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Ruhr-Universität Bochum (RUB), Department of Economics
Universitätsstr. 150, 44801 Bochum, Germany

Technische Universität Dortmund, Department of Economic and Social Sciences
Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

Bergische Universität Wuppertal, Schumpeter School of Business and Economics
Gaußstraße 20, 42119 Wuppertal, Germany

Series Coordination:
RWI Press Office, presse@rwi-essen.de, phone +49 (0) 201 8149-213
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Introducing and Terminating Monetary Incentives in Non-Regenerating Forests: Insights from a Framed Field Experiment

Nils Christian Hoenow[1,2,*] Gunther Bensch[1] Michael Kirk[2]

Abstract

In regions with low soil fertility, smallholder farmers often clear forest to sustain agricultural yields. This pattern becomes more problematic where forest regrowth is slow, contributing to local forest loss and global environmental challenges such as climate change and biodiversity decline. This paper presents findings from a framed field experiment that examines how different types of monetary incentives affect forest-clearing decisions in northern Namibia, a semi-arid region with negligible forest regrowth. We implemented a common-pool resource game with 518 smallholder farmers across 25 villages, in which a forest stock declines dynamically based on participants' clearing decisions, without immediate regrowth. The game spans three periods: a baseline without incentives, an intervention period with individual or collective rewards or an individual fee, and a post-incentive phase. This setup allows us to assess both the immediate effects of incentives and their persistence after incentive removal. All incentive types reduce clearing compared to the baseline, but not significantly more than in the control condition, where clearing also declined – an unexpected trend likely linked to features of the dynamic game design. Incentive effects largely dissipate after removal, with no strong evidence of lasting motivational crowding-in or crowding-out. Overall, our results suggest that moderate payments may be insufficient to sustain cooperation in persistent resource dilemmas. More broadly, they highlight the importance of multifaceted analysis including control conditions and careful experimental framing in field-laboratory studies, coupled with caution in generalizing findings to other settings or policy applications.

JEL classification: C91, D91, Q15, Q23, Q57

Keywords: common-pool resources, framed field experiment, deforestation, conservation policy, sustainable land use

[1] RWI – Leibniz Institute for Economic Research, Hohenzollernstr. 1-3, 45128 Essen, Germany
[2] School of Business and Economics, University of Marburg, Am Plan 1, 35037 Marburg, Germany
[*] Corresponding author, christian.hoenow@rwi-essen.de
ORCIDS: NCH 0000-0002-7873-0025, GB 0000-0001-7964-7533, MK: n/a

1. Introduction

Smallholder farmers in many parts of the world face the constant challenge of maintaining agricultural yields that sustain their livelihoods. In regions where soil fertility declines rapidly with continued cultivation, farmers often maintain productivity by periodically shifting to fresh, fertile land created by clearing surrounding forests. The conversion of forests to agricultural land is the major driver of deforestation (Hosonuma et al. 2012; Pendrill et al. 2022), with negative implications on local ecosystem services and the global climate (Costanza et al. 1997; Geist and Lambin 2004; Mitchard 2018). Given these negative externalities, policy interventions appear justified in regions where clearing is unsustainable. However, classical regulatory instruments – such as taxation, or penalty payments –are difficult to implement in low-income rural settings, particularly where land rights are unclear and small-scale subsistence livelihoods dominate (Engel 2016). Payments for ecosystem services (PES) offer an alternative policy option that provides positive incentives to land users for environmentally sustainable land use (Ferraro and Kiss 2002; Pattanayak et al. 2010; Wunder et al. 2020).

This paper uses a newly designed framed field experiment to test the effect of different types of monetary incentives and their termination on deforestation decisions by smallholder farmers in areas with limited biomass production. We assess individual and collective PES in rural northern Namibia and compare them with individual negative incentives in the form of fees and with an untreated control condition in a forestry-framed common-pool resource game. The game involves three periods: a baseline period without incentives, a period with the three types of monetary incentives, and a post-incentives period. Each of these periods consists of four consecutive rounds.

We contribute to the behavioural environmental economics literature in two ways, offering insights into ongoing debates on PES design and on the methodology of framed field experiments. First, we provide an integrated comparison of positive and negative, individual and collective monetary incentives within a single experimental framework and examine their persistence after termination. Importantly, our comparative analysis also includes a control condition that never receives any external incentives, enabling us to assess the termination of monetary incentives using a counterfactual Difference-in-Differences design in addition to within-subject comparisons. This dual design enables a more rigorous evaluation of whether extrinsic incentives generate only price effects or also undermine or reinforce intrinsic motivations in the longer term – known as motivational crowding-out and crowding-in (Frey and Jegen 2001; Vollan 2008; Rode et al. 2015). As such payments are unlikely to be provided indefinitely, concerns have been raised about potential negative effects once payments are withdrawn. To date, however, studies in deforestation contexts have not documented lasting motivational crowding-out impacts (Jayachandran et al. 2018; Calle 2020; Etchart et al. 2020).

Second, we develop and apply a dynamic common-pool resource game design. The game is framed to semi-arid forestry contexts characterized by negligible natural forest regrowth. In our design, participants receive a fixed forest stock at the beginning of each period. Within a period, the stock declines over rounds based on participants' clearing decisions and does not regenerate between rounds, reflecting a non-regenerating forest context. While these dynamic features increase realism, the game remains simple: participants only make binary choices between clearing a parcel of forest or not.

We add to the growing experimental literature on the termination of monetary incentives, particularly in PES contexts. Prior framed field experiments on PES termination have predominantly focused on tropical settings with static or fully regenerating forest stocks. Using static common-pool resource game designs in Lao PDR, Salk et al. (2017) and Andersson et al. (2018) found no crowding-out, but some indication of crowding-in effects following the termination of rewards. Similar findings emerged from studies in Colombia by Maca-Millán et al. (2021), Lliso et al. (2022) and Moros et al. (2023). The design by Moros et al. (2023) is conceptually closest to ours, combining land-allocation decisions with a control group and a post-incentive phase, but featuring a replenishing forest and no fee treatment. Only few studies have examined negative (penalty) incentives in conservation contexts. Kaczan et al. (2019) incorporated penalty incentives in a dictator-game experiment in Tanzania and found crowding-in following termination of these incentives. While PES programs have also been evaluated in real-world settings (see Jayachandran et al. 2017; Börner et al. 2020; Blanco et al. 2023; Vorlaufer et al. 2023), economic experiments remain valuable for isolating behavioural mechanisms and identifying how incentive structure shapes responses.

Our results using within-subject comparisons show that clearing rates significantly decrease with the introduction of all three tested incentive types and rise again after their termination, albeit never reaching initial, pre-incentive clearing levels. Critically though, the effect of the incentives as well as any lasting ones after their termination become insignificant when compared against behaviour in the control condition using Difference-in-Differences, as players in the control group also decrease their clearing over time, even without receiving external incentives.

Beyond these empirical findings, our results highlight how central design and analytical choices are to both the observed behavioural responses and their interpretation. In particular, baseline rounds, untreated control conditions, and their combined use in Difference-in-Differences comparisons are relevant for drawing robust inferences about the effects and durability of conservation incentives in framed field experiments.

The remainder of this paper is structured as follows: Section 2 describes the field context of smallholder agriculture in areas with limited biomass production and how we tailored the common-pool resource game and the monetary incentives to this context. Section 3 outlines the estimation strategy and data. Section 4 presents the empirical results, which are discussed in Section 5 in light of the existing literature. Section 6 concludes.

2. Forestry-framed Common-Pool Resource Experiment

We designed a framed field experiment to mimic the dynamics of forests managed as a common-pool resource in an environment with negligible forest regrowth. This section explains how the game reflects the real-world context of forest use in our study area (Section 2.1), then outlines the conceptual foundations of the monetary incentives (2.2) and finally presents the design of the common-pool resource game in Section 2.3.

2.1. Forestry context and game framing: The case of northern Namibia

Our experimental game is structured to reflect key features of real-world forest use under conditions similar to those in our study areas, the Kavango East and West regions in northern Namibia. These are two of Namibia's fourteen first-level administrative regions, characterized by a semi-arid subtropical climate, relatively high seasonal rainfall, and substantial forest cover. This sets them apart from the drier southern parts of the country, while making them representative of many semi-arid areas.

Rural livelihoods in the Kavango regions depend primarily on subsistence agriculture based on annual crops. Given sandy and nutrient-poor soils, farmers often abandon cultivated fields after a few years and create new fields by clearing forest area (Mendelsohn 2009; Brown and Mujetenga 2010). This cyclical clearing pattern is mirrored in our game, where players face repeated decisions to either stay on existing parcels or clear new land. Simultaneously, surrounding forests are vital to these households by providing foods, timber, firewood, and grazing area for their cattle (see, e.g., Angelsen et al. 2014 and Wunder et al. 2014, on the role of forests in rural livelihoods).[1] From an individual farmer's perspective, however, clearing is more profitable than conserving, at least in the short term. Cultivation offers private returns in the form of agricultural yields, whereas conservation benefits are more diffuse. A livelihood analysis for the Kavango region by Pröpper et al. (2015) shows that the direct use value of forest resources to households exceeds the total value of crop harvests, but is lower when compared on a per-hectare basis.

Our game models this social dilemma dynamically: forest stocks carry over across rounds within each period, individual payoffs from clearing exceed those from staying, and the remaining forest at period end generates a group-level payoff. This setup maintains the value of forest conservation until the final round of each period, thereby avoiding unrealistic behaviour often observed in finite-horizon games, where players tend to deplete resources once the game's conclusion nears. It further reflects (discounted) future use values, including both forest benefits and cropping options (Campbell et al. 2002; Barnes et al. 2010).

Local land tenure arrangements further influence land use decisions. Forests are formally communal land under state ownership but traditional authorities, such as village headpersons, exercise partial control over the land, e.g. via local rules. Although farmers are often expected to request permission to clear, such requests are rarely denied, and cleared parcels are typically considered the claimant's property. The absence of formal private land rights precludes the use of land as collateral. Together, these conditions favour short-term conversion over sustainable land use and long-term investment (Mendelsohn 2009; Namibian Ministry of Lands and Resettlements 2015).

The problem of land conversion is further exacerbated by the slow natural regrowth of forests in the region, which may take centuries to restore cleared areas to their previous state

[1] We specifically collected information from our study participants to map how households benefit from forests: 66% use it as grazing area for their cattle, 84% collect firewood, 91% collect timber, 91% collect fruits and other foods, and 41% even sell forest products for income.

(Immanuel 2019). To reflect this ecological reality, the game assumes no forest regrowth across rounds within a period. This design transforms the resource from a repeatedly replenished stock, as commonly conceived in standard common-pool resource games, into a dynamically degrading one in which clearing decisions have persistent consequences for subsequent rounds. By eliminating regrowth, the game more closely mirrors dryland contexts where forest loss is effectively irreversible over farmers' planning horizons. It also increases the salience of intertemporal trade-offs and the cumulative long-term social cost of defection. Qualitative interviews conducted prior to the experiment confirmed that local authorities and villagers perceive current land use as unsustainable, with many expecting a complete disappearance of forests in their area within the coming decade.

While the game reflects central features of forest use in this study setting, several simplifications are necessary to translate these features into a game format. Clearing decisions are modelled as binary choices, without capturing variation in parcel size, and soil fertility is reduced to two discrete states – old and new fields. The game does not account for agricultural inputs such as fertilizer, which are costly but might extend cultivation on existing plots, and forest benefits are monetized even though they are largely non-monetary. Moreover, while the forest stock does not regenerate within periods, it is reset at the beginning of each new period. Participants thus face a dynamically declining forest within each period, preserving the ecological logic of irreversible clearing. The reset serves purely an experimental purpose and does not represent ecological regeneration; without it, depletion from earlier phases would confound the identification of incentive and post-incentive effects.

2.2. Monetary incentives: Conceptual foundations and design rationale

Monetary incentives may affect conservation behaviour not only through direct price effects but also through their impact on intrinsic motivations. A substantial body of psychological and economic research suggests that incentives can crowd out intrinsic motivation when perceived as controlling, yet may crowd in motivation when perceived as supportive (e.g., Frey and Jegen 2001). Classic examples include Titmuss (1970), who documented reduced blood donations when payments are introduced, and Gneezy and Rustichini (2000a), who showed that fines for parents' late pick-ups at day-care centres increased tardiness, as the fine was interpreted as a price that replaced an intrinsic sense of responsibility to be on time (see also Deci 1971; Deci et al. 1999; Bowles 2008). In environmental contexts, similar concerns have been raised: monetizing conservation tasks may weaken pre-existing norms and motivations, particularly once payments are discontinued (Vatn 2010; Vollan 2008; Moros et al. 2019).

Whether such crowding-in or crowding-out effects occur depends not only on the presence of incentives but also on how they are structured and perceived. Two design dimensions are particularly relevant for behavioural responses: whether incentives take the form of rewards or fees, and whether they are provided at the individual or collective level.

Concerning the first dimension, reward-based PES dominate policy debates, while negative incentives such as fees or penalties have received far less attention in framed field experiments on forest use. Existing evidence remains limited and inconclusive. Kaczan et al. (2019), for example, observed that penalty payments can promote conservation better than PES incentives and may even lead to crowding-in if they enforce socially optimal behaviour. Comparative evidence between positive and negative incentives in environmental settings remains scarce.

Regarding the level of implementation, individual reward payments provide clear attribution and direct incentives but may be costly to monitor. Collective schemes are often easier to administer where monitoring is expensive, yet introduce an additional social dilemma, as rewards depend on group performance (Engel 2016; Vollan et al. 2018; Hayes et al. 2019). Laboratory and field experiments comparing individual and collective incentives yielded mixed results (e.g. Travers et al. 2011; Narloch et al. 2012; Gatiso et al. 2018), suggesting that their effectiveness depends on perceptions of responsibility, fairness, and group dynamics. Grillos et al. (2024) further observed that collective payments may foster the emergence of informal institutions.

Taken together, the literature provides no unambiguous prediction. Reward payments may increase cooperation while in place but risk crowding-out once removed if perceived as monetizing conservation. Individual incentives may generate clearer behavioural responses than collective ones, while fees may either strengthen compliance through deterrence or, if perceived as controlling or price-like, undermine intrinsic motivation. These theoretical ambiguities call for direct empirical comparison.

Our experiment therefore tests three types of monetary incentives: individual rewards, collective rewards, and individual fees. Collective fees as penalty payments based on group performance are not included due to ethical and feasibility concerns. All incentives are calibrated to preserve the underlying social dilemma, maintaining a trade-off between individual gains from clearing and collective benefits from forest conservation.

To assess not only immediate behavioural responses to these incentives but also potential motivational crowding effects, the design includes a post-incentive period. During the incentive period, price effects and motivational mechanisms operate simultaneously and cannot be fully disentangled. The post-incentive phase, by contrast, allows us to examine whether behavioural changes persist, fade, or reverse once payments are removed – a policy-relevant feature given that monetary incentives are rarely permanent.

2.3. Design of the common-pool resource game

The specific design of our common-pool resource game draws on earlier dynamic common-pool resource game implementations (e.g. Cardenas et al. 2013; Gatiso et al. 2015) and builds on the contextual features and game framing considerations outlined in Section 2.1 and 2.2. Our design decisions reflect both the ecological conditions of the study region and the behavioural rationale underlying the tested incentive mechanisms. A key innovation of our design is that the forest stock is non-regenerating within a period. Unlike many common-pool resource experiments that replenish the resource each round within a period, the forest stock in our game declines dynamically based on group clearing decisions and carries over from round to round without being replenished. Payoffs depend on the amount of forest remaining at the end of each period rather than on immediate regrowth.

Each game session comprises three periods of four rounds each. Participants are randomly assigned to one of four experimental conditions. Figure 1 illustrates the sequence of periods, rounds and treatments: Period 1 serves as the baseline, in which all participants play the same common-pool resource game without any external incentives. In Period 2, treatment conditions are exposed to one of three incentive types, whereas the control condition continues without incentives. In Period 3, incentives are removed and all participants play the same game as in Period 1. This extends the design introduced by Hoenow and Kirk (2021), allowing us to experimentally test different types of incentives. Together with our control group, this design allows us to analyse both short-run behavioural responses and post-incentive dynamics across different incentive types. It also enables comparison between within-subject changes and Difference-in-Differences estimates based on an untreated counterfactual.

Figure 1: Experimental design across periods and experimental conditions

Period	Period 1 "Baseline"	Period 2 "Incentives"	Period 3 "Post-Incentives"
Round	Round 1 2 3 4	Round 1 2 3 4	Round 1 2 3 4
Treatment 1: Individual Reward	no external incentives	Individual Reward	no external incentives
Treatment 2: Collective Reward	no external incentives	Collective Reward	no external incentives
Treatment 3: Individual Fee	no external incentives	Individual Fee	no external incentives
Control Condition	no external incentives	no external incentives	no external incentives

Due to logistical constraints, we could not apply all experimental conditions in every village but randomized the allocation to three or four conditions per village.[2] We set the group size to seven players to keep the decision process tractable while guaranteeing realism in the sense that the marginal per capita return of cooperation remains reasonably small. Communication between players is prohibited to reflect the decentralized and uncoordinated nature of clearing decisions. Participants receive real payouts in Namibian dollars (N$) and neither the introduction nor the removal of incentives is announced in advance.

Each period of the game begins with one parcel of cultivated land as initial endowment for each player. In each of four rounds, the players decide privately whether to stay on their old field or clear a new one from the communal forest (Figure 2). The forest initially consists of 28 parcels, referred to as hectares towards the participants, and each clearing decreases the size of the forest by one parcel. After each round, an enumerator announces how many parcels were cleared by the group as a whole and updates a poster showing the forest's remaining size (Figure 3). The forest stock is only reset to 28 at the beginning of a new period, as discussed in Section 2.1. Individual decisions are not disclosed.

Payoffs are calculated at the end of each period. Staying yields a private payoff of 1N$ per round and old field, represented by one bag. Clearing implies a higher individual payoff in

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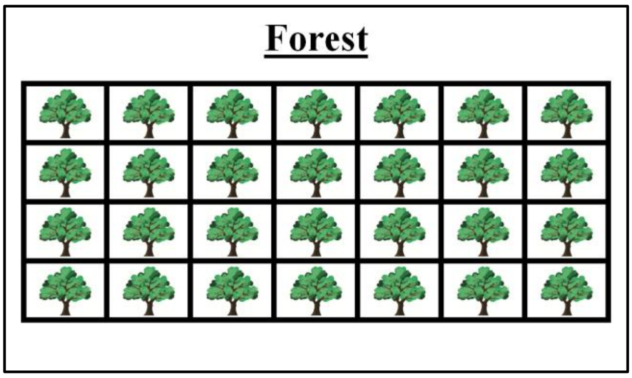
[2] In addition, the experimental design included another treatment on resource scarcity, which is not assessed as part of this paper, but in a companion paper (Hoenow and Kirk 2021).

that round as a fresh field yields three and a half bags, or 3,50N$, after which the field turns into an old field. Additionally, all players receive 1N$ per forest parcel left at the end of the period. This creates a tension between short-term individual gain and long-term group benefit. Clearing is individually optimal (Nash equilibrium), whereas full cooperation – everyone staying on their old field only – is socially optimal.

Figure 2: Main choice set as shown on participant's tablet

Figure 3: Poster with forest parcels used in the game



Note: Figure 2 shows a screenshot from the data collection software "Kobo-Toolbox".

In Period 2, payoffs are adjusted by the three incentive treatments: players in the *Individual Reward* treatment condition receive an additional 1N$ for staying on their old parcel, resulting in a total of 2N$ each round they decide not to clear forest. In the second treatment of *Collective Reward*, all players in the group receive 0.50N$ per round as long as at least half of the forest (14 parcels) is left by the end of the round. This threshold-based group bonus is a common way of designing collective rewards in the experimental literature (e.g., Travers et al. 2011; Narloch et al. 2012). The third treatment of *Individual Fee* payments is symmetrical to the Individual Reward treatment as players have to pay 1N$ for each parcel they clear, reducing their clearing reward from 3.50N$ to 2.50N$. Under all experimental conditions, clearing remains individually optimal while staying remains socially optimal, but with lower advantage: the private gain from clearing relative to staying is reduced to only 0.50N$ per round for Individual Reward and Individual Fee. The game thus preserves the core tension between individual and collective payoffs even under treatment, albeit with adjusted margins.

Payoffs and incentive levels were calibrated through extensive pre-testing to ensure that clearing rates in the baseline were neither close to 100% nor close to zero, and that participants received a reasonable final compensation. A formalized representation of the game and its payoff structures can be found in Appendix A. The game is intentionally kept simple to ensure participant comprehension and maintain experimental control. More complex dynamics – such as gradually declining field yields or non-linear social benefits from forest size – were excluded after pre-tests revealed comprehension difficulties. Furthermore, there is no scarcity-driven competition in resource extraction: the forest is large enough that all players could clear in every round without exhausting the resource before the end of a period. This design ensures that players never need to worry about missing out on clearing opportunities, and that every decision to stay and spare a forest parcel results in a clear social benefit.

3. Estimation Framework and Data Collection

3.1. Estimation framework

We estimate three key treatment effects. The *incentive effect* is defined as the change in clearing decisions between the baseline period (Period 1) and the period in which incentives are introduced (Period 2). The *incentive-removal effect* captures changes between Period 2 and the post-incentive period (Period 3). The *aggregate post-incentive effect* measures the net change from baseline to the post-incentive period (Period 1 vs. Period 3), combining the first two effects. This aggregate measure also serves to identify potential motivational crowding effects: a decline in clearing beyond the baseline level may indicate crowding-in of intrinsic motivation, whereas a return to – or increase beyond – der baseline clearing may suggest crowding-out.

To identify these effects, we apply two complementary estimation approaches: Difference-in-Differences (DiD) and within-subject comparisons. The DiD approach leverages the presence of a control condition and estimates treatment effects through interaction terms between treatment assignment indicators and period indicators. Specifically, the incentive effect is captured by the interaction with the Period 2 indicator, while the aggregate post-incentive effect corresponds to the interaction with the Period 3 indicator. The DiD approach is our preferred specification, as it accounts for common time trends, including potential learning or adaptation over time in the control condition. We additionally estimate within-subject comparisons for each treatment condition, comparing individual clearing decisions across pairs of periods. This approach, which is common in the literature (e.g., Andersson et al. 2018), captures individual-level changes without relying on a control condition. While it does not control for time trends, it complements the DiD strategy by highlighting behavioural shifts from the introduction and removal of incentives at the individual level.

The outcome variable in all models is the total number of parcels cleared by each participant, aggregated across the four rounds of a period. Period-level aggregation reflects the dynamic structure of the game, where rounds are neither independent nor fully comparable, precluding a round-level panel structure of the type used in repeated-game designs (e.g. Moros et al. 2023). For each approach, we estimate three different specifications. The first is a parsimonious model without any control variables. The second adds baseline controls for socio-demographic and farming characteristics (see Table 1), the average number of parcels cleared by the other members of a participant's group in the baseline or previous period, and village fixed effects to capture unobserved location-specific factors. The inclusion of village fixed effects is particularly important, since not all treatments were implemented in every village (following Handberg and Angelsen 2019). The third specification builds on the second but excludes participants who answered either of two control questions incorrectly (see next sub-section), in order to account for potential misunderstandings of game rules. Standard errors are clustered at the session level to account for intra-group dependence.

As a robustness check, we also estimate an ANCOVA model using baseline outcomes as covariates given the strong predictive power of baseline clearing behaviour. A formal description of all model specifications is provided Appendix B.

3.2. Data collection

We conducted the experiment in rural villages of the Kavango regions in northern Namibia, covering an area of about 250 km by 100 km along the Okavango River. Preparations began in winter 2016 (June – August) and involved qualitative interviews and surveys with Namibian officials, institutional representatives, traditional authorities and local farmers. The experiment and individual surveys were conducted in winter 2017 (May – August) in 25 randomly selected villages.

Villages were selected from a list of rural settlements located within a day's drive from the nearest tar road and at least 10 km from the urban centre of Rundu. The sample was stratified by distance to the Okavango River to ensure balance between the more densely populated riverside and the more forested inland areas (see maps in Appendix C). Only villages with a population of at least 80 were selected to ensure a sufficiently large pool of potential participants. We visited each village headperson several days ahead of the experiments to schedule a village meeting so that all villagers could be informed and invited. It was made clear that participants would receive monetary compensation, but that only a limited number of individuals could participate in the 'workshops', as we referred to the experimental sessions.

At the beginning of the village meeting, participants were randomly drawn by lot amongst those willing to participate. The same lots determined the allocation to experimental conditions. Participants were then spatially separated by experimental condition to prevent exposure to other groups' instructions or decisions. Our trained local research assistants explained the common-pool resource game, using posters and examples to illustrate different choices without recommending specific behaviour. Participants made their decisions using the data collection software "Kobo-Toolbox"[3] on tablet computers. Additional assistance was provided during the instruction phase to make sure that all participants were confident using the tablets independently by the time the game started. One trial round was played, the results from which were not made public.

After the game, participants answered two control questions to assess their understanding of the game mechanics. Wrong answers were neither corrected nor penalized; however, participants who answered incorrectly are excluded in one of our main estimation specifications. Out of 518 participating individuals, 480 answered both control questions correctly (93%, see also Panel B of Table 1). Full protocols for village meetings and workshops as well as game instructions can be found in Appendix D. Next, individual surveys of about 15 minutes were conducted with each participant to elicit background information, which we use as control variables in our estimations. All payments were made privately at the end of the workshop. Each workshop lasted about 4 hours, of which around 90 minutes were devoted to the game and instructions. Participants earned an average of 80N$ (~6US$), exceeding the average local daily wage (cf. Pröpper et al. 2013).

[3] See https://www.kobotoolbox.org/.

The study sample comprises 518 participants: 126 participants per treatment condition and 140 participants in the control condition. These allocations were informed by basic power calculations based on expected behaviour in the game, as precise ex-ante power analysis was not feasible given the novel design. Post-hoc power calculations are presented in Section 4.

Descriptive statistics are provided in Table 1. Panel A reports control variables, which represent socio-demographic, game-related, and farming characteristics of the sample. Panel B and C report the share of incorrect responses to control questions and average forest clearing in the baseline period. We see that the sample predominantly consists of smallholder farmers, about half of which are household heads. Slightly more women than men participated across all experimental conditions. About two-thirds of participants perceive their land tenure as 'very safe', and a similar proportion report that clearing rules are in place in their village. In the baseline period, players cleared an average of about two forest parcels, out of a maximum of four. Column (5) reports p-values from joint significance tests across experimental conditions, while asterisks in columns (1) to (3) indicate statistically significant pairwise differences from the control condition presented in column (4). None of the joint tests show significant imbalance, and only two out of the 36 pairwise comparisons in Panel A are significant at the 10% level, fewer than expected to occur by chance.

We observe stronger differences between treatment and control for the comprehension control questions reported in Panel B, with participants in the control group being more likely to answer these questions incorrectly, despite facing a less complex game without additional incentives. We therefore present robustness checks in Section 4 that exclude participants who answered at least one comprehension question incorrectly. However, since these comprehension questions were administered after the game, excluding participants based on their responses may introduce post-treatment bias (Montgomery et al., 2018). The restricted-sample results should therefore be interpreted with caution and are presented as supplementary evidence rather than as the basis for our main conclusions.

Table 1: Summary statistics of sample by experimental conditions

	mean (sd)				Control vs. T1 vs. T2 vs. T3
	Individual Reward (T1)	Collective Reward (T2)	Individual Fee (T3)	Control	*p-value*
	(1)	(2)	(3)	(4)	(5)
Panel A – control variables					
Participant age	38.13 (26.69)	38.99 (18.44)	40.43 (18.20)	40.57 (17.71)	0.76
Participant is female	0.60	0.54*	0.60	0.64	0.34
Participant's years of schooling years	6.25 (6.44)	6.34 (4.85)	6.24 (5.01)	5.72 (5.84)	0.61
Participant is native to village	0.69	0.63	0.68*	0.57	0.29
Participant is head of household	0.44	0.47	0.47	0.46	0.99
Household size	8.12 (5.35)	8.58 (5.78)	8.58 (5.56)	8.21 (6.13)	0.87
Number of participants in group who are friends or household members	3.67 (5.06)	3.32 (5.14)	3.04 (5.59)	3.39 (5.26)	0.82
Participant is farmer	0.94	0.95	0.95	0.94	0.93
Hectares cultivated	2.74 (2.78)	2.71 (1.79)	2.60 (2.33)	2.62 (1.91)	0.92
Clearing rules in village	0.59	0.60	0.72	0.59	0.34
Left any fields fallow in past 5 years	0.37	0.46	0.42	0.44	0.55
Tenure perceived as very safe	0.66	0.71	0.69	0.66	0.99
Panel B – sub-sample for robustness check					
Participant gave wrong answer to either of two control questions	0.06*	0.06	0.05**	0.13	0.21
Panel C – outcome in baseline period					
Total cleared in Period 1	1.94 (2.23)	2.10 (1.52)	1.91 (2.48)	1.84 (2.22)	0.71
Number of observations	126	126	126	140	518

Note: Column (5) shows the *p*-values for *F*-Tests on the joint significance across experimental conditions, with clustered standard errors at village level. Asterisks indicate statistical significance of pairwise *t*-tests between participants of the control condition and the respective treatment participants for the respective control variable, with * $p<0.10$ and ** $p<0.05$.

4. Results

4.1. Descriptive patterns and behavioural types

Before turning to our model-based econometric results, we first explore descriptive patterns in forest-clearing behaviour across the three periods. Figure 4 shows the average number of parcels cleared per participant by treatment condition and period. Clearing decreases in Period 2 not only in the treatment conditions, where incentives are introduced, but also in the control condition. This suggests that behavioural change may not be driven solely by incentives, but also by broader dynamics such as increased group awareness or adaptation to the game. These trends seem to continue in Period 3, where the control condition tends to decrease clearing even further. In contrast, average clearing increases modestly in the Individual and Collective Reward treatments following the removal of incentives, while it remains relatively stable in the Individual Fee condition.

To complement these aggregate trends, we classify our participants into three behavioural types: full cooperators (those who never clear new parcels), full defectors (those who clear in every round), and those who follow a mixed strategy. Figure 5 displays the shares of full cooperators and full defectors by treatment condition and period. In the baseline period, around half of participants pursue a mixed strategy, while the remainder split evenly between full cooperation and full defection. With the introduction of incentives in Period 2, the share of full cooperators rises in all treatment conditions, while the share of full defectors declines. In the control condition, cooperation also increases, but defection rather increases, suggesting the shift toward cooperation may come from participants abandoning mixed strategies rather than ceasing to defect. After the removal of incentives in Period 3, the proportion of full cooperators does not decline in any group. The small uptick in defection in the reward treatments suggests some reversion, but this is not mirrored by a loss of cooperators.

These descriptive patterns suggest that behavioural types are relatively stable across periods, but that incentives can induce short-term behavioural change and help stabilize cooperative behaviour beyond their active period. However, since this descriptive analysis does not allow us to isolate the causal impact of incentives, we now turn to formal regression models.

Figure *4: Total clearing in each period by experimental conditions*

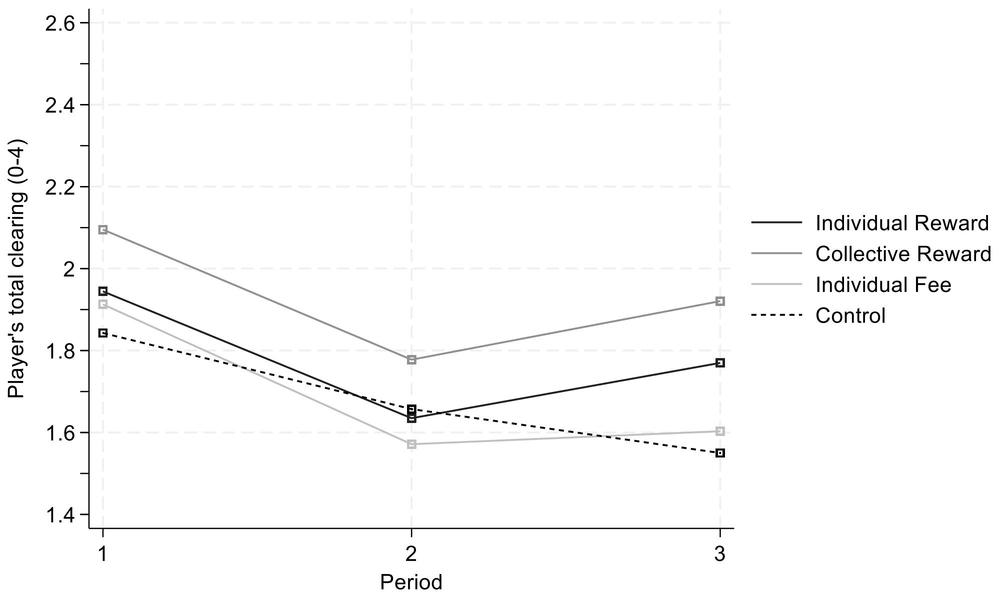
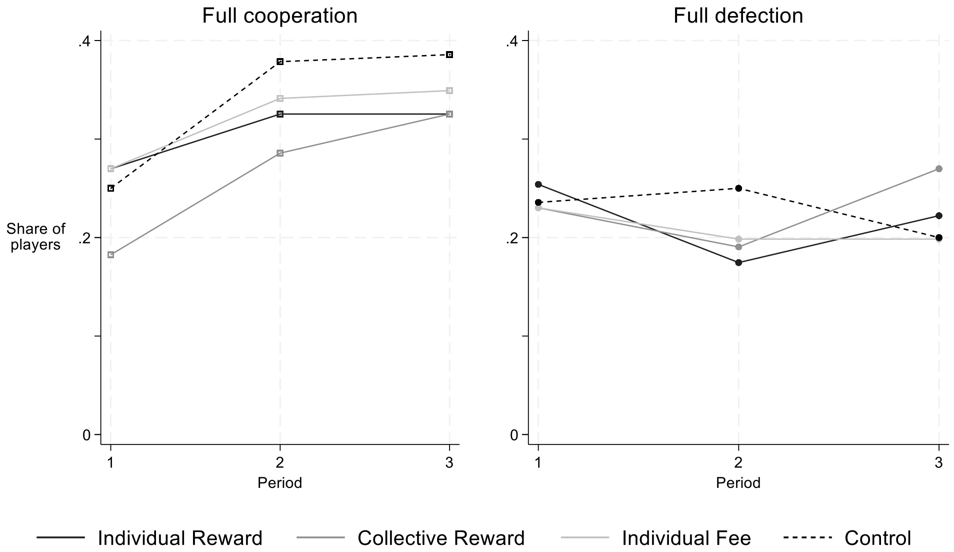


Figure 5: Full cooperation and full defection across periods and experimental conditions



4.2. Incentive effect

We begin by examining the *incentive effect* to find out whether the treatments led to reduced forest clearing while in place. We first consider within-subject estimates, comparing the differences in participant-level clearing between Period 1 and Period 2. These comparisons reveal statistically significant reductions at the 5% level in all three treatment conditions. The estimated decreases are substantial with around 0.3 parcels on average for all three treatment conditions, irrespective of the model specification (see columns 1 to 3 in Table 2).[4] Relative to baseline clearing rates, these decreases correspond to effects sizes of around -17%.

Importantly, the control condition also shows a notable, though smaller decrease in clearing from Period 1 to Period 2, corresponding to about 10% relative to its own baseline. This decrease in the control condition attenuates the estimated treatment effects in our preferred DiD framework. The resulting DiD estimates range from -0.12 to -0.18 parcels, none of which are statistically significant (columns 4 to 6 in Table 2). Pairwise tests likewise yield no significant differences between the three incentive types; for conciseness, only the *p*-values for

[4] The exclusion of respondents who failed the comprehension questions does not substantially affect the effect sizes or their statistical significance. This also applies to the analysis of incentive removal and the aggregate post-incentive effect.

the comparison between Individual Reward (T=1) and Individual Fee (T=3) are reported at the bottom of columns (4) to (6) of Table 2.[5]

4.3. Incentive removal effect

We now assess the *incentive-removal effect* as the change in clearing behaviour from Period 2 (incentives) to Period 3 (post-incentives). Across all three treatment conditions, clearing tends to increase once incentives are withdrawn, indicating a partial reversal of prior behavioural change. However, these increases are modest and not significant for any within-subject comparison. In contrast, the DiD estimates reveal increases that are significant at the 10% level. Specifically, clearing increases by 0.24 to 0.27 parcels (13 to 15%) in the Individual and Collective Reward conditions, depending on the model specification. While the increases in the Individual Fee condition remain smaller and insignificant, pairwise comparisons show no significant differences between the three treatment types in the magnitude of the removal effect.

[5] Simple between-subject comparisons, i.e., comparing groups within Period 2, do not yield significant incentive effects, either. Indeed, between-subject differences in Period 2 are small and comparable to baseline differences across groups in Period 1. As such, these comparisons are not considered informative and are not discussed further.

Table 2: Main results on clearing decisions

	Total cleared in a Period								
	within estimator			Difference-in-Differences					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Incentive effect (P2 – P1)									
Individual Reward (T1)	-0.31***	-0.31***	-0.31***	-0.12	-0.12	-0.16			
	(0.09)	(0.10)	(0.11)	(0.12)	(0.12)	(0.12)			
Collective Reward (T2)	-0.32**	-0.32**	-0.32**	-0.13	-0.13	-0.16			
	(0.12)	(0.13)	(0.13)	(0.14)	(0.14)	(0.16)			
Individual Fee (T3)	-0.34***	-0.34**	-0.33**	-0.16	-0.16	-0.18			
	(0.11)	(0.12)	(0.13)	(0.15)	(0.15)	(0.17)			
Control condition	-0.19**	-0.19**	-0.16*						
	(0.07)	(0.08)	(0.09)						
Incentive-removal effect (P3 – P2)									
Individual Reward (T1)	0.13	0.13	0.14				0.24*	0.24*	0.27*
	(0.10)	(0.11)	(0.12)				(0.13)	(0.13)	(0.13)
Collective Reward (T2)	0.14	0.14	0.14				0.25*	0.25*	0.27*
	(0.10)	(0.11)	(0.11)				(0.14)	(0.15)	(0.15)
Individual Fee (T3)	0.03	0.03	0.03				0.14	0.14	0.15
	(0.06)	(0.06)	(0.07)				(0.12)	(0.12)	(0.12)
Control condition	-0.11	-0.11	-0.12						
	(0.10)	(0.11)	(0.11)						
Aggregate post-incentive effect (P3 – P1)									
Individual Reward (T1)	-0.17	-0.17	-0.17	0.12	0.12	0.11			
	(0.12)	(0.12)	(0.13)	(0.13)	(0.13)	(0.14)			
Collective Reward (T2)	-0.17	-0.17	-0.18	0.12	0.12	0.10			
	(0.16)	(0.17)	(0.18)	(0.19)	(0.19)	(0.19)			
Individual Fee (T3)	-0.31***	-0.31**	-0.31**	-0.02	-0.02	-0.03			
	(0.10)	(0.11)	(0.11)	(0.16)	(0.16)	(0.16)			
Control condition	-0.29**	-0.29**	-0.28**						
	(0.11)	(0.12)	(0.12)						
Village FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Others' choices	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Exclusion of wrong control answers	No	No	Yes	No	No	Yes	No	No	Yes
Number of observations[A]	252-280	252-280	238-244	1554	1554	1440	1036	1036	960
Number of unique participants[A]	126-140	126-140	119-122	518	518	480	518	518	480
Adjusted R-squared	0.00-0.01	0.52-0.86	0.50-0.87	0.00	0.17	0.18	0.00	0.17	0.18
p-value: $\beta_{T1} = \beta_{T3}$ (P2)	–	–	–	0.83	0.83	0.89	–	–	–
p-value: $\beta_{T1} = \beta_{T3}$ (P3)	–	–	–	0.48	0.48	0.48	0.38	0.38	0.29
Baseline control mean	1.57-2.10	1.57-2.10	1.58-2.12	1.84	1.84	1.94	1.66	1.66	1.79

Note: A = For the within-subject models, a range is shown, as the number of observations used slightly varies across treatment conditions; standard errors clustered at village level in brackets; *** p<0.01, ** p<0.05, * p<0.1.

4.4. Aggregate post-incentive effect

The *aggregate post-incentive effect* as the change between Period 3 and Period 1 reflects the combined impact of both the incentive phase and the subsequent removal of incentives. From a within-subject perspective, average clearing in Period 3 remains below baseline levels for all experimental conditions. The reduction is statistically significant only for the Individual Fee treatment, with a substantial average decrease of 0.31 parcels (-16%). The control condition shows a similar overall declining trend. Applying the DiD framework, aggregate post-incentive effects appear weaker and remain statistically insignificant throughout. For the two reward treatments, the estimates shift signs compared to the within-subject analysis. The estimate for the Individual Fee treatment is close to zero.

Robustness checks using ANCOVA estimations (Appendix Table B1) largely confirm the DiD findings, while indicating a statistically significant aggregate crowding-out effect – i.e. increased clearing – for the Individual Reward treatment. We further scrutinize our estimation results by determining post-hoc Minimum Detectable Effects (MDEs) using a significance level of α=0.10 and power of 0.80. The MDE is calculated by scaling the estimated standard errors of the respective treatment coefficients from our main specifications by the corresponding critical values.[6] Based on the observed standard errors, MDEs for the incentive effect range from 0.30 to 0.42 parcels, while those for the aggregate effect range from 0.32 to 0.47 parcels. These correspond to approximately 15–25% of baseline clearing levels. This suggests that while moderate-to-large effects are within detectable range, smaller impacts may remain statistically undetected.

5. Discussion

This section discusses our experimental results in light of existing literature, focusing on three key aspects: (i) the observed behavioural responses to incentives and their termination, (ii) the decline in forest clearing in the control group, and (iii) the role of game-specific or contextual

[6] With a desired statistical power $(1-\kappa)$ of 0.80, the MDE is calculated as follows: $MDE = \left(t_{\frac{\alpha}{2}} + t_{1-\kappa}\right) \times \sigma_{\widehat{\beta}}. = (t_{\frac{\alpha}{2}} + |z_{.8}| \times \sigma_{\widehat{\beta}}$. For a significance level (α) of 0.10, it becomes $(|z_{.05}|+|z_{.8}|) \times \sigma_{\widehat{\beta}} = (1.64 + 0.84) \times \sigma_{\widehat{\beta}} = 2.48 \times \sigma_{\widehat{\beta}}$. Hence, the scaling factor is 2.48. For comparison, for α of 0.05, this equation yields a factor of 2.80.

features in explaining differences from previous studies on the lasting effects of conservation incentives.

In designing the external incentives – Individual Reward, Collective Reward, and Individual Fee – we calibrated payoffs such that clearing remained slightly more profitable than staying, thereby preserving the underlying social dilemma. Within this preserved dilemma structure, average cooperation remained well above the Nash equilibrium of full defection, yet still far below the social optimum of full cooperation in all conditions and periods. This suggests that, while most participants did not act as purely self-interested agents, the game's inherent social dilemma remained unresolved.

Incentive effects proved to be modest. A within-subject analysis suggests average reductions of around 17%, but coefficients shrink to about half and become statistically insignificant in the Difference-in-Differences framework. These results suggest that moderate monetary incentives may not be sufficient to shift cooperation substantially in settings where social dilemmas persist. More fundamentally, our results echo prior research, suggesting that incentives would need to be higher to entirely overcome the dilemma (Gneezy and Rustichini 2000b; cf. also Prediger et al. 2014 and Handberg and Angelsen 2019). Our results on the aggregate effect after the termination provide no clear evidence of lasting net motivational crowding effects, aligning with recent literature on the termination of incentives (e.g., Maca-Millán et al. 2021).[7] It does not support, however, indications for crowding-in effects after the removal of reward incentives, as observed, for example by Andersson et al. (2018), Lliso et al. (2022), and Moros et al. (2023). Only from the within-subject perspective, our results seem to suggest an aggregate post-incentive crowding-in, echoing Kaczan et al. (2019). Overall, the literature is heterogeneous and tends to find no or only weak crowding-in effects following the termination of incentives. This is broadly consistent with our findings but underscores substantial variability across studies.

[7] Another factor shaping lasting effects may be the framing and interpretation of negative incentives. While we framed negative incentives rather neutrally as a "fee" that has to be paid for clearing, lasting effects might depend on how exactly they are implemented and presented. If the fee is viewed as a controlling punishment, its termination might crowd-in intrinsic motivation due to a perceived relative increase in self-determination. If, conversely, the fee is presented and understood as a tax or a price for taking resources, appropriation rates might increase after termination as the respective policy could have caused a monetarization of the resource.

A striking and unanticipated finding is the continuous decrease in forest clearing in the control condition. This contrasts with the widely observed pattern in the literature, where cooperation typically decreases over time in repeated social dilemma games due to conditional cooperation driven by self-interested behaviour (e.g., Ledyard 1995; Neugebauer et al. 2009; Fischbacher and Gächter 2010). In our experiment, by contrast, cooperation increases over time, a trend also reported in other studies (Andreoni 1988; Croson 1996; Cookson 2000; Croson et al. 2005; Eckel et al. 2016; Chaudhuri and Paichayontvijit 2017). The structure of our game includes two design features that may explain this result. First, the game is divided into distinct periods, each beginning with a reset of the forest stock. This may create the perception of an opportunity to revive cooperation as argued by Andreoni (1988) and Cookson (2000). Second, unlike standard repeated games with independent rounds, our game design involves dynamic interdependence across rounds within each period as the forest state carries over round to round. This may reinforce a longer time horizon and the value of restraint.

Another possible explanation for the distinct pattern in the control condition stems from our experimental design including a true control condition – an element that is often not included in similar studies. Control participants may have been better able to focus on the cooperative structure of the game, while those exposed to incentives generally have to process more complex payoff structures (Pashler 1994; Loewenstein and Wojtowicz 2025). Finally, it remains possible that all groups were still adjusting toward behavioural steady states. Diverging results using within-subject comparisons and Difference-in-Differences further underscore the value of including a non-incentivized control condition. They illustrate how behavioural developments over time – independent of treatment – can substantially affect inference, making it essential to account for developments over time both in games and real-world policy evaluations.

6. Conclusion

Our study contributes to the ongoing debate on the effectiveness of monetary incentives in ecosystem conservation, drawing on a novel forestry-framed common-pool resource game tailored to a semi-arid context with negligible forest regrowth. While all incentive types tested – individual, collective, positive, and negative – temporarily influenced behaviour, none yielded large or lasting changes in cooperation. After incentives were removed, behavioural effects largely dissipated, and crowding-out or crowding-in effects remained weak and statistically inconclusive. No incentive type outperformed the others in our study. Post-hoc power analysis suggests that only effects exceeding 15–25% of baseline clearing would likely have been detected, indicating that smaller yet potentially policy-relevant effects may have gone undetected.

These findings caution against a reliance on moderate monetary incentives in persistent social dilemmas. The policy choice of whether and which incentive to adopt is likely to depend more on contextual factors, such as local monitoring opportunities and capacities, land rights, or political and economic conditions. At the same time, recent work by Grillos et al. (2024) highlights that collective PES can encourage the creation of local institutions, showing that incentives may influence cooperation through channels beyond immediate extraction choices.

From a research design perspective, our findings illustrate how critically both experimental and inferential design choices can substantially influence observed outcomes and their interpretation. Our game was carefully tailored to reflect key contextual features – notably low forest regrowth – which necessarily interact with design elements like payoff structures, round-to-round feedback, and forest stock rests across distinct periods. These features likely influence participants' learning and cooperation patterns. At the same time, our inferential strategy – relying on both within-subject comparisons and Difference-in-Differences – revealed how behavioural trends over time, even in the absence of treatment, can substantially affect estimated impacts. While these features enhance the internal validity and contextual realism of the experiment, they also underscore the need for scrutiny in how experimental games are framed. As also highlighted by Reitmann and Sievert (2024), behavioural responses in framed field experiments can be highly context-specific, calling for careful design to elicit the intended motivations – and for caution in generalizing results beyond the experimental setting, whether to other contexts or to policy applications. This is further reflected in the variability of findings across experimental studies on the effects of incentives and their persistence, which show an overall inconclusive picture.

Given this likely dependence on context and game design features, future research could build on our experimental design to systematically test how elements such as payoff structures, the framing of negative incentives, or game duration affect motivational dynamics and conservation outcomes. Ideally, such experimental efforts are complemented by studies embedded in real-world PES applications, helping to bridge the gap between behavioural insights and applied policy design.

❖

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CRediT authorship contribution statement

NCH: Conceptualization; Data curation; Formal analysis; Investigation; Methodology; Project administration; Resources; Software; Supervision; Validation; Visualization; Writing – original draft; Writing - Review & Editing **GB:** Conceptualization; Formal analysis; Methodology; Project administration; Software; Supervision; Validation; Visualization; Writing – original draft; Writing - Review & Editing **MK**: Conceptualization; Data curation; Funding acquisition; Methodology; Project administration; Resources; Supervision; Writing – original draft

Declaration of competing interests

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The data and analysis code are available at https://osf.io/v8afj/

SUPPLEMENTARY INFORMATION FOR

Introducing and Terminating Monetary Incentives in Non-Regenerating Forests: Insights from a Framed Field Experiment

Appendix A: Formalization of Common-Pool Resource Game

A.1: The Baseline Game (Period 1)

The experiment was set up as a dynamic common-pool resource game over multiple rounds in which the social benefit occurs at the end of a period. Since no discount rates are applied across the rounds, each decision equally affects the final private and social outcome, and the personal payoff can effectively be written as the sum of all four rounds. The order of decisions did therefore not make any difference in economic terms. It might, however, play a behavioural role, as in the dynamic game players would learn about the other group members' decision after each round and possibly respond to that by behaving in a certain way in the following rounds. (Note: Players might also anticipate reciprocal behaviour of others and therefore cooperate more early on in the game.)

We firstly write the equation for the baseline payoff of a player in each individual round (R): Player i makes a dichotomous decision to either clear or not clear a new field. Let c_i be 1 if player i decides to clear and 0 if they decide to stay. All other players j are confronted with the same decision. Then equation (1) becomes:

$$\underbrace{\boldsymbol{R_i(c_i\,,c_j)}}_{\text{payoff of player } i \text{ in one round}} = \underbrace{\boldsymbol{3.50 * c_i}}_{\text{yield from clearing}} + \underbrace{\boldsymbol{1 * (1 - c_i)}}_{\text{yield from staying}} + \underbrace{\boldsymbol{1 * (1 - c_i)}}_{\text{benefit from spared forest}} + \underbrace{\boldsymbol{1 * (6 - \sum_{j=1}^{6} c_j)}}_{\text{benefit from forest spared by others}}$$

which can be reduced to

$$\boldsymbol{R_i(c_i\,,c_j) = 8 + 1.50 * c_i - \sum_{j=1}^{6} c_j}$$

R_i = payoff player i in any round

c_i = decision between *0 = stay* and *1 = clear* by player i

c_j = decisions between *0 = stay* and *1 = clear* by other players *j={1,2,3,4,5,6}*

For the whole game over 4 rounds (r) the total payoff of a player (P) can be written as a function of all decisions made by that player and their group members. The player's total payoff then equals the sum of the payoffs over all four rounds, as presented in the following equation (2):

$$P_i = \sum_{r=1}^{4} R_{i,r}\left(c_{i,r}, c_{j,r}\right) = 3.50 * \sum_{r=1}^{4} c_{i,r} + 1 * \sum_{r=1}^{4}\left(1 - c_{i,r}\right) + 1 * \sum_{r=1}^{4}\left(1 - c_{i,r}\right) + 1 * \sum_{r=1}^{4}\left((6 - \sum_{j=1}^{6} c_{j,r})\right)$$

total payoff of player *i*	yield from clearing	yield from staying	benefit from spared forest	benefit from forest spared by others

which can be reduced to

$$P_i\left(c_{i,r}, c_{j,r}\right) = 32 + 1.50 * \sum_{r=1}^{4} c_{i,r} - \sum_{r=1}^{4}\sum_{j=1}^{6} c_{j,r}$$

P_i = total payoff of player *i* in the game (= period)

$c_{i,r}$ = decision between *0 = stay* and *1 = clear* by player *i* in round *r = {1, 2, 3, 4}*

$c_{j,r}$ = decisions between *0 = stay* and *1 = clear* by other players *j={1,2,3,4,5,6}* in round *r = {1,2,3,4}*

As $c_{i,r}$ is now the only remaining variable that player *i* can manipulate, it becomes clear that in each round clearing increases their individual payoff and also for the whole game it is individually optimal to clear a new field in every round. For the group as a whole, however, the total payoffs are maximized when no player clears any new field, which can be seen in the following equation (3) by setting $c_{i,r} = c_{j,r} = c_r$:

$$S(c_r) = 7 * \left[32 + 1.50 * \sum_{r=1}^{4} c_r - 6 * \sum_{r=1}^{4} c_r\right] = 224 - 31,5 * \sum_{r=1}^{4} c_r$$

S = social payoff of the group, i.e., of all players combined

c_r = decision between *0 = stay* and *1 = clear* by each player in round *r = {1, 2, 3, 4}*

As clearing in each round linearly reduces the social payoff, the maximum is found at $c_r = 0$, i.e., no clearing. In this case the social benefit is 224N$ which equals 32N$ per player per period. If all players act entirely selfishly and always clear, the social payoff sums up to only 98N$, which is 14N$ per player.

A.2: The Incentives Period (Period 2)

After the first four rounds, the game restarted. For the second period of the game, the game was therefore starting with a new forest stock of 28 parcels but now the incentive treatments were introduced for the three treatments conditions.

➢ Control Condition

In the control treatment players played the same game as in the first period, i.e., without receiving any additional incentives. The payoff equation is therefore the same as in the baseline game of period 1 and can be written per round as equation (4a):

$$\boldsymbol{R_i(c_i\,,c_j) = 8 + 1.50 *\ c_i - \sum_{j=1}^{6} c_j}$$

and over all four rounds as equation (4b):

$$\boldsymbol{P_i(c_{i,r}\,,c_{j,r}) = 32 + 1.50 * \sum_{r=1}^{4} c_{i,r} - \sum_{r=1}^{4}\sum_{j=1}^{6} c_{j,r}}$$

➢ Treatment 1: Individual Reward

In the Individual Reward treatment, players were receiving an individual bonus of 1N$ for each round of not clearing. For this treatment, payoffs per round can be written as equation (5a):

$$\boldsymbol{R_i(c_i\,,c_j) = 3.50 * c_i + 1 * (1 - c_i) + 1 * (1 - c_i) + 1 * \left(6 - \sum_{j=1}^{6} c_j\right)} + [\textbf{Incentive}: \boldsymbol{1 * (1 - c_i)}]$$

which can be reduced to

$$\boldsymbol{R_i(c_i\,,c_j) = 9 + 0.50 * c_i - \sum_{j=1}^{6} c_j}$$

over all four rounds this can be written as equation (5b):

$$\boldsymbol{P_i(c_{i,r}\,,c_{j,r}) = 36 + 0,50 * \sum_{r=1}^{4} c_{i,r} - \sum_{r=1}^{4}\sum_{j=1}^{6} c_{j,r}}$$

➢ Treatment 2: Collective Reward

All players in the group were receiving a bonus payment of 0.50N$ in each round, as long as the total forest stock remained larger than 50% of the initial forest, i.e., larger than or equal to 14 parcels. The bonus payment was set to only 0.50N$ per round in order to achieve a comparable final outcome as the other treatments. The collective rewards are always paid for the first two rounds, as it is not possible for the group to clear more than 14 parcels of forest by the end of the second round. Payoffs per round are then given by equation (6a):

$$\boldsymbol{R_i(c_i\,,c_j) = 3.50 * c_i + 1 * (1 - c_i) + 1 * (1 - c_i) + 1 * \left(6 - \sum_{j=1}^{6} c_j\right)} + [\textbf{Incentive}: \boldsymbol{0.50\ if\ forest \geq 14}]$$

which can be reduced to

$$\boldsymbol{R_i(c_i\,,c_j) = 8 + 1.50 * c_i - \sum_{j=1}^{6} c_j\ \ +\ 0.50, if\ forest \geq 14}$$

over all four rounds this can be written as equation (6b):

$$\boldsymbol{P_i(c_{i,r}\,,c_{j,r}) =\ \ 32 + 0,50 * \sum_{r=1}^{4} c_{i,r} - \sum_{r=1}^{4}\sum_{j=1}^{6} c_{j,r} +\ 0,50 * \sum_{r=1}^{4} w_r}$$

with $\boldsymbol{w_r}$ = 1 if stock remains ≥**14**

= 0 otherwise

- Treatment 3: Individual Fee

In this treatment players had to pay a fee of 1N$ for clearing forest in each round. The fee was not framed specifically as a punishment, penalty or tax, but was just neutrally called a fee. The reason for selecting the term "fee" is based on the consideration of trying to make each treatment's framing as comparable as possible. We believed that a stronger wording as "punishment" or "penalty" sounds overly deterring, possibly creating a demand-effect in participants' behaviour. Calling it a "tax" on the other hand shifts framing towards a market-based perspective; understanding the fee as a "fair price" for clearing is not the treatment's intention, either. In conclusion, "fee" was selected as the most adequate equivalent to "rewards". Consequently, payoffs per round are as in equation (7a):

$$\boldsymbol{R_i(c_i\,,c_j) = 3.50 * c_i + 1 * (1 - c_i) + 1 * (1 - c_i) + 1 * \left(6 - \sum_{j=1}^{6} c_j\right)} - [\textbf{Incentive}: \boldsymbol{1 * c_i}]$$

which can be reduced to

$$\boldsymbol{R_i(c_i\,,c_j) = 8 + 0.50 * c_i - \sum_{j=1}^{6} c_j}$$

over all four rounds this can be written as equation (7b):

$$\boldsymbol{P_i(c_{i,r}\,,c_{j,r}) =\ 32 + 0,50 * \sum_{r=1}^{4} c_{i,r} - \sum_{r=1}^{4}\sum_{j=1}^{6} c_{j,r}}$$

It is important to note that all incentive treatments did not change the individually or socially optimal decisions. It remained individually optimal to always clear and socially optimal to never clear. The gains from clearing were then just smaller than in the baseline game. The

Individual Reward and the Individual Fee treatment were directly comparable against each other as they both reduced the gains from clearing by 1N$. The Collective Reward treatment changed the structure of the game as it did then depend on the current remaining size of forest, and thereby on the decisions of all group members and not just one player, whether the rewards could be achieved in the respective round or not.

A.3: The Post-incentives Period (Period 3)

After the end of the last round in the second period, players of the incentive treatments were informed that for the next period the incentives were going to be removed again. For this third period, all players thus once more played the game without incentives, like in the first period. In order to avoid end-game effects, players were never told how many periods, i.e., repetitions of the game, they were going to play in total.

Appendix B: Estimation equations and ANCOVA results

We estimate the treatment effects described in Section 3.1 using the following specifications. All models are estimated at the participant–period level, with the outcome variable Y_{ip} denoting the number of parcels cleared by individual i in period p. Data are available for three periods: $p = 1$ (baseline), $p = 2$ (incentives), and $p = 3$ (post-incentives). Treatment assignments T_{ik} and period indicators P_p are binary variables.

Difference-in-Differences Specification

$$Y_{ip} = \alpha_0 + \sum_{p=2}^{3} \alpha_{1p} P_p + \sum_{k=1}^{3} \alpha_{2k} T_{ik} + \sum_{k=1}^{3} \beta_{2k} (T_{ik} \times P_2) + \sum_{k=1}^{3} \beta_{3k} (T_{ik} \times P_3) + \mathbf{X}_i' \alpha_3 + \gamma_j + \epsilon_i \quad [1a]$$

Here, X_i is a vector of baseline socio-demographics and farming characteristics, γ_j denotes village fixed effects and ϵ_i is the unobserved household-specific error term. The key impact estimates are the coefficients β_{2k} (*incentive effect*) and β_{3k} (*aggregate post-incentive effect*).

We estimate a model using only Periods 2 and 3, referred to as p^+ in the following equation, to isolate the *incentive-removal effect*,β_{3k}:

$$Y_{ip^+} = \alpha_0 + \alpha_{13} P_3 + \sum_{k=1}^{3} \alpha_{2k} T_{ik} + \sum_{k=1}^{3} \beta_{3k} (T_{ik} \times P_3) + \mathbf{X}_i' \alpha_3 + \gamma_j + \epsilon_i \quad [1b]$$

Within-subject specification

We also estimate within-subject regressions for each treatment condition $k = \{1,2,3\}$ and period pair $p^+ \in \{(1,2), (2,3), (1,3)\}$:

$$Y_{ip^+} = \alpha_0 + \beta_{p^+k} P_{p^+} + \mathbf{X}_i' \beta_5 + \gamma_j + \epsilon_i \; [2]$$

These models yield nine estimations in total. The coefficient β_{p^+k} captures the average within-subject change in clearing behaviour for each treatment condition across each pair of periods.

ANCOVA Specification

As a robustness check, we estimate the following ANCOVA models, which condition on past outcomes to control for baseline differences, for Periods 2 and 3 (p^+) separately:

$$Y_{ip^+} = \alpha_0 + \sum_{k=1}^{3} \beta_k T_{ik} + \alpha_2 Y_{ip=1} + \mathbf{X}_i' \alpha_3 + \gamma_j + \epsilon_i \quad [3a]$$

For the *incentive-removal effect* using outcome from Period 2 as a covariate:

$$Y_{i3} = \alpha_0 + \sum_{k=1}^{3} \beta_k T_{ik} + \alpha_2 Y_{ip=2} + \mathbf{X}_i' \alpha_3 + \gamma_j + \epsilon_i \quad [3b].$$

In both cases, the coefficient β_k captures the treatment effect in the respective period, controlling for the previous period's outcome. Equation [3a] provides the *incentive effect* for $p^+ = 2$ and the *aggregate post-incentive effect* for $p^+ = 3$, while equation [3b] identifies the *incentive-removal effect*.

ANCOVA results are shown in Table B1.

Table B1: Results on clearing decisions using ANCOVA estimations

	Total cleared in Period 2			Total cleared in Period 3			Total cleared in Period 3		
	(1)	(2)	(3)	(5)	(6)	(7)	(8)	(9)	(10)
Incentive effect (P2 – P1)									
Individual Reward (T1)	-0.10	0.00	-0.03						
	(0.11)	(0.12)	(0.13)						
Collective Reward (T2)	-0.07	0.01	-0.06						
	(0.14)	(0.16)	(0.18)						
Individual Fee (T3)	-0.14	-0.08	-0.14						
	(0.14)	(0.17)	(0.19)						
Incentive-removal effect (P3 – P2)									
Individual Reward (T1)				0.24*	0.29**	0.29**			
				(0.13)	(0.13)	(0.13)			
Collective Reward (T2)				0.27*	0.30*	0.27*			
				(0.14)	(0.14)	(0.15)			
Individual Fee (T3)				0.12	0.10	0.08			
				(0.13)	(0.13)	(0.12)			
Aggregate post-incentive effect (P3 – P1)									
Individual Reward (T1)							0.14	0.27**	0.26*
							(0.12)	(0.13)	(0.14)
Collective Reward (T2)							0.18	0.29	0.23
							(0.18)	(0.20)	(0.20)
Individual Fee (T3)							0.00	0.02	-0.02
							(0.15)	(0.17)	(0.18)
Village FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other group member choices	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Exclusion of wrong control answers	No	No	Yes	No	No	Yes	No	No	Yes
Number of observations	518	518	480	518	518	480	518	518	480
Adjusted R-squared	0.56	0.57	0.56	0.62	0.64	0.64	0.53	0.56	0.54
p-value: $\beta_{T1} = \beta_{T3}$ (P2)	0.78	0.62	0.53	–	–	–	–	–	–
p-value: $\beta_{T1} = \beta_{T3}$ (P3)	–	–	–	0.35	0.14	0.07	0.43	0.25	0.21
Baseline control mean	1.84	1.84	1.94	1.66	1.66	1.79	1.84	1.84	1.94

Note: Standard errors clustered at village level in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Appendix C: Maps of study area

Figure C1: Map of Namibia and study area

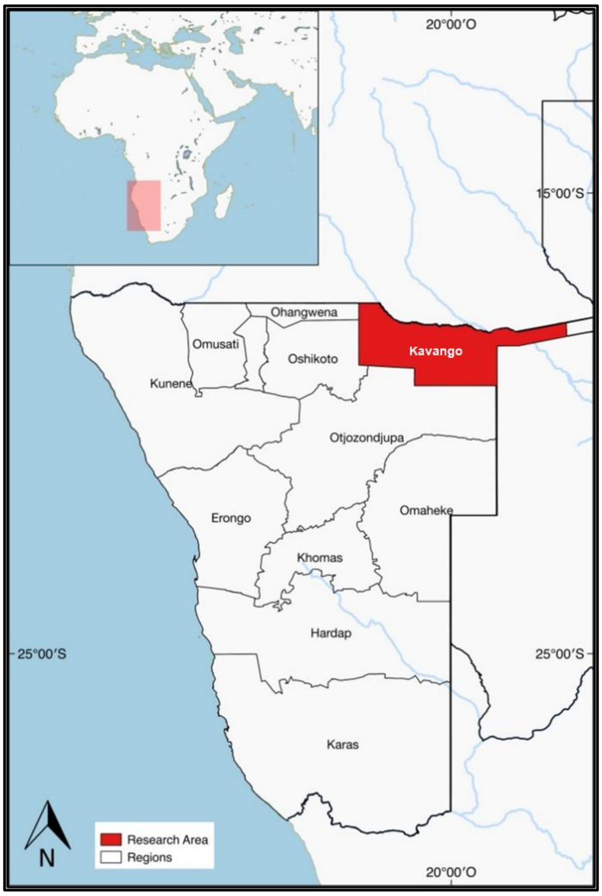
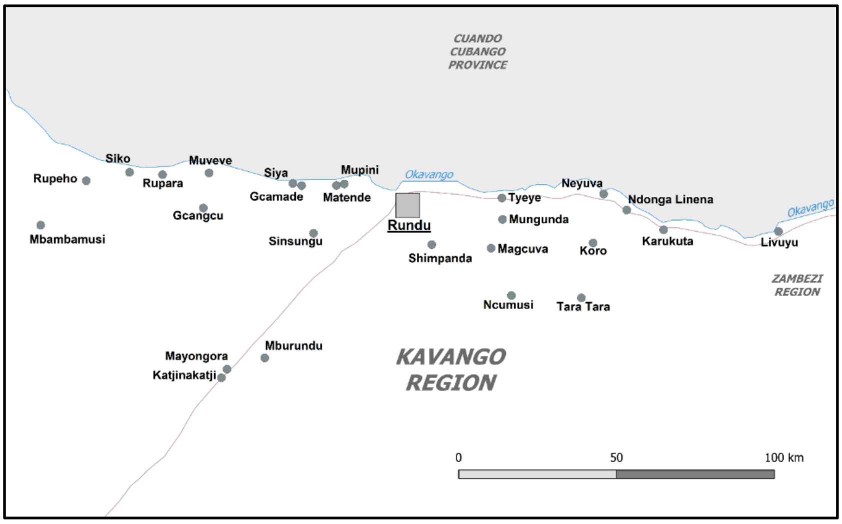


Figure C2: Map of study area with visited villages



Appendix D: Experimental workshop protocols

All protocols and instructions were translated by our assistants from English into the respective local languages – primarily Rukwangali and Rusambyu as well as their regional dialects – and then translated back into English by another assistant in order to ensure that all translated instructions were on point. Also, all wordings and phrases used in the instructions were discussed intensively with our local assistants in preparation of the experiment as to best reflect the local conditions and to make all instructions as clear and easily understandable as possible.

D.1: Village meeting introduction

To begin with, we would like to thank you all for coming here today. My name is Christian Hoenow. I am from the University of Marburg in Marburg. Together with the Ministry of Agriculture, Water and Forestry we are conducting research under the SASSCAL project. [NAME OF EXPRIMENTERS] here are also part of the project.

Doing research means we are just here to collect data, but we do not bring any type of development project into the village. What you answer in the workshop will not determine whether villages are selected for future projects. Also, we are not here to teach you anything. On the contrary, we want to learn from you. About how you do agriculture and how you use the forest. These information will help us identify potential problems and come up with possible solutions. Such solutions however will not be brought by us. We only write down what we find and then the information can later be used by the Government or by Organizations.

Today we would like to conduct a small workshop with a certain number of people. At the end of the workshop we will also ask you several questions one by one. Unfortunately, not everyone from this village can participate since the workshop can only include a certain number of participants (28). Since we want everyone to have the same chance to participate, we have prepared a bag with as many cards as people present. Each adult that is older than 18 years may draw a card. We will ask you to fully concentrate on the workshop and we will be asking many questions. If you already know that you cannot attend for up to 5 hours, or do not wish to answer many questions, you should please not draw. Participation is, of course, voluntary!

- If you draw a [COLOR] card, you will participate in the workshop, which is led by [NAME OF EXPERIMENTER]

- If you draw a white card, you unfortunately cannot participate in any of the events.

Do you have any questions?

[LET EVERY ADULT DRAW A CARD]

[CONTINUE WITH GENERAL INSTRUTIONS]

D.2: General instructions

To begin with, thank you again for coming here today. We will conduct a workshop where you will earn real money. Different participants may receive different amounts of money. The money that you can earn is not our private money, but it is provided by the German Government.

All information collected today will be used for research only. Neither the Government of Namibia or Germany nor any other organization will receive the data for other purposes. Also, neither your names nor any village-specific information will be revealed in the results. All decisions made will remain anonymous to others.

The schedule for today looks as follows:

1. We will explain the procedure of the workshop.
2. We will play small workshop like a game. This is when you can earn money.
3. After the games each of you answers a short survey-questionnaire.

It is not the purpose of the game to be better than others. Also, there are no right or wrong answers and we do not expect anything in particular from you. All payments are determined exactly the way we will explain to you later.

Before starting, I would like to give you some general information:

1. If at any time, you think that this is something that you do not wish to participate in for any reason, you are free to leave. You will however only get all money you earned if you stay until the end of the workshop.
2. If you already know that you will not be able to stay for at least 5 hours, then you should leave right away.
3. We require your complete and undistracted attention. Please, follow the instructions carefully and do not use your phone or engage in any other distracting activity.
4. It is not allowed to talk to each other during the workshop, unless we tell you to. You can ask questions after raising your hand. If you talk to each other when you are not allowed to, you will be excluded from the workshop and the payments.
5. Every one of you has received a unique player ID. Please keep this ID until the end. You must return the ID before receiving the money at the end of the workshop.

After knowing these rules, is there anybody who does not wish to participate anymore?

Do you have any questions?

D.3: Game instructions

[Seat groups 1-7 in blocks according to their ID color and number]
I will now explain what you will do in the workshop today. Please pay attention to what I say, as it is important that you understand everything. We have allocated you into three groups of 7 people each. I am the instructor and will tell you what the rules are, answer questions if there are any and let you know about the outcomes. We will play something similar to a game. I do not participate in the game myself. The game is played within your group of 7 people only.

In this game you will earn real money. The amount you receive depends on your decisions and the decision of other players. The numbers we mention in the game are exactly real dollar. We will note for each of you how much you earn during the game and in the end we will give you the money in Namibian Dollars. In total you will earn between 24 and 120 Namibian Dollars at the end of the day.

It is not the purpose of the game to be better than others. There is no right or wrong, good or bad. It is possible that some players get more than other, that all players get a lot or that all players get only little. It all depends on your decisions in the game and the decisions of your group members.

The game we play is about agriculture and forest use. You will be making decisions about whether you want to clear forest in order to clear a new field or if you want to continue using your old field. There is a forest of 28 hectares [show at TABLE_FOREST] that belongs to your group. You will play the game over 4 rounds. In the beginning the size of the forest is 28 hectares.
As you know, the soil fertility decreases over time. In the game we have old and new fields. Each player starts with an old field that will give you 1$ per round. If you decide to clear forest to get a new field, you will earn 3,50$ in that round. In every round you can decide to clear a new field. That means you clear one hectare of forest and cultivate a new field there. Thus, each clearing causes the forest stock to decrease by one hectare. A new field will give you 3,50$ only once. In the next round, the new field becomes an old field and gives you 1$, unless you clear a new field again. Clearing means you leave the old field and again get 3,50$ instead of 1$.
We are aware that in reality some of you are cultivating more than one hectare and more than one field at the same time because they have a larger household and better equipment.

But for this game, every player can only cultivate one field at the same time. That means, when you decide to clear a new field, you will only be using the new field and leave the old one fallow. Also we know that the soil fertility is slowly decreasing over time, so that in reality there are more conditions than just new and old fields.

However, we want to keep the game simple and are therefore only playing with old and new fields. For the game this works as follows: In the first round you have to decide whether you want to clear a new field or stick to your old one. If you stay in the old one you will get 1$ in the first round. If you clear and cultivate a new field you will get 3,50$ for the first round.

Every round you make the decision to start with a new field or stay on the old one.

[**Show example for decisions:**

eg. "Clearing twice" : 3,50 + 1 + 3,50 + 1 (1st and 3rd round)

or "Clearing three times" : 3,50 + 3,50 + 1 + 3,50 (1st, 2nd, and 4th round)]

It is possible to clear a new field in every round, which means you get 3,50$ in every round. As you see, each of you will make 4 decisions in total. Remember that there is 28hectares of forest and there are 7 players in the group. So even if everyone is clearing every round there will be 7 player = 7 hectares that they can clear per round. Times 4 rounds equals 28 hectares. It is therefore not possible to deplete to whole forest before the end of the game, even if everyone is clearing new forest all the time. [show FIGURE_FOREST]

So this is the agricultural side of the game and obviously it would be reasonable to start a new field every round in order to get the highest yields from the fields. The fields are your own and therefore the yields you receive from your field are your own personal money that you can keep. There is however also another part of the game which is about the forest. You are aware that forests are not worthless but are valuable natural resources for everyone. This could for example be fruits, mushrooms, timber or firewood that you collect from the forest. Also, cattle can graze in the forest, especially during times of harvest. Finally, forests play an important role in keeping the environment healthy and sustainable. And a large remaining forest can be used by future generations for both forest benefits and leaving options for new fields. These benefits are represented in the game by a payoff that derives from the remaining forest at the end of the game. In particular, everyone will receive 1$ for every hectare of forest that is left after the end of the last round. The number of hectares of forest left in the end depends on how much was cleared in previous rounds by you and your group members.

It is important to know that the forest benefits go to everyone. Whereas fields are owned and cultivated by one player only, the benefits from the forest at the end go to everyone equally, as nobody owns parts of the forest. By each time you clear, you decrease the forest by one hectare. Each time you do NOT clear, it means that there will be a hectare of forest remaining which will give you and every other player 1$ in the end.

We will now do a test round for the decision making. This is not the real decision that will affect your payoffs, but it is just meant to help you understand how it works.
We will not make the decisions publicly but we will be using the tablet computers.
Therefore, all the decision you will make remain completely anonymous. The other players in your group will not find out how you decided. They will only see after each round how much forest was cleared by the group as a whole.

[Show FIGURE_FOREST and FIGURE_DECISION]

[show TABLE_DECISION].

- Remember to not show to your neighbors what you decide
- Press the **left** side if you wish to stay on your **old field**.
- Press the **right** side if you wish to clear a **new field**.
- Only push the tablet **lightly** and **shortly**. We can also use this **pen**.
- The selected answer will be marked **orange**. You can still **change** your decision.
- When done, swipe **right** and **hand the tablet back** to me/assistant.

[Let Players try test round on tablet]

Let’s now look at some examples (write down numbers of examples on the whiteboard! TABLE_EXAMPLES):

D.4: Game examples presented to participants

EXAMPLE 1:

I will use this table to make examples of how players can decide. If a player decides to stay he/she gets 1$ **[mark].** If a player decides to clear he/she gets 3,50$ **[mark]**

[erase again]

Let's go through an example:

Imagine every player was just sticking to their old field over all rounds. Everyone then gets 1$ in each round, so 1+1+1+1 = 4$ over the 4 rounds.

In addition to that yield, there is the benefit that comes from the forest. Since nothing was cleared, the forest size remains at its initial stock of 28 hectares. For every hectare, each player receives 1 Dollars. That is 28x1 = 28 Dollars.

Remember, that the forest benefits are the same for everyone as everyone is using the same forest.

Your field on the other hand is your own and only generates benefits for yourself.

"As no one cleared, the forest size remains large and there is a lot of forest benefits for the group."

Summing up the benefit from the forest and the yield from the field, everyone receives 4 + 28 = 32 $.

	👤	👤	👤	👤	👤	👤	👤
Round 1	1	1	"				
Round 2	1	1	"				
Round 3	1	1	"				
Round 4	1	1	"				
SUM Yields	4	4	4	4	4	4	4

Remaining Forest = 28

SUM Total	32	32	32	32	32	32	32

EXAMPLE 2:

Let’s go through another example together: Now imagine one out of the seven players decides to clear a new field in all 4 rounds. He/she will then get 3,50$ in each round. Over 4 rounds that is 3,50+3,50+3,50+3,50 = 14$. Then the other 6 players who stay on their old field all the time still only get 1+1+1+1 = 4$ from their fields.

Since he/she cleared in every round and no one else did, the forest decreased by one hectare every round. Which is 4 hectares in total. The remaining forest in the end of the game is then 28 – 4 = 24 hectares. Remember that everyone gets 1$ per hectare of the remaining forest at the end of the game, which is 24$ for everyone.

The 6 players who did not clear do then get their agricultural yield of 4 + the forest benefit of 24 = 28$ in total.

“The one player who decided to clear every round receives 14 from agricultural yields + the same 24 from the remaining forest = 38$ in total. Which is more than what those who did not clear got.

Those other 6 player who did not clear however, just as in the previous example, now get 28 in total which is because the forest size decreased to 24 hectares from the clearing of player one”

Round 1	3.50	1	“				
Round 2	3.50	1	“				
Round 3	3.50	1	“				
Round 4	3.50	1	“				
SUM Yields	14	4	4	4	4	4	4

Remaining Forest = 24

SUM Total	38	28	28	28	28	28	28

EXAMPLE 3:

But now imagine all 7 players decide to always clear a new field in all rounds. They will then get 3,50$ in each round. Over 4 rounds that is 3,50+3,50+3,50+3,50 = 14$ for everyone.

Since everyone cleared in every round, the forest decreased by 7 hectares every round. Which is 7*4 =28 hectares in total. There is therefore no forest remaining at the end of the game.

All players do then get their agricultural yield of 14 + but nothing from the forest, so 14$ in total.

"Note that when ALL PLAYERS decide to always clear, each players' final payoff is much smaller compared to when most of the forest stock is conserved.

If the whole group does not clear, then the group as a whole get the most. If you personally decide to clear, you always get more, but the other players in the group will get less. So when everybody decides to clear a lot, the group benefits from the forest become smaller or vanish and in total everybody is getting less."

	Player 1	Player 2	Player 3	Player 4	Player 5	Player 6	Player 7
Round 1	3.50	3.50	"				
Round 2	3.50	3.50	"				
Round 3	3.50	3.50	"				
Round 4	3.50	3.50	"				
SUM Yields	14	14	14	14	14	14	14

Remaining Forest = 0

SUM Total	14	14	14	14	14	14	14

EXAMPLE 4:

Let's go through another example together: Now imagine one out of the seven players decides to always clear a new field. He/she will then get 3.50$ in each round. Over 4 rounds that is 3,50+3,50+3,50+3,50 = 14$. 4 Players decide to clear twice, for example in round1 and in round 3. They then get 3,50+1+3,50+1 = 9$ from their fields. The remaining 2 players only clear once and therefore get 3,50+1+1+1 = 6,50$ from their fields.

Now the forest has decreased by 4 hectares because of the clearing by the first player. By 4*2 = 8 hectares from clearing by the four players. The last two players who only cleared once caused a decrease of 2*1 = 2 hectares over all round. In total the forest size therefore decreased by 4 + 8 + 2 = 14hecatres. 28 – 14 = 14 hectares of forest that are left in the end. Remember that everyone gets 1$ per hectare of the remaining forest at the end of the game, which is 14$ for everyone.

The one player who decided to clear every round receives 16 from agricultural yields + 14 from the remaining forest = 28$ in total.

The four players who cleared twice each do then get their agricultural yield of 9 + the forest benefit of 14 = 23$ in total.

The two players who cleared once each do then get their agricultural yield of 6,50 + the forest benefit of 14 = 20,50$ in total.

"We can see that those, who clear a new field more often, get higher payoffs in the end. If however everyone decides to clear all the time, the forest benefits will vanish and everyone's final payoffs decrease" **[compare Ex.3].**

	👤	👤	👤	👤	👤	👤	👤
Round 1	3.50	3.50	3.50	3.50	3.50	3.50	3.50
Round 2	3.50	1	1	1	1	1	1
Round 3	3.50	3.50	3.50	3.50	3.50	1	1
Round 4	3.50	1	1	1	1	1	1
SUM Yields	14	9	9	9	9	6.50	6.50

Remaining Forest = 14

SUM Total	28	23	23	23	23	20.50	20.50

Should we have another example?

[spatially separate groups now according to their color, set them in order 1-7]
(e.g., at different sides of a building, behind the car, under different trees etc.)

Then, before we start with the game, we would like to ask you some questions. This is just to test whether you all understood the rules of the game. Remember that during the game you are not allowed to talk to each other. If there is anything unclear please ask us now and we will explain it to you. If there is anything unclear later during the game, you can still ask questions, but you will have to raise your hand and I will come to you to help you in private. So, are there any questions right now?

[ask and answer if so, give another example if requested]

1. **The more hectares of forest the group has cleared, the smaller becomes the forest stock. True or False?**
   [True]
2. **The benefit you receive from the forest is the same for everyone. True or False?**
   [True]
3. **The more often I clear and start a new field, the more money I will get in the end. True or False?**
   [Always True, does not depend on what the other players do]
4. **The more often the other players clear, the less money I get in the end. True or False?**
   [True, others clearing means decreased forest and decreased forest benefits for me]

The outcome does not depend on chance or luck but only on you and your group members' decisions. We promise that we do all the calculation correctly as explained in the game instructions above and will not deceive you at any stage of the game. Also, there is absolutely no possibility of you or any other player cheating or not playing according to the rules given, as you can only decide in every round if you want to clear a new field or not. We will collect your answers using these tablet computers. They are very easy to use and we will show you how it works. You only have to press "clear" or "not clear = stay" in the screen. Here we have a picture of how it looks like [Show TABLE_DECIDE]. If anything is unclear please ask us for help and do not ask the other players. We will calculate and announce to everyone how much of the forest is left after each round. [Show

TABLE_FOREST] (Our calculations are done by the computer and must therefore not be questioned). You will however not learn who cleared or who did not. You only see how much forest is remaining after each round. All the decisions you make will be kept anonymous and no one will find out what the other players did. Even after the end of the game, you are not obliged to tell anyone what you and the other players decided.

Procedure: You and your group members will make your decision in each round. Here [point TABLE_DECISION] you have to press whether you wish to stay on your old field or start a new one in every round. You are not allowed to talk to each other while the others make their decisions and while we calculate the results. After each round we will tell you how many forest parcels are left. After the last round (which is after four rounds) the game ends. You will again be informed about the final forest size, which equals the benefits that everyone receives from the forest. It is however not the end of today's workshop. We will afterwards continue with another game.

Remember, that you must not talk to each other and your decisions will remain anonymous.

ARE THERE ANY QUESTIONS?

Then let us now start with the game decisions. We start with the first round out of four and each of you can please make his/her decision on the tablet. [show TABLE_DECISION].

- Press the **left** side if you wish to stay on your **old field**.
- Press the **right** side if you wish to clear a **new field**.
- Then swipe **right** and **hand the tablet back** to me.
- Remember to not show to your neighbors what you decide

[make decisions]

Thank you. This was the first round. [YY] hectares of forest were cleared. The remaining forest is [XX] hectares [mark on TABLE_FOREST]

D.5: Decision making and debriefing

We continue with the 2nd / 3rd / last (4th) round out of four and each of you please make his/her decision on the tablet. [show TABLE_DECISION].

- Press the **left** side if you wish to stay on your **old field**.
- Press the **right** side if you wish to clear a **new field**.

[make decisions]

[YY] hectares of forest were cleared. The remaining forest is [XX] hectares [mark on TABLE_FOREST]

Thank you for playing. The final remaining forest size is XX hectares. That means everyone will receive XX dollar from the remaining forest in addition to their agricultural yields.

Has anything been unclear during the first game? If so please ask us now. We will now continue with another game that works very similarly.

[REPEAT GAME 3x]:

[Period 1: Baseline]

[Period 2: ---See experimental conditions below [D.6]!---]

[Period 3:] We will now play the same game again. That means we start again with a forest stock of 28 hectares. The decisions, payoffs and results from the first game do in no way influence this second game. However, the rule we introduced in the previous session is abolished again [only if treated, nothing abolished for control condition]. *(Note: This period not relevant for Scarcity Experiment)*

Debriefing and Control Questions

[AFTER ALL HAVE MADE THEIR DECISION]

Then, before we have a break, we also ask you to answer **two short questions** for understanding individually. This is just meant to help us as feedback to check if everyone understood the game.

[go to experimenter individually, try to keep groups separated so that previous players cannot go back to talk to others**]**

1. Imagine you clear a new field once and everyone else in the group clears a new field three times. Who will get a higher payoff in the end? You, the others, or same for everyone?
2. Imagine you clear a new field two times and everyone else in the group never clears a new field. Who will get a higher payoff in the end? You, the others, or same for everyone?

This is now the end of the first part of the workshop. You are now allowed to talk to each other again.

We will have some snacks and drinks and afterwards continue with a short survey-questionnaire that each of you please answer one after another. When that is done, we will do the payments and we are done.

C.6: Experimental conditions

[Control Condition]

Thank you for your decisions. We will now play the game again. Again for 4 rounds and starting with a fresh forest of 28 hectares. All payoffs are as before and you will later get the real money for both the first session that you just played and also for the next sessions that we will be playing now. You will still get 1$ in each round for staying on an old field and 3.50$ for a round when you decide to clear a fresh one. Everyone also still receives 1$ for each tree that remains in the end.

[Treatment 1: Individual Reward]

Thank you for your decisions. We will now play the game again. Again for 4 rounds and starting with a fresh forest of 28 hectares. All payoffs are as before and you will later get the real money for both the first session that you just played and also for the next sessions that we will be playing now. You will still get 1$ in each round for staying on an old field and 3.50$ for a round when you decide to clear a fresh one. Everyone also still receives 1$ for each tree that remains in the end.

There is however an addition to the game that we will have:

In each round that you decide NOT to clear forest, you will receive 1$ as an additional reward

Let's look at an example:

Example:

Imagine a player was clearing a new field in round 1 and in round 3. He/she then gets 3,50+1+3,50+1 = 9$ over the 4 rounds from his/her fields. And he/she gets 1$ Reward in Round 2 and Round 4, when he/she did not clear, which is 2$ that get added to the 9 $

In the end, in addition to that yield and the reward, there is still the benefit that comes from the forest which is left after the end of round 4.

Another player who cleared in all 4 rounds, does not get any reward. He/she gets 3,50+3,50+3,50+3,50 = 14$ from his/her yields + 0$ Reward

A player who never clears however gets 1$ reward for every round, which is 4 in total. He/she gets 1+1+1+1 = 4$ from the yields and 1+1+1+1 = 4$ from reward, total = 8$

Round 1	3.50	3.50	1+1	1+1	1+1	1+1	1+1
Round 2	1+1	3.50	1+1	1+1	1+1	1+1	1+1
Round 3	3.50	3.50	1+1	1+1	1+1	1+1	1+1
Round 4	1+1	3.50	1+1	1+1	1+1	1+1	1+1
SUM Yields	9+2	14+0	4+4	4+4	4+4	4+4	4+4

Remaining Forest = 22

SUM Total	31+2	36	26+4	26+4	26+4	26+4	26+4

[Treatment 2: Collective Reward]

Thank you for your decisions. We will now play the game again. Again for 4 rounds and starting with a fresh forest of 28 hectares. All payoffs are as before and you will later get the real money for both the first session that you just played and also for the next sessions that we will be playing now. You will still get 1$ in each round for staying on an old field and 3.50$ for a round when you decide to clear a fresh one. Everyone also still receives 1$ for each tree that remains in the end.

There is however an addition to the game that we will have:

As a group, you will receive an additional reward in each round when there are at least 14 hectares of forest left. If 14 or more hectares of forest are left, everyone will receive 0,50$ reward per round additionally to what he/she gets from the fields and from the forest.

Let's look at an example:

Example:

Imagine again the following situation [See table]. Now, if there is at least 14 hectares of forest left, after round 1, everyone receives 0,5$ additionally. It does not depend on who cleared how much as long as the whole forest remains at 14 or more hectares.

If the remaining forest in a round is less than 14, then no one would get the extra reward.

	🚹	🚹	🚹	🚹	🚹	🚹	🚹
Round 1	3.5+0.5	3.5+0.5	3.5+0.5	3.5+0.5	3.5+0.5	3.5+0.5	3.5+0.5
Round 2	3.5+0.5	3.5+0.5	3.5+0.5	3.5+0.5	1+0.5	1+0.5	1+0.5
Round 3	3.5	3.5	3.5	3.5	3.5	1	1
Round 4	3.5	1	1	1	1	1	1
SUM Yields	14+1	9+1	9+1	9+1	9+1	6,50+1	6,50+1

Remaining Forest = 11

SUM total	25+1	20+1	20+1	20+1	20+1	17.5+1	17.5+1

[Treatment 3: Individual Fee]

Thank you for your decisions. We will now play the game again. Again for 4 rounds and starting with a fresh forest of 28 hectares. All payoffs are as before and you will later get the real money for both the first session that you just played and also for the next sessions that we will be playing now. You will still get 1$ in each round for staying on an old field and 3.50$ for a round when you decide to clear a fresh one. Everyone also still receives 1$ for each tree that remains in the end.

There is however an addition to the game that we will have:

In each round that you decide to clear forest, you have to pay 1$ for doing so.

Let's look at an example:

Example:

Imagine a player was clearing a new field in round 1 and in round 3. He/she then gets 3.50 +1+3.50 +1 = 9$ over the 4 rounds from his/her fields. But he/she has to pay 1$ for clearing in round 1 and in round 3. So in total he/she gets 9 – 2$ = 7$

In the end, in addition to that yield and the reward, there is still the benefit that comes from the forest which is left after the end of round 4.

Another player who cleared in all 4 rounds, has to pay 1$ in every round for doing so. He/she gets 3.50 +3.50+3.50 +3.50 = 14$ from his/her yields - 4$ payment for clearing = 10$

A player who never clears however does not have to pay anything. He/she gets 1+1+1+1 = 4$ from the yields

Round 1	3.50-1	3.50-1	1	1	1	1	1
Round 2	1	3.50-1	1	1	1	1	1
Round 3	3.50-1	3.50-1	1	1	1	1	1
Round 4	1	3.50-1	1	1	1	1	1
SUM Yields	9-2	14-4	4	4	4	4	4

Remaining Forest = 22

SUM Total	31-2	36-4	26	26	26	26	26

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