suggested by Segerson (1988) farmers additionally receive a subsidy $s > 0$ compensating them for additional conservation effort whenever $B < B^*$ holds. Thus for both mechanisms a typical farmer's profit can be written as

$$\Pi_i(b_i, \mathbf{b}_{-i}) = \begin{cases} pb_i - t\left[B - B^*\right] & \text{if} \quad B > B^* \\ pb_i & \text{if} \quad B = B^* \\ pb_i + s\left[B^* - B\right] & \text{if} \quad B < B^* \end{cases} \tag{1}$$

where $\mathbf{b}_{-i}$ denotes the vector of decisions of the other farmers in the community except farmer i. Note that the total tax bill is the same for each farmer. In case of the pure tax mechanisms, we have $s = 0$.

For the pure tax mechanisms, it is easy to see that if $t \geq p$, then the symmetric efficient outcome $\mathbf{b}^* = (b^*, ..., b^*)$ is a Nash equilibrium. In addition to this symmetric equilibrium, any strategy profile $\mathbf{b} = (b_1, ..., b_n)$ satisfying $\sum_{i=1}^{n} b_i = B^*$ is also an (possibly asymmetric) equilibrium. If $t < p$, then the only equilibrium of the game is $\mathbf{b} = (\bar{b}, ..., \bar{b})$.

Under the tax-subsidy mechanism the symmetric efficient outcome $\mathbf{b}^* = (b^*, ..., b^*)$ is a Nash equilibrium if

$$s \leq p \leq t \tag{2}$$

holds, i.e. the tax rate must be no smaller than the marginal profit the farmers obtain per beehive harvested, p, and, at the same time, the subsidy must be no greater than p. Under this mechanism too, any strategy profile $\mathbf{b} = (b_1, ..., b_n)$ satisfying $\sum_{i=1}^{n} b_i = B^*$ is an efficient equilibrium.

Note that, depending on the values of the parameters, the farmers can improve upon the one-shot Nash equilibrium under both mechanisms by engaging in explicit or tacit *collusion*. Under the collective tax mechanism, $b_i = b^*$ is a collusive outcome, i.e. it maximizes the joint profits of the n farmers if $pb^* \geq pb - t[nb - nb^*]$ holds for any $b > b^*$.[11] The last inequality, however, is equivalent to $nt \geq p$. Thus even if $t < p$, for the whole group the only equilibrium $\mathbf{b} = (\bar{b}, ..., \bar{b})$ is Pareto-dominated

[11] It does clearly not pay for the group to harvest less than b^*.

by the efficient symmetric strategy profile $\mathbf{b}^* = (b^*, ..., b^*)$ if $nt > p$.

The tax-subsidy scheme is not collusion-proof if $p < ns$. If this condition holds, the whole group is better off by reducing the harvest level below b^* since $pb^* < pb + s[nb^* - nb]$, which is equivalent to $p < ns$. In this case, joint profits are maximal for $\mathbf{b} = (0, ..., 0)$. Thus for the *tax-subsidy mechanism* to be collusion-proof we require

$$s \leq \frac{p}{n} \leq t \tag{3}$$

to hold.[12]

4 The Experiment

4.1 Frame and subject pool

Because our experiments were conducted in the field and the subjects participating were largely illiterate, we tested and adjusted our experimental design and procedure in the course of four weeks of pilot experiments conducted in the area.[13] To enhance external validity and to facilitate comprehension among the subjects, we tried to keep the experiment as close as possible to the Ethiopian farmers' daily life. In

[12] As we will see below, our treatments are finitely repeated games. As is well known, the one shot Nash equilibrium outcome is also the outcome of the subgame perfect equilibrium of the finitely repeated game. Cochard et al. (2005b) consider theoretically the regulation of repeated non-point emissions and conclude (based on Neymann, 1999) that collusion in an ambient-based instrument can be sustained under finitely repeated interactions when the number of repetitions is not commonly known. This is not the case in our experiment. However, the paper by Kreps and Wilson (1982) can be applied to justify the collusive outcome as equilibrium outcome in a finitely repeated game.

[13] To ensure that the experimental sessions were manageable for the subject pool, we run some pilot sessions and then modified several design features such as the payoffs' functional forms, simplicity of presentation, and length of session.

this way the farmers were familiar with the context used and the tasks demanded. We framed our experiment as the harvesting of beehives for several reasons. First, the harvesting of honey constitutes an integral part of the farmers' subsistence in the forest areas. Second, as described above, harvesting honey satisfies two crucial conditions of our theoretical model: (a) it causes an externality, and (b) only total harvesting can be observed, not individual harvesting levels.

In recruiting our subjects we took account of the organization of the farmers into idirs. In each village we contacted the head of the community and asked him to select, for each session, groups of farmers (or "heads of households") living in the same idir.[14]

4.2 Design

In our specification of the theoretical model described above we fixed the parameters as $n = 5$, $\bar{b} = 10$, $b^* = 5$, and $p = 30$ cents. Moreover, we specify the damage function as $D(B) = (B - 24.85)^2$, where units are measured in €.[15] Then we chose two different versions of the tax mechanism, one with a low tax rate of $t = 10$ cents, and a second with a high tax rate of $t = 40$ cents. We refer to the corresponding treatments as *Tax10* and *Tax40*, respectively. For the tax-subsidy mechanism we chose $t = 40$ cents and $s = 20$ cents. We refer to the corresponding treatment as

[14] As a consequence, our subject pool is made up of male subjects only. Although there are some households headed by females, those are in the minority and are subject to discrimination by male-headed households. We used also the authority of the heads of the villages to organize the farmers and ensure that they arrive on time for the sessions, since some farmers live an hour's walk away from the place where the experimental sessions were conducted. In some cases, relatives were grouped together for one session, but only if they lived in separate households.

[15] The first-order condition for the social optimum, $D'(B) = p$, is then equivalent to $2 * (5b - 24.85) = 0.3$, yielding $b = 5$. The damage function will be used to compare efficiency of the different mechanisms in section 5.

Tax-Subsidy.

For *Tax10* we have $t < p$, therefore the unique one-shot Nash equilibrium is $b_i^N = \bar{b} = 10$, yielding a payoff of 50 cents per subject.[16] However, since $nt > p$, subjects could increase their payoffs by colluding and coordinating on the social optimum $b^* = 5$, yielding a payoff of 150 cents per subject.

For *Tax40* and *Tax-Subsidy*, the symmetric one-shot Nash equilibrium is $b_i^N = b^* = 5$, yielding a payoff of 150 cents per subject. Since for *Tax40* the right-hand inequality of (3) is satisfied, subjects cannot improve upon the Nash payoff by colluding.

Under the *Tax-Subsidy*, by contrast, the left-hand inequality of (3) is violated. Therefore the group could increase their payoff to 500 cents per subject by colluding and coordinating on $b_i^{C,Tax-Sub} = 0$.[17]

A total of 23 experimental sessions, lasting about 2 hours each, were conducted in local schools during the summer holidays.[18]

For each session we recruited 5 farmers participating in two of the three treatments. Table 1 indicates both the different types of sessions and the order of the treatments conducted in each type of session. Each type of session was played with at least two different groups.

When the farmers arrived at the place where the experiment was being conducted, they were each assigned a desk. The instructions were explained orally and translated into the local language by an assistant.[19]

[16] This is calculated according to equation (1): $\Pi_i(10, ..., 10) = 30 \cdot 10 - 10 \cdot [50 - 25] = 50$.

[17] In line with equation (1) we obtain $\Pi_i(0, ..., 0) = 20 \cdot (25 - 0) = 500$.

[18] Since news of the experiment spread quite fast, we switched to a different area as soon as we noticed that the participants we were recruiting had already been informed about the experiment. As a consequence, we stayed in each area for a maximum of 4 days.

[19] A transcription of the instructions can be found in Appendix A. We avoided any mention of an external principal or agency as the farmers associate the words "agency" or "principal" with the

Type of session	Treatment 1	Treatment 2
1	Tax-Subsidy	Tax 40
2	Tax-Subsidy	Tax 10
3	Tax 10	Tax-Subsidy
4	Tax 40	Tax-Subsidy
5	Tax 40	Tax 10
6	Tax 10	Tax 40

Table 1: Sequence of the treatments conducted in each type of session.

Table 2 summarizes the parameters used for each treatment, the resulting symmetric equilibrium play, and the symmetric collusive strategy.

Treatment	Instrument	Tax	Subsidy	Strategy	
				Symm. equil.	Collusive
Tax 10	collective tax	10	0	10	5
Tax 40	collective tax	40	0	5	5
Tax-Subsidy	collective tax or subsidy	40	20	5	0

Table 2: Experimental design and symmetric equilibria

At the beginning of the session, each farmer randomly received an envelope containing several decision cards. Each card displayed 10 empty boxes representing the 10 beehives owned. Since most of the farmers were illiterate, the decision cards also displayed symbols (e.g. flower, tree, sun, etc...) indicating the identity of the farmer in the session. To preserve anonymity the farmers were not allowed to show their cards to the others.

For every period representing a harvesting season, each subject was asked to decide on the number of beehives he wanted to put up for later harvesting. To indicate their decisions, subjects were told to cross the number of boxes corresponding to

government and this association might have reminded them of problems connected with delayed payments and hidden agendas and thus have biased their behavior.

their decision on one of their decision cards. Then the participants put the cards into a box. Once all the cards had been collected, the individual number of harvested beehives was displayed on the board next to the corresponding symbol. Although the participants could observe individual decisions, anonymity was guaranteed as they could not match the decisions to the individuals. The revelation of individual decisions made clear to the farmers what impact their actions had on the other group members' payoffs. It also facilitated coordination of actions. Furthermore this design feature reflects the subjects' real situation, as in the forest the farmers themselves are often able to observe individual behavior of the members of their community.[20]

Finally, the total number of beehives harvested was announced, and the rules governing the corresponding control mechanism were applied. Then the payoffs were calculated and put into (or in case of a negative payoff were removed from) the plates displaying the different symbols used to identify the subjects (i.e. flower, tree, sun, etc.).[21]

The transparent procedure used here enabled us to gain the trust of the farmers and helped them not to forget the rules during the game itself. Moreover, the distribution of the money at the end of each period fascinated them and incited them to continue for a further round.

[20]Within a threshold public good game, Croson and Marks (1998) observe that providing identifiable information on individual contributions is not significantly different from providing only group information. Following these results, the efficiency of our instruments is not enhanced by the availability of the information on individual harvesting.

[21]The participants were informed that if payoffs were negative at the end of the session, they would not have to pay any money to the conductor of the experiment, i.e. aggregate payoffs would be set equal to zero. We did so because the opportunity cost of participation was quite high, and we were afraid that subjects would not participate if they ran the risk of coming home with less money than they had before participating.

After explaining the procedure to the participants we conducted 3 trial periods using the board. The first treatment was then repeated for 12 periods. Then the second treatment was explained, following the same procedure, and it was also repeated for 12 periods. The farmers were not allowed to communicate.

At the end of the session, the subjects received the money accumulated in the respective plates.[22]

As the farmers were reluctant to reveal the truth when asked directly about the motivation for their decisions, we refrained from systematic interviews after the sessions.

5 Results

Our data set covers decisons of 115 subjects in the 23 sessions conducted. A Kruskal-Wallis test indicates differences among villages for all treatments but *Tax40*. These differences, however, do not reveal any distinctive pattern depending on the village as can be seen from the Figure 1. Instead, they may be the result of specific conditions prevailing during the experiments.[23] We therefore abstain from separate presentation of the three villages in the following. Each subject only participated in one session.[24] In describing our results we distinguish between inexperienced and

[22]The average payoff was 40 Ethiopian birr (equivalent to 4€). To assess the saliency of the reward, note that the daily wage for an unskilled rural worker ranges between 10 and 20 birr.

[23]We can see that there is not a specific pattern depending on the village. The only two exceptions are in Yehebito. One is the coordination in reducing harvesting in Yehebito in the tax-subsidy treatment. But this only occurred in one group. The second one is a *Tax10* treatment with experienced subjects. Here the subjects coordinated in 5 during the 5 periods. See table **??** in appendix B

[24]We eliminated the data of 4 sessions (20 subjects) from the analysis since those subjects obviously had been informed by neighbors and relatives about the decisions they thought to be "optimal actions" in earlier sessions. Since the treatments were different, those informed subjects

experienced subjects. Inexperienced subjects are those who participated in "treatment 1" of each session, that is, those who were taking part in a particular treatment for the first time. Experienced subjects are those who had already participated in a different treatment during one session, i.e. those participating in "treatment 2" of a given session.

5.1 Aggregate behavior

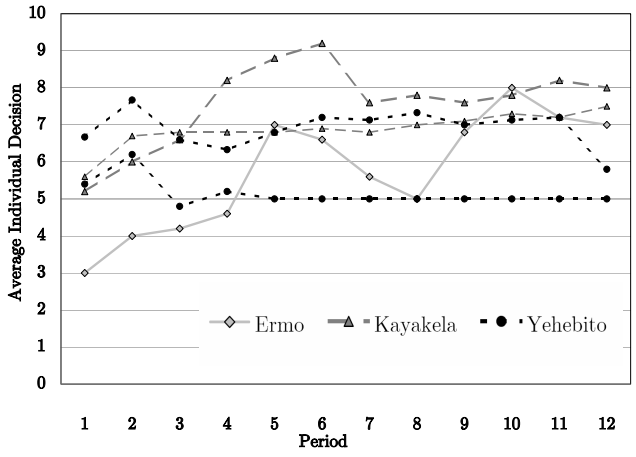
Figure 2 gives an initial impression of the performance of all three mechanisms.

Taking the distribution of decisions for *Tax10* displayed in Figure 2(a), we observe a bimodal distribution in the harvesting decisions. Fewer than one third of the inexperienced and about one third of the experienced participants play the one-shot Nash equilibrium, whereas almost half of the inexperienced and almost 40% of the experienced participants comply with the desired harvesting level of 5 units. There are two possible explanations. Either a significant number of the subjects had realized that compliance through collusion would yield a higher payoff than non-cooperative payoff maximization, or some of the subjects harvesting only 5 units believed that compliance was socially desirable. Since all subjects within one group belong to the same idir, we believe that the group context had a strong influence on their behavior and that it is mainly responsible for the deviation from the Nash equilibrium.[25]

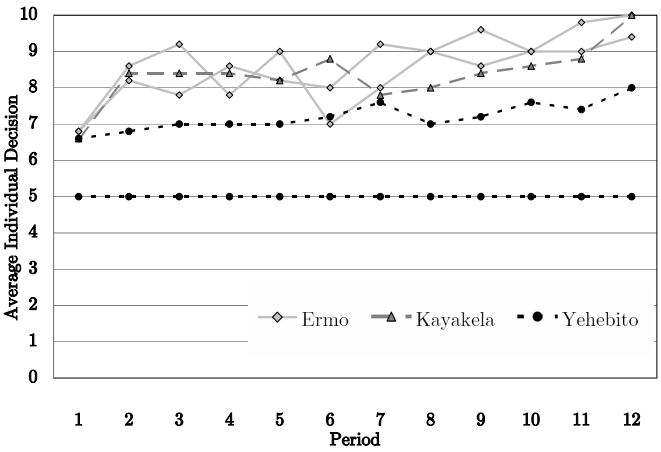
If we look at the evolution of the aggregate harvest over time, displayed in Fig-

chose actions which have been only optimal for the earlier treatments but not with respect to the treatments they were participating in themselves.

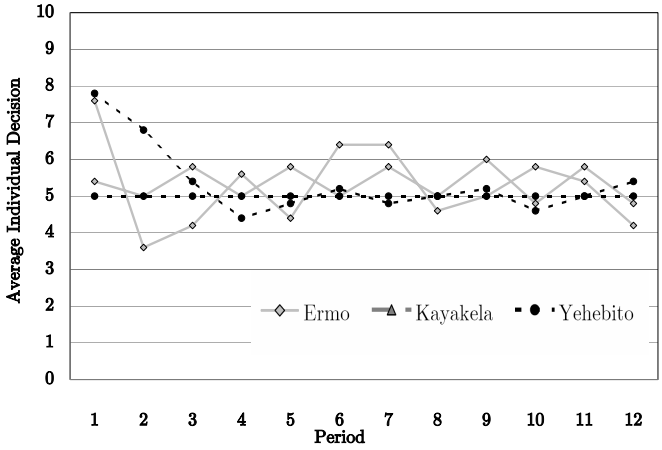
[25]The results are similar to those observed in standard public good experiments: contributions are close to the collusive outcome in the first periods and then decrease towards the Nash prediction in later periods. However, in real world situations collusion may be higher because farmers might be able to communicate and end-game effects disappear.



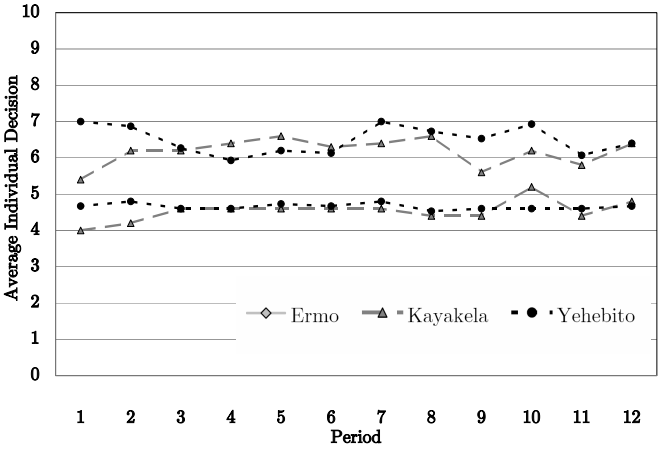
(a) Tax 10 Inexperienced



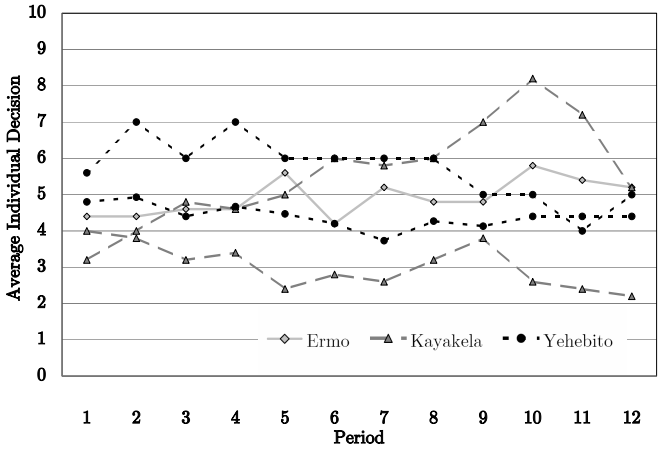
(b) Tax 10 Experienced



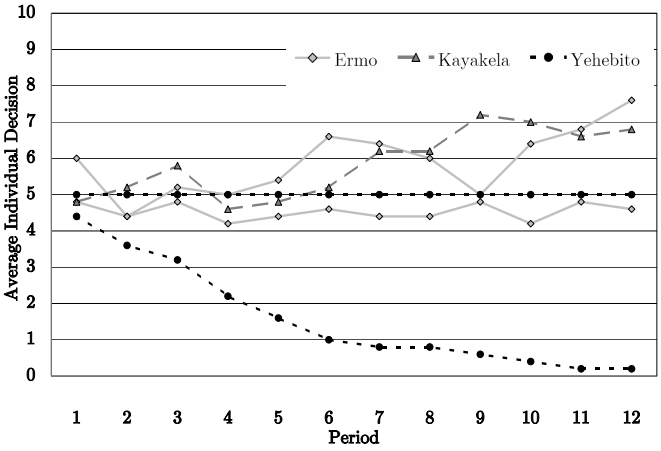
(c) Tax 40 Inexperienced



(d) Tax 40 Experienced

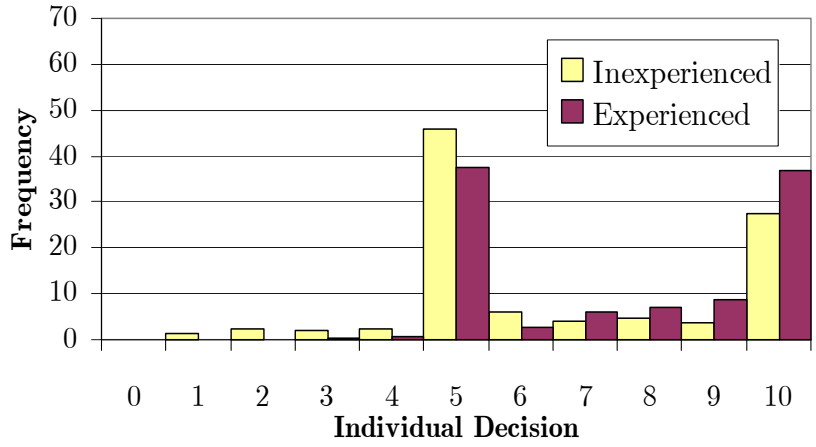


(e) Tax-Subsidy Inexperienced

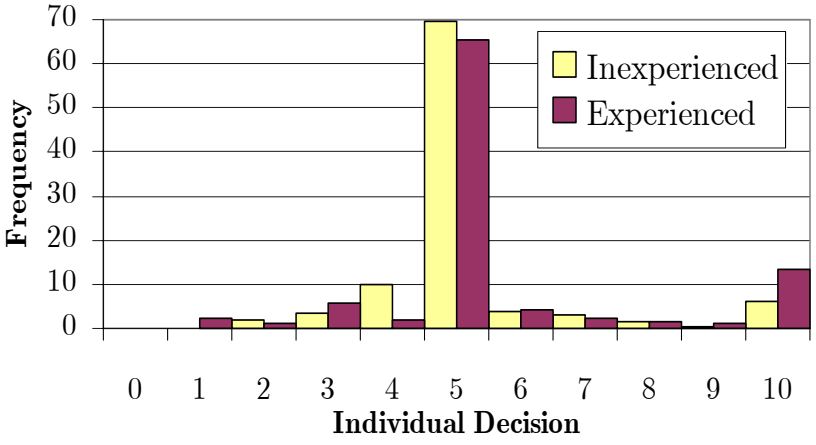


(f) Tax-Subsidy Experienced

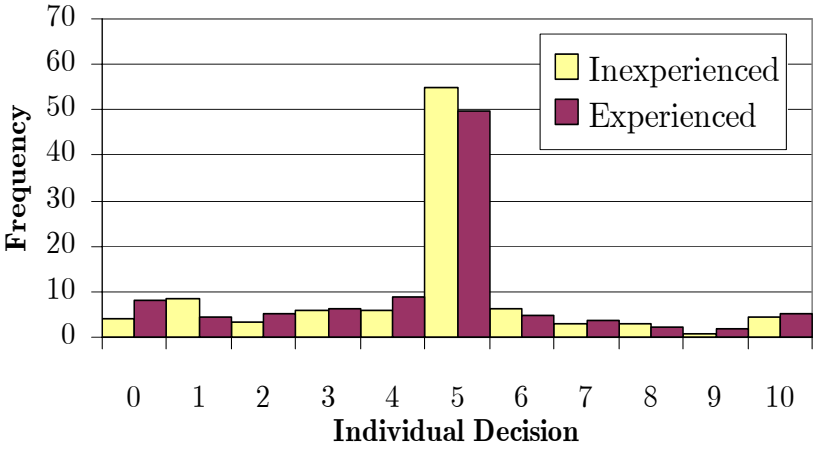
Figure 1: Dynamics of the mean individual decision per village



(a) Collective Tax 10



(b) Collective Tax 40



(c) Tax-Subsidy

Figure 2: Frequency distribution of individual decisions

ure 3, we observe that it increases slightly over time and that the small number of inexperienced subjects deciding to harvest fewer than 5 beehives no longer pursue this course after having gathered experience, a typical behavior also been observed in many other experiments. Using the terminology of Cárdenas et al. (2000), cooperative behavior is crowded out by short-term individual payoff maximization. Nevertheless, Table 3 confirms that in *Tax10*, on aggregate, both inexperienced and experienced farmers harvest fewer beehives than predicted by applying the concept of a one-shot Nash equilibrium.[26]

Treatment	Aggregate		Inexperienced		Experienced	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Tax 10	6.97	2.42	6.62	2.45	7.54	2.26
Tax 40	5.49	1.92	5.25	1.52	5.60	2.07
Tax-Subsidy	4.62	2.20	4.64	2.12	4.58	2.31

Table 3: Summary statistics per treatment and experience condition

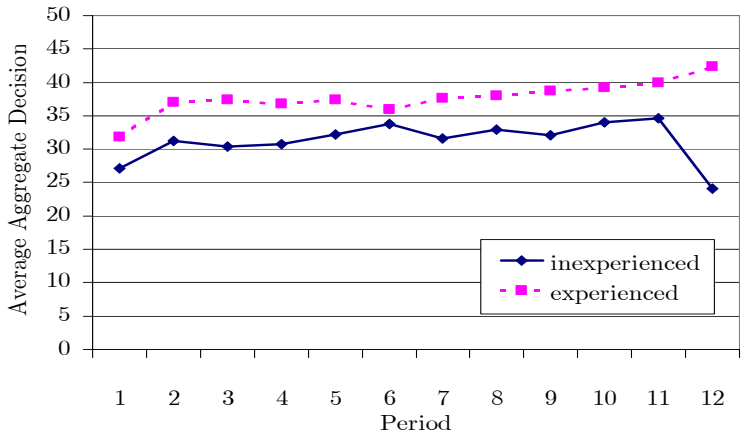
When we apply the high-tax mechanism *Tax40* (Figure 2(b)), the frequency of compliance increases significantly, reaching 70% and 65% for inexperienced and experienced subjects, respectively.

However, looking at Table 3, we observe a surprisingly high share of participants still harvesting more than 5 units.[27] Note that, from a rational viewpoint, harvesting more than 5 units is only profitable if one expects the other participants to harvest fewer than 5 units.

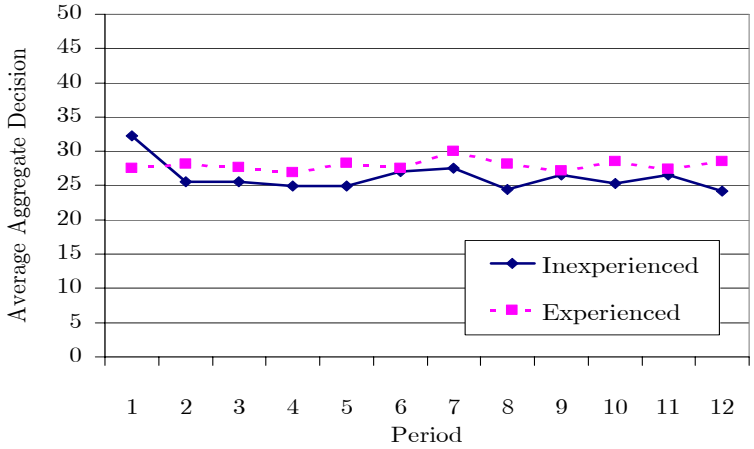
Indeed, for inexperienced subjects overharvesting and underharvesting can be

[26] A Wilcoxon test shows that the number of beehives harvested per farmer is significantly lower than 10 and higher than 5 for both inexperienced and experienced farmers.

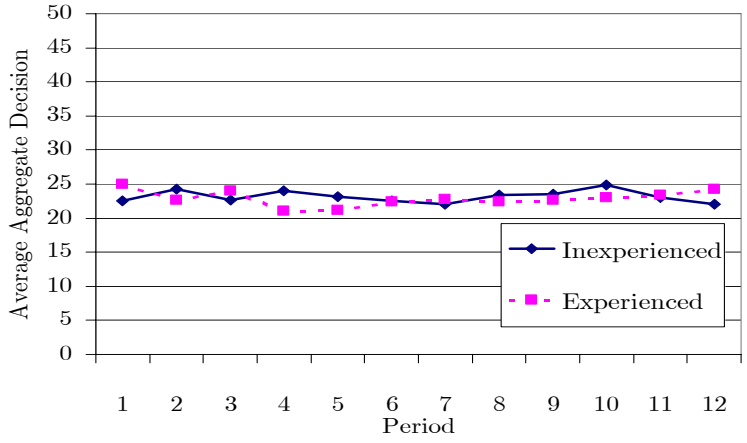
[27] A Wilcoxon test confirms that the average number of collected beehives is indeed significantly higher than 5 for both inexperienced and experienced subjects.



(a) Collective Tax 10



(b) Collective Tax 40



(c) Tax-Subsidy

Figure 3: Dynamics of the average aggregate harvesting.

observed with approximately equal frequency. For experienced subjects, however, we observe overharvesting more often. Since harvesting 5 units is the only equilibrium, and the equilibrium is Pareto-efficient for the players, there seems to be no rational explanation for this at first glance. However, considering of the relative position of payoffs in the case of excess harvesting offers an explanation. Look at the following example:

Subject	A	B	C	D	E
Harvest	7	8	7	8	5
Payoff	-190	-160	-190	-160	-250

In total, the whole group harvests 35 units, i.e. 10 more than "required by the regulator." Player E, the only one employing a compliance strategy, gets the lowest payoff. If he increases his harvest to, say, 8 units, the outcome changes as follows:

Subject	A	B	C	D	E
Harvest	7	8	7	8	8
Payoff	-310	-280	-310	-280	-280

Since -280 is less than -250, it does not seem to be rational to increase harvesting from 5 to 8. However, player E clearly improves his relative position compared the other players while worsening the payoffs for the others at the same time. This behavior might be explained by inequality aversion, but also by a certain degree of reciprocity through retaliation.

Finally, under *Tax-Subsidy*, we observe a uni-modal distribution of the decisions with a peek at the Nash equilibrium (see Figure 2(c)). However, a considerable number of the subjects harvest fewer than 5 units. This behavior may be induced by tacit collusion since the joint profit maximum would be achieved if each subject harvested nothing.[28]

[28] A Wilcoxon test shows that the mean is significantly lower than 5. However, a disaggregate

Looking at the dynamics of the harvest over time (see Figure 1), there is no obvious time trend. Moreover we observe that for *Tax-Subsidy* experience has no impact.[29]

Let us now turn to the crucial question of which mechanism performs best. If we look at the frequencies of socially optimal outcomes, i.e. harvesting 25 beehives altogether, we find that under *Tax40* about 30% of all outcomes lead to 25 beehives, while under the other two mechanisms around 19% of the outcomes are socially optimal. However, to evaluate overall efficiency we also have to study the overall outcome. For this purpose we apply Spraggon's (2002) efficiency measure, defined as the ratio of the difference between actual welfare and the welfare in the *status quo* to the difference between welfare in the socially optimal state and the *status quo*:

$$E = \frac{SW\,actual - SW\,status\ quo}{SW\,optimal - SW\,status\ quo} \times 100$$

Table 4 illustrates the efficiency of each mechanism in terms of the experience of the subjects. We see that, although *Tax40* induces more outcomes that are socially optimal, *Tax-Subsidy* performs equally well and even slightly better for experienced subjects whereas *Tax10* performs considerably worse.

Table 4 also includes the efficiency results obtained in similar experimental studies conducted in the laboratory with students as subject pool. Note that in *Tax 10* the gains from harvesting ($p = 30$) exceeds the fine ($t = 10$), i.e., the fine is 1/3 of the harvesting gains. Therefore, we can compare this setting to the *status-quo* treatment in Cochard et al. (2005a) and the *no-instrument* treatment in Vossler et al.

view shows that the participants only took advantage of the opportunity for tacit collusion in 3 of the 10 groups and thus reduced the aggregate harvest to below 25.

[29] A M-W test shows that the difference between inexperienced and experienced farmers is statistically significant only for *Tax10* and *Tax40*.

(2006). From Table 4 we observe that the efficiency under *Tax 10* is significantly higher than that of the *laissez-faire* situation implemented in the other studies, even though the implemented tax is not incentive compatible. Regarding the efficiency under *Tax 40*, our results regarding efficiency are in line with the other papers. Note that the tax rate applied is 1.33 times the minimum tax necessary to induce compliance, i.e., $t = p = 30$. Finally, the efficiency results obtained under *Tax-Subsidy* are in line with Spraggon (2002) although we do not observe that experience reduces efficiency.

Instrument	Our results		Spraggon (2002)		Vossler	Cochard
	Inexp.	Exp.	Inexp.	Exp.	*et al.* (2004)*	*et al.* (2005)
Tax10	89.5	74.2			17.3	-1.01
Tax40	99.7	98.6	86.7	92.51	94.3	80.1
Tax-Subsidy	99.5	99.3	98.1	91.3	-222.4	-41.4

Table 4: Efficiency comparison (in %). [* Average rounds 21-30 with cheap talk.]

We summarize our observations on aggregate behavior as follows:

Result 1: i) *Tax40* induces the highest frequency of socially optimal outcomes among the three mechanisms tested.

ii) With respect to efficiency, *Tax40* and *Tax-Subsidy* perform equally well. *Tax10* performs considerably worse.

iii) Under *Tax10* a relatively high number of subjects comply with the optimal outcome in the first rounds. Compliance breaks down as subjects gather experience.

5.2 Individual behavior

Due to the panel structure of our data set (with observations on each individual over time) we can use panel data analysis to account for the heterogeneity across individuals, in particular the dynamics of individual behavior over time.[30]

As mentioned above, Cárdenas and Ostrom (2004) propose that subjects may use different levels of information when deciding about cooperation within a group: the dynamics of the game, material incentives, and the composition of the group. Moreover they emphasize the importance of the subjects' individual characteristics. In the following analysis we control for these aspects.

Table 5 shows how the subjects react to both the behavior of the group in the past and to the material incentives provided by each mechanism. The dependent variable is the individual decision per period. The independent variables are:

- Harvest in $t = 1$: harvesting decision in the first period used as a proxy to measure the subjects' individual initial willingness to comply with the regulation independently of the group dynamics. We therefore include this variable only in the case of inexperienced subjects.

- Period: period number accounting for the time trend in the decisions normally observed in public goods experiments.

The second group of explanatory variables identifies the effect of the material incentives (tax or subsidy) resulting from the mechanisms in the case of over- or under-harvesting, respectively.

- Tax charged in $t - 1$: dummy variable taking the value of 1 if the group was charged a tax in the previous period, and 0 otherwise. Additionally,

[30] Since the individual observations are not independent within each group, we use a *group fixed effects* panel regression to deal with differences across groups.

we multiply this variable by a dummy variable (characterizing the individual harvesting decision in $t-1$), taking the value 1 if the subject's past decision was equal to (lower than, more than) 5, and 0 otherwise.

- Subsidy paid in $t-1$: dummy equal to 1 if a subsidy was paid to the group in the previous period, and 0 otherwise. Following the same procedure as above, we then use dummies to account for the subjects' reaction to the monetary incentives, depending on their decisions in the past period.

As the farmers could observe the individual decisions of the other group members, we also include a group of independent variables accounting for the subjects' reaction to the decisions of the other group members in the previous period:

- decisions over (equal to) 5: number of group members whose harvesting decision in the previous period was higher than (equal to) 5. This variable accounts for the subjects' reaction to free-riding. With the variable "equal to 5" we capture the farmers' response in the presence of group members who are compliant.

Regarding the time trend in *Tax10*, we observe a positive and significant coefficient for the "period" variable with experienced farmers. This result is consistent with observations made in other public goods experiments where contributions tend to decrease over time. However, when subjects are inexperienced, no significant decreasing trend is observed.[31]

[31] Cárdenas (2005) obtained similar results in common pool resource experiments with Colombian villagers. In fact, in the absence of regulation those villagers exhibited a more cooperative behavior as compared to students. One explanation provided by the author is that, contrasting from students, the villagers' experience with the real situation refrains them from overusing the common resources.

Variable	Tax 10				Tax 40				Tax-Subsidy			
	Inexperienced		Experienced		Inexperienced		Experienced		Inexperienced		Experienced	
	Coef.	$p > \|z\|$	Coef.	$p > \|z\|$	Coef.	$p > \|z\|$	Coef.	$p > \|z\|$	Coef.	$p > \|z\|$	Coef.	$p > \|z\|$
Constant	4.521	0.000	7.200	0.000	3.555	0.000	8.092	0.000	5.191	0.000	5.268	0.000
Harvest in $t = 1$	0.130	0.005	-		0.243	0.000	-		0.162	0.012	-	
Period	0.017	0.497	0.067	0.011	0.000	0.992	-0.018	0.527	-0.006	0.801	0.016	0.540
Information on the group's decisions in $t-1$												
Decisions over 5	0.005	0.972	-0.550	0.028	0.262	0.123	-1.032	0.000	-0.024	0.877	-0.222	0.242
Decisions equal to 5	0.049	0.736	0.158	0.548	0.033	0.815	-0.783	0.000	-0.162	0.232	-0.254	0.046
Material Incentives												
Tax charged and the harvest decision in $t-1$:												
More than 5	0.673	0.000	0.477	0.000	-0.142	0.080	0.701	0.000	0.324	0.001	0.825	0.000
Equal to 5	0.236	0.376	-0.729	0.048	-0.685	0.027	0.208	0.365	-0.774	0.041	0.254	0.459
Lower than 5	0.295	0.204	-0.026	0.908	-0.678	0.012	-0.629	0.004	-0.761	0.283	0.107	0.605
Subsidy paid and the harvest decision in $t-1$:												
More than 5	-	-	-		-		-		0.273	0.175	0.875	0.000
Equal to 5	-	-	-		-		-		-0.523	0.116	0.122	0.698
Lower than 5	-	-	-		-		-		-0.842	0.000	-0.784	0.000
N. obs.	430		275		220		494		385		275	
N. groups	8		5		4		9		7		5	
R^2	0.3864		0.5589		0.1482		0.4603		0.4416		0.5172	
F-test $(p - value)$	5.62 (0.000)		12.95 (0.000)		4.47(0.004)		11.01(0.000)		3.95(0.008)		4.57(0.001)	

Table 5: Group Fixed Effects Regression to explain the dynamics of the decisions in terms of the group's behavior and material incentives. F-test for the H_0: Group Fixed Effects=0.

As to the effect of material incentives in the panel data analysis, we observe similar patterns for all the mechanisms applied. When a tax has been charged in the previous period, participants who over-harvested in that period tend to increase their harvest in the following period, while those who harvested five units or less tend to reduce their harvest in the next period. The same pattern can be observed with the tax-subsidy mechanism.

We see from Table 5 that the harvesting decision in the first period significantly determines the harvesting strategies in the following periods (see line 2: variable "Harvest in $t = 1$").[32] Moreover, those who harvested fewer than five units in the last period reduce their harvest level further if a sanction or reward has been applied by the regulator in the last period (see coefficient of variable "Lower than 5"). By contrast, those who harvested more than five units in the last period tend to increase their harvesting level (see coefficient of variable "More than 5").

Figure 4 the distribution of the harvesting decisions in period 1 for each of the applied treatments. We can observe that in the first period of all three treatments between 50% and 60% of the subjects' decisions correspond to the social optimum, that is, harvesting exactly 5 beehives. Regarding those subjects that deviate from the social optimum in the first period, we find differences between the Tax and Tax-Subsidy treatments. Whereas in both tax treatments (*Tax10* and *Tax40*) over-harvesting corresponds to approximately 25% of subjects' decisions and under-harvesting represents only around 12%, the opposite is observed under *Tax-Subsidy*. We summarize these findings as follows:

Result 2: The subjects' individual characteristics, represented by the decision

[32]Frey and Meier (2004) use a field experiment framed on charitable giving to test conditional cooperation. Their results support the theory of conditional cooperation. However, the effect varies. Those who never contribute, do not change their behavior, while those who are indifferent about contributions, react more sensitively to information about others' behavior.

in the first period, significantly determine their behavior under the different control mechanisms.

These findings give rise to define *initial compliers* and *initial non-compliers* as subjects that harvest five units or less, or more than five units, respectively, in the first period. Table 6 shows the proportion of *initial compliers* (i.e. $b_{t=1} \leq 5$) and *initial non-compliers*(i.e. $b_{t=1} > 5$) observed in each treatment. We observe that in most cases subjects' decisions are not affected by the tax (subsidy) being charged (paid) in the previous period (see row $\Delta b_t = 0$ in table 6).

Focusing now on the behavior of *initial compliers* we find that, in most cases, their behavior is not affected by the fine and/or subsidy. This is in particular true for those subjects choosing exactly $b_{t=1} = 5$. However, those subjects that start underharvesting ($b_{t=1} < 5$) tend to revise their decision (with decreasing and increasing their harvest equally likely) when a fine (subsidy) was charged (paid) in the previous period.

Regarding the behavior of *initial non-compliers*, we observe that a high tax rate (as in *Tax40* and *Tax-Subsidy*) is effective to reduce harvesting in the next period ($\Delta b_t < 0$) as compared to the low tax rate and the subsidy. The fact that *initial non-compliers* tend to be more sensitive to the punishment for over-harvesting (tax) than to the reward for under-harvesting (subsidy) might explain why no significant collusion is observed in the *Tax-Subsidy*. We summarize these results as follows:

Result 3: Based on their reaction to monetary incentives, different strategy patterns can be observed if any sanction or reward has been executed in the last period:

i) The *initial compliers* tend to further reduce their level of harvesting.

ii) The *initial non-compliers* tend to further increase their level of harvesting.

We now turn to the subjects' feedback on the individual decisions of the other

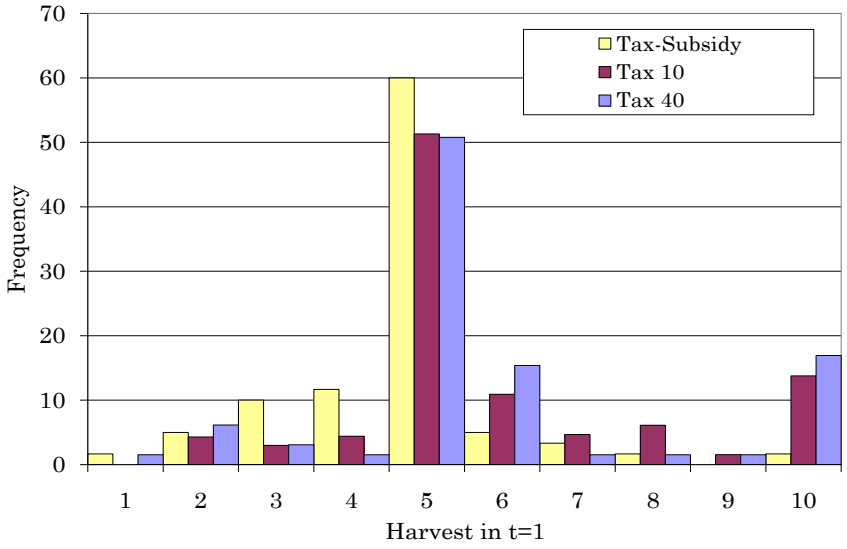


Figure 4: Distribution of harvesting decisions in t=1.

(a) Tax-Subsidy

$\Delta b_t = b_t - b_{t-1}$	Fine charged in $t-1$			Subsidy paid in $t-1$		
	$b_{t=1} > 5$	$b_{t=1} = 5$	$b_{t=1} < 5$	$b_{t=1} > 5$	$b_{t=1} = 5$	$b_{t=1} < 5$
$\Delta b_t < 0$	35.0	30.1	40.4	17.2	19.7	27.3
$\Delta b_t = 0$	42.5	58.3	21.2	62.1	60.7	46.9
$\Delta b_t > 0$	20.0	11.7	38.5	20.7	19.7	25.8

(b) Tax 10 and Tax 40

$\Delta b_t = b_t - b_{t-1}$	Fine charged in $t-1$					
	Tax 10			Tax 40		
	$b_{t=1} > 5$	$b_{t=1} = 5$	$b_{t=1} < 5$	$b_{t=1} > 5$	$b_{t=1} = 5$	$b_{t=1} < 5$
$\Delta b_t < 0$	20.8	14.9	26.3	33.2	6.5	20.6
$\Delta b_t = 0$	54.7	62.5	35.1	48.4	88.8	52.9
$\Delta b_t > 0$	24.5	22.6	38.6	18.5	4.7	26.5

Table 6: Distribution of the change in the subjects' decisions when a fine (subsidy) was charged (paid) the previous period.

group members. Generally speaking, we find a significant effect only among experienced participants. The coefficients for both variables "decisions over 5" and "decisions equal to 5" are negative. This indicates that the higher the number of subjects harvesting five units or more in the previous period, the more likely it is for an experienced individual to decrease his or her own harvest in the next period. The first effect is only observed if a collective tax is charged. The second effect is only significant for *Tax40* and *Tax-Subsidy* where a high fine is charged in case of over-harvesting.

In fact, the subjects' response to an increase in the number of non-compliances in the group is to reduce their own harvest in the collective tax treatments. Conversely, when the number of compliers in a certain period increases, the subjects on average respond by reducing their harvest. In case of punishment (taxation), the reason might be reciprocity while in the *Tax-Subsidy* tacit collusion might be the motive.[33] We summarize our final result as follows:

Result 4: Regarding group composition, i.e. the frequency of complying and non-complying in the group, only experienced subjects react significantly on over- and under-harvesting by the group. In particular

i) Under *Tax10* and *Tax40* the frequencies of non-compliances in one period increases the likelihood of reducing total harvest in the next period.

ii) Under *Tax40* and *Tax-Subsidy* an increase in the frequency of compliances also increases the likelihood of reducing total harvest in the next period.

[33]Note however from Figure 1, only one group out of five succeeded in this tacit collusion in the minimum harvest.

6 Conclusion

In this study we have presented the results of a framed field experiment conducted in south-west Ethiopia where the remaining rain forest can be considered a quasi-open-access common pool resource. Our main objective was to test whether a collective tax with a high, and a low tax rate, respectively, and a collective tax-subsidy mechanism, originally proposed by Segerson (1988), can solve the problem of overexploiting forest resources. The experiment was related to honey production, thus enabling the subjects to identify with the situation easily.

The bottom line of our research is, first, that the Pigouvian tax turns out to be an efficient instrument to regulate problems of overexploiting common pool resources. Secondly, our experimental design allows us to shed some light on the role of experience on both, subjects being are already familiar with a similar control mechanism and having learnt something about the behavior of other group members. Thirdly, we conclude that the low fine could possibly be used as an initial regulation mechanism in order to introduce collective punishment in rural communities in developing countries. Since, however, experience significantly reduces its efficiency, Pigouvian levels have to be implemented in the long run. By contrast, subsidies on the reduction of harvest might result in inefficiently low harvest rates, since people take advantage of the mechanism by colluding. The likelihood of this effect, however, goes down as the number of non-compliers within the group increases, though in real situations collusion might be facilitated through coordination by cheap talk.

It would be an interesting question for future research to investigate how the strategies pursued by the two types of subjects relate to their actual societal position in their respective groups. A further step of research would be to conduct a controlled real field experiment.

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A Instructions

A) Welcome and introduction

Thank you for coming. I am a German PhD student doing research here in Ethiopia. I have been living in Ethiopia for almost one year. I have done some work in Metu and Mizan Teferi, and I am glad that I can now work here in Bonga. I am an economist and I want to play a game with you - not a game for children but games for grown-ups, because we are playing for money. We will play two games, each one has 12 rounds. Please understand that there are no right or wrong answers. And do not worry! We will play three test rounds before each game to make sure that everybody understands how it works.

B) General rules

There are three general rules. The first rule is anonymity. This means that no one knows who is doing what. For this reason we play with symbols. Each one of you will get a symbol. You can find the same symbols here on the board and on these plates. Can everybody see the plates and symbols?

The second rule is that I do not want you to communicate with each other. You can ask me as many questions as you want, but please do not talk to each other.

The third rule is that you can earn money but you cannot lose money. The amount of money you can earn depends on how you play the game. At the end of the games you can take the money with you.

C) Specific rules

Each one of you owns 10 beehives. All the beehives are full. You have an agreement with us that you will only harvest 5 of these ten beehives. If you harvest one beehive you earn 30 cents. After each round we calculate the total number of beehives harvested. If, in total, more than 25 beehives are harvested, you will have to pay a tax of 40 (10) cents for every beehive exceeding that number 25. Every

one of you will have to pay this tax. (For the Tax-Subsidy treatment: If in total, fewer than 25 beehives are harvested, you will receive a subsidy of 20 cents for every beehive below that number. Every one of you will receive this subsidy.) Now you have to decide how many beehives you want to harvest. You have 10 full beehives. You can harvest all of them or none of them or some of them. It's entirely up to you.

The amount of money you earn depends on your decision and the decisions of the other person paying this game. I repeat. Harvesting gives you 30 cents. If the total is more than 25 you have to pay a tax of 40 (10) cents per extra beehive harvested. (For the Tax-Subsidy treatment: If the total is less than 25, you will receive a subsidy of 20.)

D) Procedure

You can now take the envelopes, but do not open them. Inside there are these playing cards, enough for 2 games (Exemplary cards are shown). On each playing card there are ten boxes for ten beehives. For each round you take one playing card and write crosses into the boxes according to the number of beehives you want to harvest. (Translator shows how to make a cross into a box.) Then you fold the playing card and put it into this cardboard box. Then we write the decision of each one next to the corresponding symbol, calculate the total and distribute the money in each of the plates.

B Differences among Villages

As already explained in section 5 a Kruskal-Wallis test shows that there are significant differences among villages for all implemented treatments and experience conditions but Tax 40. In table 7 we present the decisions summary statistics per treatment, experience condition and village. However, we do not observe a special pattern for a given area.

(a) Tax 10

Villages	Inexperienced		Experienced	
	Mean	S.D.	Mean	S.D.
Ermo	5.63	2.04	8.56	1.93
Kayakela	7.11	2.26	8.36	1.96
Yehebito	6.47	2.59	6.10	1.92

(b) Tax 40

Villages	Inexperienced		Experienced	
	Mean	S.D.	Mean	S.D.
Ermo	4.91	1.04	5.21	1.72
Kayakela	4.30	2.46	5.86	2.51
Yehebito	4.72	2.11	3.29	2.10

(c) Tax-Subsidy

Villages	Inexperienced		Experienced	
	Mean	S.D.	Mean	S.D.
Ermo	5.30	1.80	-	-
Kayakela	-	-	5.62	2.18
Yehebito	5.18	1.17	5.58	2.01

Table 7: Summary statistics of the harvesting decisions per treatment and village.