Do Payments for Environmental Services Affect Forest Access and Social Preferences in the Long Run? Experimental Evidence from Uganda

Tobias Vorlaufer, Joost de Laat, Stefanie Engel

Abstract: Conservation policies and programs may trigger unintended, potentially irreversible, changes that were initially not anticipated. Concerns have been raised that the introduction of payments for environmental services (PES) fosters the privatization of natural ecosystems to the detriment of marginalized groups. We assess the long-term impacts of PES on sharing of access to natural resources, associated norms, and social preferences. The studied PES program was implemented as a randomized control trial in western Uganda. Using survey and experimental data collected six years after the last payments were made, we find that the PES program did not lead to a lasting shift in resource sharing practices but did induce stronger social norms for resource sharing. Moreover, landowners in former PES villages exhibit more egalitarian social preferences than landowners in control villages. These results highlight that despite introducing unequal conservation benefits to communities, long-lasting negative spillovers of PES could be avoided.

JEL Codes: C93, O13, Q57, Q23

Keywords: payment for ecosystem services, PES, social impact, social norms, forest resources, sharing, RCT, impact evaluation

CONSERVATION POLICIES AND PROGRAMS—like any policy or intervention that aims to encourage or discourage specific behavior or practices—may trigger unintended, potentially irreversible, changes that were initially not anticipated. For example, they may

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Dataverse data: https://doi.org/10.7910/DVN/OJSRMF

Received July 7, 2021; Accepted June 13, 2022; Published online January 9, 2023.

Journal of the Association of Environmental and Resource Economists, volume 10, number 2, March 2023.

https://doi.org/10.1086/721440

affect intrinsic motivations to engage in pro-environmental behavior (Rode et al. 2015; Ezzine-de-Blas et al. 2019), affect individual beliefs about the behavior of others, the regulator, or oneself, and subsequent behavior (Koessler and Engel 2021) or affect behavior that has not been targeted by the intervention itself, so-called behavioral spillovers (Truelove et al. 2014; Nilsson et al. 2017). Conservation policies and programs may also impact community dynamics—how individuals interact and share resources, particularly if interventions introduce an unequal distribution of costs and benefits, and specific actors perceive this as being unfairly treated (e.g., To et al. 2012). Interventions can also leave lasting impacts if their introduction permanently changes social preferences or establishes new social norms, for example, around resource sharing.

In this paper, we report findings from a follow-up study to an experimental (randomized control trial , RCT) evaluation of a payments for environmental services (PES) program implemented across 121 communities in Uganda between 2011 and 2013, which compensated local landowners for forest conservation and afforestation activities. In the original study by Jayachandran et al. (2017), the PES program was found to reduce deforestation by half and led forest owners to restrict access to their forest for other community members while payments were in place. In the follow-up, we study whether the PES had unintended long-run impacts on resource sharing from private forests six years after payments were terminated. We focus on two mechanisms that potentially support a permanent shift in behavior: social norms and (endogenous) social preferences. While a few studies have analyzed the long-term environmental effectiveness of terminated PES (Pagiola et al. 2016; World Bank 2018; Calle 2020; Pfaff and Costedoat 2021), to our knowledge we provide the first rigorous, empirical study investigating to what extent PES affect local communities' resource sharing practices, associated social norms, and social preferences in the long term.

PES have become popular alternatives to conventional command-and-control and integrated development and conservation programs (Engel 2016; Salzman et al. 2018) and are now widely applied in both the Global North and the Global South. Particularly in the Global South, the attractiveness of PES is at least partly driven by their promise of creating win-win solutions that reconcile environmental conservation and poverty alleviation goals (Pagiola et al. 2002; Engel et al. 2008; Wunder 2008; Zilberman et al. 2008). In theory, PES provide net benefits for participating land users when payments exceed the opportunity costs of switching away from status quo land

(stefanie.engel@uos.de). Funding for this research was provided by the Alexander von Humboldt Foundation in the framework of the Alexander von Humboldt Professorship endowed by the German Federal Ministry of Education and Research. The funders had no role in study design, data collection and analysis, decision to publish, or preparation of the manuscript. We thank all participants and communities for their support and hospitality. Special thanks to go to Mathew Kato Ahimbisibwe, Gloria Ayesiga, Lotte van der Haar, Angélica López Ardila, and Christine Nabulumba for their support during and after the data collection. We are grateful for feedback from Bosco Lliso and from two anonymous reviewers on an earlier version of the manuscript.

uses that the program aims to discourage. PES may also increase income security and contribute to income diversification for participating land users. A growing body of meta-analyses and reviews provide a mixed picture of the environmental impacts of PES in general (Pattanayak et al. 2010; Samii et al. 2014; Snilsveit et al. 2019; Wunder et al. 2020). Evidence on socioeconomic and livelihood impacts of PES is more limited (see Blundo-Canto et al. [2018] for a review; Snilsveit et al. [2019] for a meta-analysis).

Concerns have been raised that the introduction of PES fosters the privatization of forests or other natural ecosystems to the detriment of marginalized groups (Grieg-Gran et al. 2005; Pagiola et al. 2005). Wunder (2008, 279) goes as far as to state that "poverty concerns in selected cases should rather be with poor non-participants, especially landless" instead of poor participants who are expected to benefit from PES. For example, there may be direct effects onto other users of the land covered by PES contracts (Pagiola et al. 2005). Studies predominantly focus on the livelihood implications for land users who receive PES (or who are in principle eligible for PES) (Snilsveit et al. 2019). Yet, drawing on data from 20 countries across Latin America, Africa, and Asia, Jagger et al. (2014) find that only in 24% of state-owned, 51% of community-owned, and 59% of privately owned forests the landowners are the exclusive land users. Rather, in many settings in the Global South, neighboring households also derive benefits from a given land parcel. This concerns especially land uses such as forests and other natural ecosystems that substantially contribute to subsistence and income-generating activities by providing a wide array of products such as firewood, food, fodder, and construction material (Angelsen et al. 2014).

Clearly defined property rights at the individual level are often considered a prerequisite for individual incentive payments (Engel 2016). At a minimum, it is required that PES participants have the right to exclude others from their land, which does not necessarily require formal land titles. PES participants may therefore exclude others from using their land, such as landless and land-poor households who regularly accessed the land for livelihood activities (Pagiola et al. 2005; Lliso et al. 2021). In a study of PES programs in Latin America, Lliso et al. (2021) found that the programs often score low on recognition of marginalized actors. In the worst case, the unequal distribution of benefits to PES participants and costs to nonrecipients could affect how community members interact with each other, that is, increase conflict over resources (Grieg-Gran et al. 2005; Wunder 2008).

Such concerns that PES may have adverse impacts for households that either cannot or do not enroll have been predominantly raised at a conceptual level (Grieg-Gran et al. 2005; Pagiola et al. 2005; Wunder 2008; Zilberman et al. 2008). Qualitative case studies find that conflicts between community members (Corbera et al. 2020) and between state authorities and local (indigenous) communities (Hoang et al. 2019) over land ownership and use rights emerged or intensified after the introduction of PES. Yet, there is to our knowledge only one quantitative study that has systematically assessed impacts of PES on community dynamics. Alix-Garcia et al. (2018) found that communal PES in

Mexico have strengthened social capital and had no adverse impact on unpaid social work and trust within PES communities.

Our study contributes to this literature by assessing the long-term impacts of a successful PES scheme in western Uganda on local resource sharing practices and norms, as well as prosocial preferences, six years after the PES payments ended. In contrast to Alix-Garcia et al. (2018), we focus on a PES scheme that targeted privately owned forests. The PES program was not continued because it relied on financing as part of a one-time grant from the Global Environment Facility to the government of Uganda. Since financing of PES is often challenging, both public and private PES programs share the risks of providing only temporary incentives for land users.

We study the impact of PES with respect to three main outcomes. First, we use incentivized experiments to study the impact on social preferences, including spitefulness and egalitarian preferences. Second, we measure personal and social norms, that is, whether use restrictions and a commodification of forest resources are considered appropriate. We used a coordination experiment at the village level to incentivize the elicitation of social norms (Krupka and Weber 2013). And third, we elicit resource sharing with regard to allowing other community members to access private forests and collect resources such as firewood and poles. For this, we rely on self-reported information from both forest owners and non–forest owners. Since the PES were randomly assigned at the community level, we are able to identify causal effects with relatively few assumptions. By using both survey-based and experimental methods, we go beyond purely self-reported behavior or attitudes.

Overall, we find no evidence for adverse social impacts of PES on communities six years after the payments ended. Community members in treatment villages are not more restricted than those in control villages when it comes to accessing privately owned forests of other community members to collect resources such as poles and firewood. Respondents from former PES villages also do not consider forest use restrictions as personally more appropriate than their counterparts in control villages. However, we find some evidence that social resource sharing norms differ between PES treatment and control villages. Contrary to expectations, respondents from PES villages state that they perceive social norms to be more supportive of sharing forest access within communities. In line with this, we do not find evidence that PES have exacerbated conflicts between PES recipients and nonrecipients. On the contrary, our results suggest that egalitarian preferences are more prevalent in former PES villages. Overall, our results highlight that despite introducing unequal conservation benefits to communities, and a subsequent restriction of other community members' access to private forests owned by PES participants (Jayachandran et al. 2017), communities averted long-lasting negative spillovers of PES.

1. CONCEPTUAL FRAMEWORK AND HYPOTHESES

PES programs like the one we study in this paper may have long-run impacts that go beyond the duration of the payments and the initially targeted behavior. In this section,

we introduce a conceptual framework connecting temporary economic incentives to long-term changes in targeted and nontargeted behaviors. Based on this, we introduce three hypotheses that we can test with our data.

Our conceptual framework is shown in figure 1 and illustrates how temporary economic incentives (in our case PES) can affect those conservation behaviors directly targeted by the PES scheme (Action A) as well as behavior not initially targeted (in our case resource sharing with other community members, Action B in fig. 1). It expands the conceptual framework of motivational crowding by Bowles and Polanía-Reyes (2012). As they highlight, incentives may impact behaviors by changing economic pay-offs and by changing social preferences. They distinguish between two different mechanisms: incentives may (1) change the salience of social preferences in a given situation (state-dependent preferences) or (2) affect how social preferences are updated in the long run (endogenous preferences). This relationship is illustrated within the gray area of figure 1. If incentives crowd-in (strengthen) social preferences, both are considered complements, while in the other case (crowding-out) incentives and social preferences are seen as substitutes. The body of literature on crowding effects by PES has grown rapidly over the past few years (e.g., Kaczan et al. 2017; Andersson et al. 2018; Chervier et al. 2019; Kaczan et al. 2019; Moros et al. 2019; just to mention a few). The evidence from this large body of economic lab-in-the-field experiments and case studies is mixed. While some studies find crowding-out effects, other studies do not find these or find positive impacts on intrinsic motivations instead (so-called crowding-in) (see Rode et al. [2015], Akers and Yasué [2019], and Ezzine-de-Blas et al. [2019] for reviews).

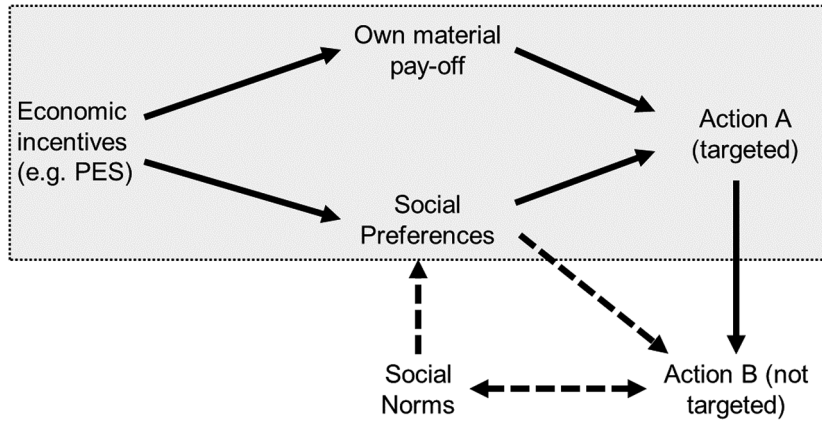

Figure 1. Conceptual framework linking economic incentives with unintended behavioral consequences. Adapted from Bowles and Polanía-Reyes (2012), who focus on the part framed in gray.

This paper focuses on a subsequent Action B that is linked to Action A (i.e., behavioral spillover). In our study, the subsequent Action B is resource sharing with other community members, while the behavior initially targeted by the PES scheme (Action A) is forest conservation. If changes in endogenous social preferences are permanent, resource sharing practices (Action B) can be potentially sustained after incentives cease and forest conservation behavior (Action A) reverts to pre-intervention levels. Reciprocity has been shown to be a powerful driver of prosocial behavior and cooperation in humans (Fehr et al. 2002). Given the unequal distribution of benefits and costs across PES participants and other community members, spiteful and/or egalitarian preferences are likely candidates of social preferences that may become more salient following the introduction of PES. Several authors have raised the point that PES may increase social tensions and grievances and could undermine cooperation at the community level (Grieg-Gran et al. 2005; Wunder 2008). For example, To et al. (2012) found that households that were excluded from participating in a PES scheme in Vietnam exhibited spiteful behavior in the form of damaging coffee plantations of PES recipients. Further, growing inequalities can also strengthen the salience of egalitarian preferences. This process may be driven by nonrecipients or even PES recipients, who judge the distributional outcomes of PES as unfair. For example, potential PES recipients in Colombia were willing to forgo parts of their monetary payments in return for more equitable PES outcomes at the community level (Lliso et al. 2020). Note that these two dynamics are not necessarily mutually exclusive (e.g., PES recipients may become more egalitarian, while the salience of spiteful preferences among nonrecipients may increase). Finally, if social preferences are not only state dependent but also endogenous, they may permanently change (and thus not revert once the economic incentives are removed) (Bowles and Polanía-Reyes 2012).

In addition, changes in practices of resource sharing may also shift the social norms that prescribe which behavior is perceived as personally and socially appropriate by the majority of people. A large body of research on social norms has shown that a shift in collective behavior can ultimately affect personal normative beliefs and normative expectations (i.e., what one considers appropriate to do in a given situation and what one believes other people consider appropriate) (Bicchieri 2017). Social norms can induce abrupt changes in collective behavior, as outlined by Nyborg (2018). These newly created personal and social norms may persist even after the initial reason for the behavioral change vanishes (in our case after PES payments ceased). In addition, social norms can help to sustain the change in social preferences even if economic incentives have vanished (indicated by the direct link between social norms and social preferences in fig. 1).

Since the benefits of the Uganda PES program were unequally distributed across community members, the program may have affected social preferences and/or social norms. Jayachandran et al. (2017) found short-run impacts over the course of the Uganda PES program not only on conservation behaviors but also on resource sharing. Forest owners in treatment villages were more likely to restrict access to forests for

non–forest owners and to increase patrolling to enforce these restrictions compared to forest owners in control villages. These changes were observed even though the PES scheme permitted firewood collection and the cutting of smaller trees. Anecdotal evidence that we collected in the form of focus group discussions prior to the follow-up survey further suggests that conflicts between neighboring landowners had emerged. Respondents accused PES recipients of attracting monkeys by providing a habitat in the conserved forests. Monkeys are the major source of human-wildlife conflicts in the area as they cause crop damage (such as to maize, sugarcane, fruits). More generally, non–forest owners and nonparticipating forest owners were likely to bear some externalities of the PES scheme.

Against this background, we first formulate a preference and a social norm hypothesis:

Hypothesis 1: Inhabitants from former PES treatment villages have different social preferences than inhabitants of non-PES villages. In particular, PES have strengthened spiteful and/or egalitarian preferences.

Hypothesis 2: Forest use restrictions are considered personally and socially more appropriate (personal and social norm, respectively) in former PES treatment villages compared to non-PES villages.

Finally, changes in social preferences and/or the social norms associated with resource sharing, may sustain restrictive resource sharing even after economic incentives are removed. We formulate our third and final hypothesis:

Hypothesis 3: Forest use restrictions reported by forest owners as well as non–forest owners are stronger in former PES villages compared to non-PES villages.

2. MATERIAL AND METHODS

2.1. Survey Instruments

2.1.1. Social Preferences

Based on incentivized experiments, respondents were classified as either generous, egalitarian, or spiteful social preference types. We adapted the elicitation task from Fehr et al. (2008) with a slightly modified payoff structure. We deem this elicitation method to be a suitable compromise between task complexity—which has to be relatively low due to the diverse sample with respect to age and education—on the one hand, and detail, on the other hand. It not only measures the degree of a specific behavior (e.g., altruism or spitefulness) but also categorizes respondents into different social preferences types. Respondents were confronted with three binary choices of resource allocations between themselves and an anonymous partner from the same village (see table 1). These three decisions allow us to categorize each respondent into

Table 1. Classification of Social Preference Types

	Envy Game		Prosocial Game		Sharing Game	
Payoff partner (in UGX)	4,000	6,000	4,000	2,000	4,000	2,000
Payoff player (in UGX)	4,000	4,000	4,000	4,000	4,000	6,000
	Type Classification					
Spiteful: minimizes partner's payoff	x			x		x
Strong egalitarian: always chooses the equitable allocation	x		x		x	
Weak egalitarian: chooses the equitable allocation if not costly.	x		x			x
Strong generous: maximizes partner's payoff		x	x		x	
Weak generous: maximizes partner's payoff if not costly		x	x			x

one of five social preference types (see Supplementary Information, sec. A.3 (SI-A.3) for more details; Supplementary Information [SI] is available online) If the respondent's decisions do not follow any of these patterns, we categorize her as "Unclassified."

For hypotheses testing, we focus on three binary indicators whether a respondent classifies as generous (weak or strong), egalitarian (weak or strong), or spiteful. After finalizing all interviews in one village, respondents were randomly allocated to the role of the recipient or decision maker and matched with a fellow villager. Payments were transferred with mobile money within a few weeks after the interview. The decisions were made with the help of color-coded decision cards and explanatory posters. The order of the games and colors associated with a specific payoff were randomly altered between respondents. Respondents took their decision in private by putting colored decision cards in envelopes (while not being observed by enumerators) to reduce potential experimenter demand effects (Vorlaufer 2019). Only after the enumerator left the venue of the interview were the envelopes opened and the decisions entered. Within the survey, social preferences were elicited first so that questions regarding forest use and PES would not affect responses. Social and personal norms as well as forest use restrictions were elicited at a later point. The survey structure was the same for respondents from treatment and control villages.

2.1.2. Personal and Social Norms

Personal and social norms regarding use restrictions by forest owners were elicited through incentivized experiments. For four different scenarios, respondents rated behavior as either appropriate or inappropriate. These scenarios described a situation where a forest owner (*a*) prohibited others to collect firewood, (*b*) prohibited others to collect poles, (*c*) asked for money in return for allowing somebody else to collect poles, and

(*d*) asked for money in return for allowing somebody else to collect firewood. Following the personal norm assessment, respondents were asked to guess what most respondents from the same village indicated about the social appropriateness of the described behavior. This social norm elicitation was incentivized through a coordination game following Krupka and Weber (2013). One of the four scenarios was randomly picked and each respondent received 3,000 Ugandan schilling (UGX ≈ 2.43 USD purchasing power parity [PPP]) if they gave the most common answer from the village. The coordination experiment was explained with the help of two prior scenarios that described two different behaviors after finding somebody's wallet. For both personal and social norms, a simple index was constructed, ranging from zero to one indicating the share of scenarios where the restrictive behavior was rated as socially appropriate (see SI-A.2 for a detailed description of the experiment and index construction).

2.1.3. Forest Use Restrictions

Information regarding use restrictions in private forests was collected through surveys of forest owners and non–forest owners. We focus here on two particular forest resources: (*a*) firewood that is used by the vast majority of households in the area as cooking fuel and (*b*) poles that are one of the main materials used for the construction of houses and other structures. Survey questions focused on whether forest owners generally allow others to collect firewood and poles on their land, whether they explicitly prohibited this, whether they asked for money in return and whether they have undertaken actions to prevent other households from doing so (within the last 12 months). Corresponding questions were also asked of non–forest owners (see SI-A.1). Based on these statements, two indices ranging from zero to one were constructed for forest owners and non–forest owners, respectively (with 0 indicating no restrictions and 1 indicating severe use restrictions).

2.2. PES Intervention

The PES program was implemented between 2011 and 2013 in western Uganda (Hoima and Kibaale district), an area where deforestation is predominantly driven by smallholders. Expansion of agricultural areas as well as timber and charcoal production are the main drivers. Land tenure in the research area is predominantly customary. Formally, the land is government owned while de facto individual households or families are recognized within communities as landowners. Only a small minority of households have formal land titles. In the initial RCT, 121 villages were randomly assigned to a treatment or control group. In treatment villages, landowners with primary forests could voluntarily enroll in the PES program. In total, 32% of potential recipients enrolled in the program. Payments were conditional on forest conservation, and additional payments were provided for tree planting within degraded forests. Enrollees received 36 USD per hectare (ha) per year for conserving natural forests. On average, enrolled forest owners received 113 USD (over two years). The program was found to

roughly halve deforestation rates over the project period, from 9.1% in control villages to 4.2% in treatment villages (Jayachandran et al. 2017).

2.3. Data Collection and Sampling Strategy

In October/November 2019, we revisited the villages of the RCT to conduct interviews with households that owned natural forests in 2011 (i.e., with either formal or informal ownership) and were covered in the base/endline study of Jayachandran et al. (2017). Ugandan and international institutional review board approvals were attained prior to the data collection. Overall, 753 households were interviewed from 58 treatment villages ($n = 363$) and 61 control villages ($n = 390$). In two treatment villages of the initial RCT, the respective village leaders did not grant permission for conducting interviews due to land tenure concerns. Here, no interviews could be conducted.[1]

Due to budget constraints, we could not collect data from households that did not own any forest in 2011. It would certainly be interesting to have survey information from noneligible households in treatment villages (i.e., landless or landowners without forest land when PES were introduced) and corresponding households from control villages. However, a substantial share of former forest owners cleared all their private forests until 2019 (see sec. 2.4 for more details). We thus also collected information on forest resource sharing practices from current non–forest owners.

Our sample includes (a random sample of) households from treatment villages, including both households that enrolled and those that did not enroll in PES. Since our budget did not allow us to reinterview the whole baseline sample, we randomly sampled households from the baseline (after excluding nonexisting households based on a prior household listing with village leaders). To ensure that households from all villages would be reinterviewed, we first randomly sampled four households from each village. Additionally, we wanted to ensure sufficient observations of households that still own forest land in 2019. We, therefore, oversampled households that owned one hectare or more forest land at baseline (assuming that households with more forest land in the past are more likely to still own forest land; see SI-B.1). In total, 295 households with less than one hectare and 484 households with one or more hectares were sampled. In all following analyses, we correct for the unequal sampling probabilities. The probability weights are derived from Monte Carlo simulations with 100,000 runs (see SI-B.2 for more details).

Overall, 146 sampled households were not interviewed, either because they could not be tracked or refused to be interviewed. In such cases, enumerators replaced the households with another randomly selected household from the same village. In total,

1. More specifically, the provided reasons for not allowing us to collect data (i.e., the fear of land grabbing by outsiders) does not originate from within the village (e.g., social relations or dynamics between community members). We therefore consider the risk that the exclusion of these two villages biases our results to be minimal. Nonetheless, it remains a possibility. In SI, sec. G, more details on the two cases are reported, and we provide lower bounds for the treatment effect estimate, following the procedure outlined by Horowitz and Manski (2000).

120 replacements were made. Detailed attrition analyses can be found in SI-E. Our final sample includes 753 households (390 from control villages, 363 from treatment villages), of which 128 households enrolled in the PES program.

2.4. Sample Characteristics

In most cases (85.8%, $n = 649$) the respective head of the household was interviewed (unless the household head was temporarily not present or unable to be interviewed). Respondents were predominantly male (75%) and on average 51.6 years old (SD = 14.7). Each household owns on average 7.7 ha (SD = 14.0) of land, of which 0.5 ha (SD = 1.2) is covered with forest. Thus, on average only 8.3% of private land is covered with forest. Slightly more than half of the households (51.8%) own forest land. The detailed sample characteristics by treatment status are provided in the SI-C.

2.5. Identification Strategy

To identify the causal effects of terminated PES on our outcomes of interest, we harness the random assignment of PES at the village level. While we cannot show that treatment and control groups were identical with respect to our outcomes, the random assignment should result in a comparable treatment and control group prior to the intervention. As shown by Jayachandran et al. (2017), differences in observable baseline characteristics between the two groups were not statistically significant. Additionally, we provide evidence in SI-C that our sampled treatment and control sample were similar in observable characteristics at baseline. One exception is food expenditure in the last 30 days, which was slightly higher in control villages. We do not consider this difference substantial as nonfood expenditure, as well as an asset ownership index as measure of household wealth, is not systematically different between groups. We, therefore, see no reason to assume that control villages do not form a valid counterfactual for villages that received PES. For testing the above outlined hypotheses, we rely on regression models. This allows us to (*a*) cluster standard errors at the village level due to the introduction of PES at this level and (*b*) include enumerator fixed effects. Additionally, we include controls to account for the stratified assignment of the treatment, that is, subcounty fixed effects and four village-level variables: number of private forest owners, average weekly household earnings per capita, distance to nearest major road, and average self-reported land size. As robustness analysis, we report additional specifications without enumerator fixed effects and control for unbalanced baseline covariates (see SI-D). The data set, analysis scripts, and survey material are accessible on the Open Science Framework: https://doi.org/10.17605/OSF.IO/R7K64.

3. RESULTS

3.1. Social Preferences

First, we find that former PES treatment villages have a higher share of egalitarian respondents six years later than non-PES villages. The distribution of social preference

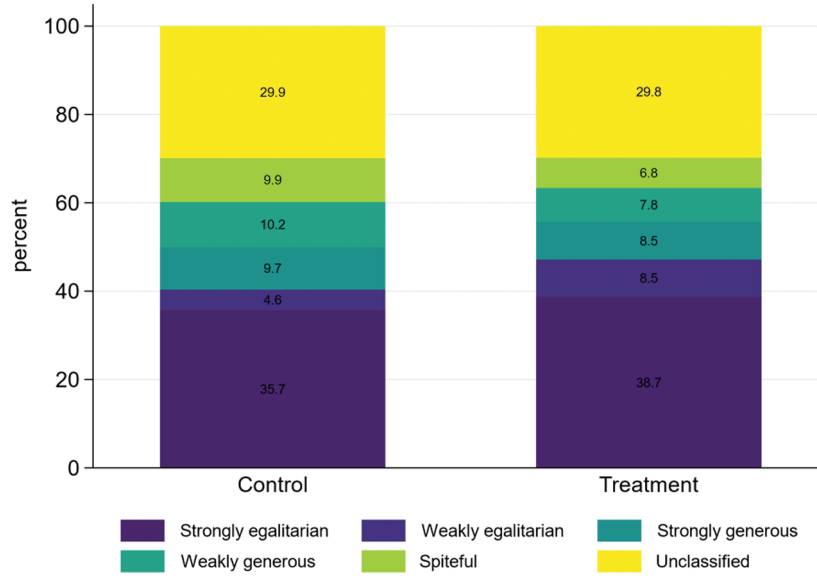

Figure 2. Distribution of social preference types by treatment

types by treatment status is shown in figure 2. Overall, egalitarians are the most common social preference type in our sample (weak: 6.5%, strong: 37.2%); 29.8% are not classified as any of the five types, while generous (weak: 9.1%, strong: 9%) and spiteful (8.4%) types are less common. However, we do observe different patterns between treatment and control villages. The share of egalitarian social preference types is larger in treatment than control villages, while the share of generous and spiteful types is relatively smaller.

Probit regression models (table 2, models 1–3) indicate that respondents in treatment villages are 7.2 percentage points more likely to classify as egalitarian and 3.2 percentage points less likely to act spitefully (the latter effect is weakly significant at the 0.1 level).[2]

3.2. Personal and Social Norms

Second, we find that while personal norms regarding forest use restrictions do not differ between former PES and non-PES villages, social norms in PES treated villages prescribe less forest use restrictions six years after the end of the program. Personal and social norms regarding prohibiting as well as asking for money in return for allowing to collect firewood and poles, respectively, are illustrated in figure 3. Respondents

2. The corresponding multinomial logit model yields similar results and is reported in SI-D.

Table 2. Regression Results

	Social Preferences			Norms		Forest Restrictions	
	Spiteful (1)	Generous (2)	Egalitarian (3)	Personal (4)	Social (5)	Forest Owners (6)	Non–Forest Owners (7)
Treatment	−.032*	−.030	.072**	.040	−.087**	−.035	−.020
	[−.066, .002]	[−.086, .026]	[.001, .143]	[−.012, .091]	[−.157, −.016]	[−.086, .015]	[−.060, .020]
Constant				.534***	.266**	.117	.558***
				[.350, .718]	[.030, .501]	[−.070, .304]	[.389, .726]
N	696	748	752	745	741	399	362
Clusters	119	119	119	119	119	106	109
Pseudo R^2	.100	.067	.105	.093	.219	.185	.723
F-statistic				98.603	63.519	18.550	5.827
p				.000	.000	.000	.000
χ^2 statistic	60.756	409.079	185.173				
p	.001	.000	.000				

Note. All models include enumerator fixed effects and stratification controls. Standard errors are clustered at the village level. Models 1–3 are Probit models reported as marginal effects. Models 4–7 are Tobit regressions censored between 0 and 1; 95% confidence intervals in brackets.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

consider restrictions for poles as more appropriate than restrictions for firewood, both personally and socially. At the same time, asking for money in return for collecting resources is considered more appropriate than a complete refusal (this difference is more pronounced for poles). Overall, respondents consider restrictions as more personally than socially appropriate. This indicates a mismatch of beliefs between what individuals consider appropriate themselves and what they believe others in their community consider appropriate. We observe few differences between treatment and control villages. Most notably, more respondents in control villages believe that asking for money in return for firewood is considered appropriate by fellow villagers than respondents in treated villages do (35.9% vs. 28.1%).

Tobit regression models with a simple index of the relative share of behaviors that are considered personally appropriate as dependent variable find no systematic differences between treatment and control villages (see table 2, model 4). However, we observe that the social norms regarding forest use restrictions are significantly different between PES and control villages (table 2, model 5). Fewer respondents in

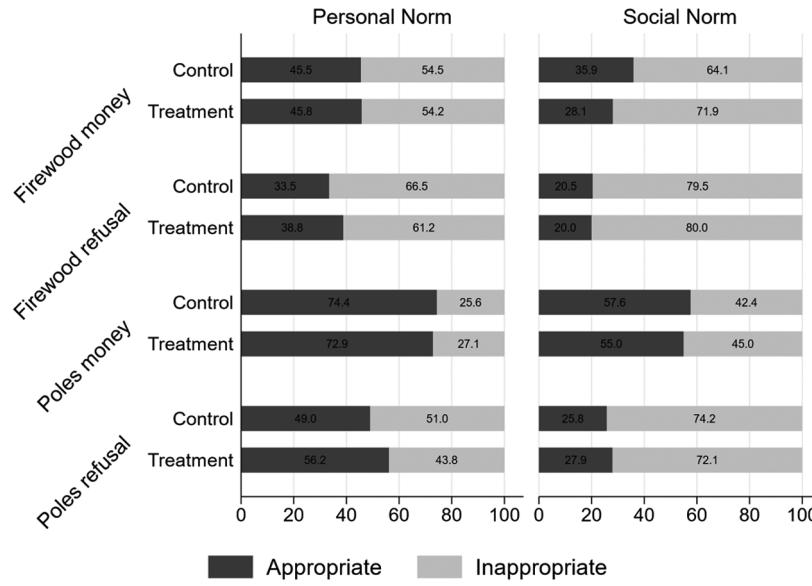

Figure 3. Personal and social norms concerning forest use restrictions by treatment. Refusal refers to prohibiting access to the resource, money refers to requiring payments for allowing access. Color version available as an online enhancement.

treated than control villages believe that their fellow villagers consider restrictions appropriate.[3]

3.3. Forest Resource Restrictions

Finally, when it comes to reporting of actual behaviors, forest owners and non–forest owners report similar forest use restrictions in former PES and non-PES villages. Table 3 summarizes forest use restrictions and sharing reported by forest owners and non–forest owners by treatment status. The vast majority of forest owners (72.1%) reported that other households collected firewood and/or poles from their land in the past 12 months. Yet, half of the forest owners (51.4%) report that they generally do not allow others to do so. Also, half of the forest owners (45.6%) explicitly prohibited someone to collect resources on their land. Only a minority (6.9%) of forest owners

3. A vast body of literature has identified economic inequality and distance to markets as relevant predictors of cooperation in forest resource management and forest outcomes. We refer interested readers to SI, sec. H. There, we report further data analysis testing to what extent forest resource sharing and associated norms are correlated with economic inequality and distance to markets and whether PES treatment effects are different for remote and unequal communities.

asked for money in return for granting permission, while it is more common for forest owners (29.8%) to take preemptive actions to restrict access (i.e., putting up signs, patrolling, fencing). Differences between treatment and control villages are minor.

This picture is also mirrored by the responses of non–forest owners. Half of non–forest owners state that no forest owners allow them to collect firewood (53.8%), and this is also indicated by the majority of non–forest owners about poles (70.3%). At the same time, only a minority of non–forest owners were explicitly prohibited to access forests (14.9%). Overall, 7.4% of non–forest owners collected forest resources without the landowners' consent, while 4.2% of non–forest owners paid forest owners for accessing forests. Again, responses from treatment and control villages are fairly similar. More forest owners (21%) than non–forest owners (2.9%) reported conflicts over forest resources with minor differences between treatment and control villages.

To test whether forest use restrictions are different between treatment and control, we construct two indices of forest use restrictions—one for forest owners and one for non–forest owners. The indices are based on four and three statements of forest and non–forest owners, respectively (see table 3). The indices take values between zero and one, whereby a larger value indicates more forest use restrictions. For this, we sum up the number of approvals to the statements shown in table 3 (see SI-A.1 for more details). In treatment villages, we observe slightly fewer restrictions than in control villages (forest owners: 0.24 [SD = 0.22] for treatment vs. 0.26 [SD = 0.23] for control villages; non–forest owners: 0.53 [SD = 0.22] vs. 0.55 [SD = 0.22]). Tobit regression models (see table 2, models 6–7) indicate that these differences are not statistically significant.

4. DISCUSSION AND CONCLUSION

Concerns have been raised that the introduction of PES fosters the privatization of forests or other natural ecosystems to the detriment of marginalized groups (Grieg-Gran et al. 2005; Pagiola et al. 2005). We do not find support for this hypothesis within a context where land ownership has been de facto private with considerable resource sharing with neighbors. We set out to assess the long-term impacts of PES on forest resource sharing and social preferences in western Uganda—to our knowledge the first experimental study to focus on the long-term impacts of PES on entire communities—to better understand the broader implications of forest conservation incentives at the community level.

While the program was running, Jayachandran et al. (2017) found that forest owners in treatment villages were more likely to restrict forest access for other community members. Focusing on the long-term impacts six years after PES were terminated, this paper does not find evidence that forest access restrictions prevailed. In line with this, we also find no evidence that respondents in former PES villages perceive forest use restrictions as personally more appropriate in their village six years after the PES program ended. But, contrary to initial expectations, we find that respondents in former PES villages believe

Table 3. Forest Use Restriction Stated by Forest Owners and Non–Forest Owners

Statement	Control %	Treatment %	Total %	Index %
	Forest Owners			
Other than members of your household, do you allow people to gather firewood or poles from your forest land?				x (reversed)
No	51.755	51.055	51.431	
Yes	48.245	48.945	48.569	
	χ^2 (1, N = 396) = .145, p = .704			
In the past 12 months, have you ever explicitly denied people to collect firewood or poles from your forest land?				x
No	51.971	57.241	54.420	
Yes	48.029	42.759	45.580	
	χ^2 (1, N = 397) = .594, p = .441			
In the past 12 months, have you ever asked people for money in return for collecting firewood or poles from your forest land?				x
No	93.724	92.451	93.132	
Yes	6.276	7.549	6.868	
	χ^2 (1, N = 397) = .364, p = .546			
In the past 12 months, have you ever taken actions to prevent people from collecting firewood and/or poles from your forest land?				x
No	70.731	69.627	70.221	
Yes	29.269	30.373	29.779	
	χ^2 (1, N = 395) = .228, p = .633			

In the past 12 months, have people from other households collected firewood or poles from your forest land?				
No	29.058	26.638	27.939	
Yes	70.942	73.362	72.061	
		χ^2 (1, N = 395) = .596, p = .440		
In the past 12 months, have you ever been in a conflict with people concerning the collection of firewood or poles from your forest land?				
No	79.207	78.768	79.003	
Yes	20.793	21.232	20.997	
		χ^2 (1, N = 397) = .001, p = .973		
	Non–Forest Owners			
How many forest owners in your village allow you to collect firewood in their private forests?				x
None	58.306	49.415	53.788	
Some	38.777	46.410	42.655	
All	2.917	4.175	3.556	
		χ^2 (2, N = 343) = 2.205, p = .332		
How many forest owners in your village allow you to collect poles in their private forests?				x
None	72.434	68.323	70.336	
Some	25.645	29.976	27.856	
All	1.920	1.701	1.808	
		χ^2 (2, N = 341) = .827, p = .8268		

Table 3 (*Continued*)

Statement	Control %	Treatment %	Total %	Index %
In the past 12 months, has a forest owner explicitly denied you to collect firewood or poles from his/her forest land?				x
No	84.350	85.858	85.116	
Yes	15.650	14.142	14.884	
		χ^2 (1, N = 352) = .539, p = .463		
In the past 12 months, have you paid a forest owner to collect firewood or poles from his/her forest land?				
No	94.691	96.822	95.775	
Yes	5.309	3.178	4.225	
		χ^2 (1, N = 352) = 1.0476, p = .306		
In the past 12 months, have you collected firewood or poles from somebody else's forest land, without the owner's consent?				
No	91.154	94.028	92.621	
Yes	8.846	5.972	7.379	
		χ^2 (1, N = 355) = 1.0329, p = .309		
In the past 12 months, have you ever been in a conflict with a forest owner concerning the collection of firewood or poles from his/her forest land?				
No	97.078	97.198	97.139	
Yes	2.922	2.802	2.861	
		χ^2 (1, N = 354) = .000, p = 1.000		

Note. The column "Index" indicates which questions were included in the index used in regression analysis (see text). The selected statements for the indices focus only on use restrictions and not self-reported behavior with respect to forest resource use/access. Column percentages are weighted by the inverse sampling probabilities. Pearson χ^2 tests do not include sampling weights.

that forest use restrictions are considered less socially appropriate than their counterparts in villages without PES exposure do. We also find that former PES villages have a greater share of egalitarian social preference types and a smaller share of spiteful types than control villages six years after the PES intervention ended.

That actual behaviors, as well as personal norms around forest restrictions, did not persist over time, indicates that forest owners returned to prior forest resource sharing arrangements after PES were terminated. This is corroborated by additional analysis of land dispute data (see SI-J) that was collected in all three survey waves (base, endline, and follow-up). While land disputes about trespassing increased temporarily during the PES program, differences between treatment and control villages disappeared again in the long run. We can only speculate why we do observe a permanent shift in social preferences, becoming more egalitarian and less spiteful in former PES villages. Could this change be driven by wealth effects, that is, that PES participants are simply wealthier? In this case, one would intuitively expect that PES recipients become more generous and not egalitarian. To shed more light on this potential explanation, we nonetheless test for treatment effects on household wealth, measured by asset ownership (see SI-F). We do not find evidence for any wealth differences six years after PES were terminated (SI, table F5). If the shift in social preferences would be solely driven by increased wealth of PES recipients, one would expect that social preferences revert back to pre-PES levels, once wealth differences disappear.

Another possible explanation for the observed results may be a simple change in resource availability. The PES program could have maintained forest cover in treatment villages and/or increased forest quality in the standing forests due to a temporary ban of resource extraction. Once PES restrictions were lifted, higher resource availability may have resulted in social norms that prescribe forest access for neighbors and in the observed shift in social preferences. We, however, consider this explanation unlikely due to the specific local context. Overall, the covered communities are highly forest constrained. While most of the land is used for agricultural production, forested areas are mostly concentrated along water streams or areas that are less suitable for agriculture. A follow-up study on the forest cover impact of the PES scheme in 2016 indicates that treatment villages have maintained a slightly higher forest cover than control villages after the program ended. However, all communities have experienced from the onset of the PES program in 2011 up to 2016 a drastic reduction in forest cover (46.6% decline in control villages, 37.4% decline in treatment villages; World Bank 2018). At the same time, the PES program with a duration of two years is unlikely to allow for a build-up of substantial forest resources in the standing forests that still prevailed in 2019. Overall, we therefore see little reason to believe that the current resource availability is fundamentally different between treatment and control villages and a potential driver of the observed differences. It may, however, be the case that prior resource availability has shaped resource sharing. Through a lab-in-the-field experiment in Colombia, Pfaff et al. (2015) find that prior exposure to resource scarcity in a sequential water appropriation setting

leads to lower resource sharing once resources are more abundant compared to groups that experienced even more abundance beforehand. One possible explanation may be a weakening of other-regarding preferences, in order to reduce cognitive dissonance and justify prior less generous behavior toward others. If the PES program increased resource abundance temporarily in treatment villages, higher resource sharing might carry over to the current situation with lower resources. Yet, without more survey waves from between the PES end and the current follow-up study, it remains difficult to determine whether such path dependence exists.

These findings are—in our view—therefore supportive of another interpretation. That some households benefited from the PES and others (neighbors in particular) did not benefit or even experienced some negative effects as a result of reduced access to forest lands, may have made community members more aware of the negative impacts of inequalities or related fairness concerns, thus leading to a shift in social preferences.[4] Our finding that the perceived social norm in PES villages favors fewer forest use restrictions is also in line with this interpretation. While we cannot formally test for the underlying causal mechanism at work, we suggest future research to test through lab-in-the-field experiments to what extent incentive payments conditional on removing positive externalities for third parties affect transfers to third parties once incentives are removed again (while holding resource levels constant; field experiments or RCTs may not be feasible due to ethical concerns).

Our findings carry relevant implications for conservation practitioners in the Global South. PES implementers should be sensitive to which resources on targeted land are shared with non-owners of the land before PES are introduced. In many settings, landless households rely on the uses of land that is formally owned by someone else (Jagger et al. 2014). Implementing organizations could proactively disseminate information among entire communities and not only program participants regarding which activities are permissible on land under PES. In addition, PES implementers could consider direct (cash or in-kind) side payments to communities or nonparticipating households to foster support for PES and mitigate potential side effects for nonparticipants. This could also be done in a participatory process whereby PES recipients commit to contribute to such funds. To our knowledge, such design options have received little to no attention from researchers so far (see Veronesi et al. [2015] for an exception). However, our results indicate that at least temporary payments do not change the way forest resources are shared in the long run, which is encouraging. Similarly, we also find that PES did not foster conflict or eroded

4. Such a causal mechanism likely requires a critical share of forest owners to participate in the PES. In the 58 treatment villages that are included in the follow-up, PES enrollment shares among eligible forest owners vary between 0% and 100% (mean = 35 %, median = 31 %). We refer interested readers to SI-I, where we correlate enrollment shares with our main outcomes of interest.

social cohesion within communities, but instead community members have strengthened egalitarian preferences and social norms that favor equitable access to forests.

Many PES schemes around the globe are temporary or lack secure long-term funding when initiated. A thorough understanding of the implications of terminated incentives therefore helps to gain a broader picture about the impact of PES. Focusing instead only on successful, long-term PES seriously risks overestimating the positive impacts of PES on a global scale. While we are—to our knowledge—the first experimental study to focus on the long-term impacts of PES on entire communities, we acknowledge that more comprehensive research is needed to better understand the social implications of conservation incentives. This includes studying the impact of PES programs of different durations (the studied PES was terminated after two years), at different points of the implementation process, and in different contexts with regard to resource access and property rights at the community level.

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