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Payments for ecosystem services did not crowd out pro-environmental behavior: Long-term experimental evidence from Uganda

Tobias Vorlaufer[a,1,2], Stefanie Engel[a], Joost de Laat[b], and Björn Vollan[c]

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Payments for ecosystem services (PES) are increasingly being implemented worldwide as conservation instruments that provide conditional economic incentives to landowners for a prespecified duration. However, in the psychological and economic literature, critics have raised concerns that PES can undermine the recipient's intrinsic motivation to engage in pro-environmental behavior. Such "crowding out" may reduce the effectiveness of PES and may even worsen conservation outcomes once programs are terminated. In this study, we harnessed a randomized controlled trial that provided PES to land users in Western Uganda and evaluated whether these incentives had a persistent effect on pro-environmental behavior and its underlying behavioral drivers 6 y after the last payments were made. We elicited pro-environmental behavior with an incentivized, experimental measure that consisted of a choice for respondents between more and less environmentally friendly tree seedlings. In addition to this main outcome, survey-based measures for underlying behavioral drivers captured self-efficacy beliefs, intrinsic motivation, and perceived forest benefits. Overall, we found no indications that PES led to the crowding out of pro-environmental behavior. That is, respondents from the treatment villages were as likely as respondents from the control villages to choose environmentally friendly tree seedlings. We also found no systematic differences between these two groups in their underlying behavioral drivers, and nor did we find evidence for crowding effects when focusing on self-reported tree planting behavior as an alternative outcome measure.

payments for environmental services | behavioral impact | motivation crowding | intrinsic motivation | climate change

Significance

Deforestation is a major driver of greenhouse gas emissions and biodiversity loss. Payments for ecosystem services (PES) are one prominent policy instrument to protect forests. However, concerns have been raised that the introduction of monetary incentives for land users to engage in environmentally friendly behavior can undermine their motivation to do so in the long term. Such a crowding-out effect may render economic incentives less effective and, in the worst case, even counterproductive. This poses a particular concern if economic incentives are terminated. We studied the long-term effects of a terminated PES program in Uganda using a rigorous experimental design, concluding that pro-environmental behavior and its underlying motives are not adversely affected in the long term by temporary economic incentives.

Payments for ecosystem services (PES) are increasingly being implemented as an alternative to conventional conservation instruments (1, 2). There are, however, concerns that providing such economic incentives to landowners that are conditional on their conservation efforts (3) can undermine their intrinsic motivation (4, 5), which is an important driver of environmentally friendly behavior (6). Over the last decade, an increasing number of studies have examined motivation-crowding effects in the context of conservation policies (7–9). The evidence to date suggests that crowding out is indeed possible, but the opposite (crowding in) or no effect is possible as well.

A major limitation of the existing literature relates to the measurement of crowding effects. Most empirical evidence draws on lab-in-the-field experiments (10–21) that have typically measured behavioral crowding in the context of cooperation within groups or of individual altruistic behavior. The crowding effects in such experiments can be measured just within minutes of the PES having been removed (14, 15, 17–21) or when such incentives are still in place (10–13, 16), thus not allowing for the measurement of truly long-term effects. In addition, one may question whether the behavior exhibited in these experiments is driven by the same underlying motives as the real-world conservation behavior that PES target. Such experiments also struggle to model the activities of PES that are typically implemented in addition to economic incentives, such as training or awareness campaigns. Other studies have evaluated the impact of real-world PES schemes. Some of them have focused on underlying behavioral drivers, such as motivation, attitudes, values, or beliefs, but have not established whether the observed changes altered conservation behavior (22–24). Other studies have instead focused on (often highly aggregated) conservation outcomes or behaviors, but without clarifying the mechanisms through which these changes occurred (25–31). With some exceptions (24–26, 29), these studies are observational, requiring relatively strong assumptions to identify causal effects. A more detailed discussion of the literature and associated methodological challenges can be found in *SI Appendix*, section I.*

*Few studies have investigated whether PES affect social capital and practices at the community level (50, 51). Their focus is not on individual, pro-environmental behavior and its underlying drivers and, hence, these are not discussed in detail here.

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[1]To whom correspondence may be addressed. Email: tobias.vorlaufer@zalf.de.

[2]Present address: Working Group Governance of Ecosystem Services, Leibniz Centre for Agricultural Landscape Research, 15374 Muncheberg, Germany

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Psychological and economic theories have both sought to explain crowding effects (32, 33), with psychologists typically more narrowly defining intrinsic motivation as doing something for the experience of doing it, whereas in economics, intrinsic motivation commonly refers to the motivation to do something good without external rewards or punishments. Furthermore, the theory of self-determination from psychology differentiates between motivation types that range from purely extrinsic, based on rewards or punishment, to purely intrinsic (34). In between, different degrees of internalized motivation exist that are still categorized as extrinsic. This includes self-image concerns or acting to avoid guilt or enhance pride. According to this view, external incentives can lead to either a crowding in or a crowding out of intrinsic motivation, whereas deficits in perceived competence, autonomy, and relatedness enhance the likelihood of crowding out (32, 34). In economics, crowding effects have been discussed from both the empirical and theoretical perspectives (35–38). Based on an extensive literature review, Bowles and Polanía-Reyes (39) developed a theoretical framework, proposing four distinct mechanisms whereby incentives negatively affected pro-social behavior: Incentives can provide information about the regulator (principal), their target, or the task itself. Incentives might also signal appropriate behavior in a given situation or affect one's sense of autonomy. Lastly, incentives may change how endogenous preferences are formed in the long term.

In this study, we examined whether a 2-y PES scheme in Uganda, which was implemented as a randomized controlled trial (RCT), would result in behavioral crowding 6 y after the scheme had ended. We elicited pro-environmental behavior at the individual level from 753 landowners across the original treatment and control groups, using incentivized, experimental methods. This limited the potential demand effects to which self-reported behavioral and attitudinal measures are commonly prone to. Behavioral crowding in PES has been primarily explained by reduced intrinsic motivation, but this effect can also be influenced by other mechanisms. In addition to motivational measures[†], we collected a broader set of behavioral drivers. The frequent interactions with the program implementers may have increased the PES recipient's knowledge and awareness about ecosystem services, which subsequently could have affected the perceived benefits and disbenefits of forest conservation. Because psychological and economic theories have identified perceived autonomy and competence as potential crowding mechanisms, we elicited self-efficacy beliefs and the perceived locus of control with respect to nature conservation (henceforth referred to as self-efficacy beliefs). Experiencing successful forest conservation might strengthen self-efficacy beliefs or weaken them, depending on the salience of the external support (i.e., PES). As our main behavioral outcome variable, we used the share of native tree seedlings that the respondents chose from among six different packages of tree seedlings that varied in their share of native vs. eucalyptus trees. While eucalyptus trees grow faster and provide a higher income in a short period, they have adverse environmental effects on groundwater and soils, unlike native trees. We demonstrate below that the respondents had knowledge of native trees being environmentally friendly or perceived them as such. Thus, the respondents faced a costly trade-off between pro-environmental behavior and short-term economic profit, which mimicked a relevant and familiar task while controlling for outside factors.

[†]In the context of our study, we consider narrowly defined intrinsic motivation as less relevant to pro-environmental behavior. We therefore adopted a broader definition of intrinsic motivation that includes all nonmonetary motives, such as guilt aversion, self-image concerns, and personal norms (*SI Appendix*, section A.2).

The PES program under study included two components—an avoided deforestation component that paid landowners for conserving forests and a reforestation component that offered economic incentives for reforestation with native trees. We consider the respondent's choice of seedlings as a distinct yet closely linked decision and deem it suitable for measuring crowding effect. Using the experimental measure of seedling choice for tree planting also had an advantage over measures relating to forest-conservation behavior, which crucially depend on the number of forested land that a respondent owned. Forest ownership not only varied among the respondents; it had also rapidly declined since the PES program was initiated in 2011 to the point that some respondents no longer had forests at the time of our survey, making forest conservation an unsuitable measure in our context. By contrast, by focusing on reforestation behavior, using the choice of seedlings as a proxy, our experimental measure maintained control over the respondents' choice options. Additionally, as a robustness check, we briefly report on self-reported reforestation behavior, even though this has the disadvantages of recall bias and strong experimenter-demand effects. Because the farmers could potentially have sold the seedlings, we provide additional information to ensure that there were no systematic differences between the treatment and control villages in terms of the use intentions and the perceived availability and price of the different tree species (*SI Appendix*, section A.1).

Based on the mixed results from the literature, as discussed above, we hypothesized that PES led to a crowding in or out of pro-environmental behavior (i.e., based on the choice of native tree seedlings). In terms of behavioral drivers, we hypothesized that PES affected: 1) the intrinsic motivation to plant native trees; 2) the perceived self-efficacy of forest owners with respect to nature conservation; and 3) knowledge about forest benefits (ecosystem services) due to repeated interactions with program implementers. In line with the main hypothesis, we did not formulate any direction for these hypotheses.

We estimate these long-term impacts—referred to as intention-to-treat estimates—using Tobit regressions that compare the outcomes between forest owners in the treatment villages with those in the control villages. While forest owners in treatment villages could enroll in the PES program during the initial RCT, this was not the case for forest owners in control villages. In contrast to the existing literature, we do not estimate the effect of receiving PES but rather the effect of being offered PES, with some forest owners enrolling in the program and others not. One main mechanism discussed in the literature on crowding effects is frameshifting, which states that the introduction of economic incentives can shift recipients' reasoning from nonmonetary to monetary, potentially leading to changed mindsets and values (similar to endogenous preferences) that may persist even if the incentives are terminated (7, 38). Such frameshifting can also occur among nonrecipients who observe fellow villagers receiving money for forest conservation because the villages are relatively small (with a median of eight PES-eligible forest owners). When the PES program ended in 2013, 85% of eligible forest owners were aware of the program. There are thus reasons to presume that the nonparticipants were also affected by the introduction of the PES program. Consequently, common approaches based on instrumental variables to estimate treatment effects on the treated are not feasible because the offer to enroll in the PES program (and the introduction of PES for fellow villagers) may have affected the behavior and motivation of eligible nonparticipants, thus violating the exclusion restriction. Notably, the main reason for nonparticipation in the PES scheme was a lack of information about the program (40). As such, baseline characteristics have small explanatory power to explain participation, which limits the scope for matching techniques. A simple correlational analysis of outcome differences per enrollment status is provided in the *SI Appendix*, section G. Here, we found no systematic differences between the PES participants and nonparticipants (*SI Appendix*, Tables S44 and S45).

Results

We focus first on the choice of tree seedlings as a measure of pro-environmental behavior and, in particular, forest conservation behavior. Survey responses corroborated that the respondents had knowledge of native trees being an environmentally friendly choice or perceived them as such compared to eucalyptus—likely because eucalyptus woodlots are widely planted in the research area. Only 0.2% of the respondents could not list a single advantage of native over eucalyptus trees. Overall, 49.7% and 42.9% of the respondents believed that native trees maintain soil fertility and groundwater levels, respectively, compared to eucalyptus woodlots. Even more respondents (78%) believed that native trees increase rainfall in comparison to eucalyptus.

In our incentivized behavioral experiment, the respondents in the treatment villages selected, on average, a package with 0.606 native trees (SD = 0.398), whereas the average was slightly lower for the respondents in the control villages at 0.575 (SD = 0.400). The package with only native seedlings was the most popular choice across the sample (38.2%) and in the control (36.0%) and treatment (40.5%) villages. The second most popular package was the one with only eucalyptus seedlings, with 19.8% across the sample and little difference between the control (20.8%) and treatment (18.7%) villages. The distribution of the share of native seedlings in the chosen package is illustrated in Fig. 1*A*.

PES did not affect the likelihood of engaging in pro-environmental behavior when compared to the control group. To assess statistical differences between the treatment and control respondents, we estimated Tobit regression models, with the share of native seedlings as the dependent variable. The results are reported in Table 1. We found that the differences between the treatment and control villages were not statistically significant either without (Model 1) or with (Model 2) control variables added. We included these additional controls because the PES may not only have affected the underlying motives for planting native instead of eucalyptus trees (i.e., pro-environmental motives) but also other behavioral determinants, such as 1) the (perceived) cost of buying native seedlings; 2) an increased local supply of native seedlings or improved knowledge about where to get native seedlings; and 3) limited land available for planting additional trees, especially native trees that occupy land for longer periods before they can be harvested (*SI Appendix*, section C and Table S17).

In addition to the experimental measure of pro-environmental behavior, we also collected self-reported information on tree planting (see *SI Appendix*, section E for more details). We found that the respondents from the control villages were 9% more likely to indicate that they had planted trees in the last 12 mo compared to their counterparts from the treatment villages (39.9% vs. 30.8%) (*SI Appendix*, Tables S31 and S35). However, when focusing exclusively on the planting of native trees as a measure of pro-environmental reforestation behavior, the difference disappeared. That is, only 12.2% and 12.8% of the respondents in the treatment and control villages, respectively, planted native trees in the last 12 mo. These differences are not statistically significant, supporting the experimental finding that the respondents from the treatment villages were just as likely as the respondents from the control villages to plant native trees (see Table 1, Model 3, and *SI Appendix*, Tables S31 and S36).

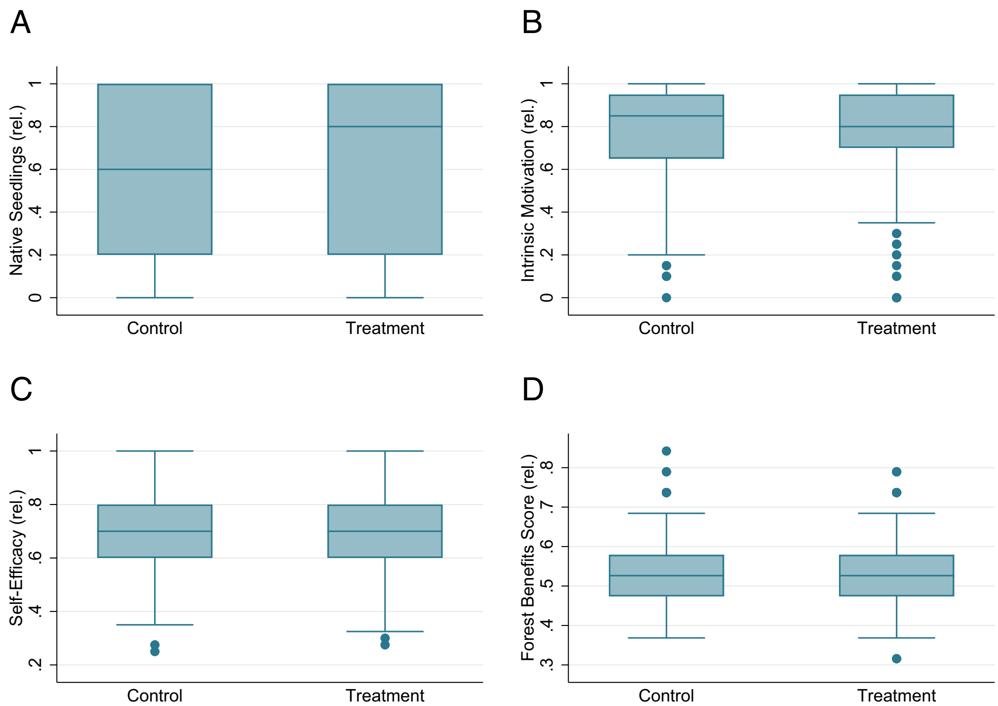

Fig. 1. Differences in outcomes of interest between the PES treatment and control groups. The median (line), box (75th and 25th percentiles), and whiskers cover 1.5 times the interquartile range. Observations outside this range are illustrated as dots. The observations were weighted using inverse sampling probabilities. Panel *A*: Relative share of native seedlings in the chosen seedling package (0–1). Panel *B*: Intrinsic motivation score (0–1). Panel *C*: Self-efficacy score (0–1) with respect to environmental conservation. Panel *D*: Score (0–1) from listed forest benefits and disbenefits.

Table 1. Regression results for behavioral and intermediate outcomes

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Native seedlings (rel.)		Self-reported tree planting	Intrinsic motivation (rel.)	Self-efficacy score (rel.)	Forest benefits score (rel.)
Treatment	0.052	0.036	0.001	0.009	−0.005	0.002
	[−0.094, 0.198]	[−0.108, 0.180]	[−0.057, 0.060]	[−0.036, 0.055]	[−0.024, 0.014]	[−0.007, 0.011]
Constant	0.421*	0.518	0.132*	0.760***	0.526***	0.472***
	[-0.001, 0.844]	[-0.115, 1.151]	[-0.014, 0.277]	[0.650, 0.870]	[0.461, 0.591]	[0.440, 0.505]
Enumerator FE	Yes	Yes	Yes	Yes	Yes	Yes
Stratification controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	–	–	–	–
Subcounty FE	Yes	Yes	Yes	Yes	Yes	Yes
N	751	741	752	749	744	719
Clusters	119	118	119	119	119	118
F-statistic	39.229	35.710	2,546.715	310.704	92.400	973.974
P value	0.000	0.000	0.000	0.000	0.000	0.000
Pseudo R-squared	0.031	0.052		0.337	-0.676	-0.178
R-squared	–	–	0.108	–	–	–

Note: 95% CIs in brackets. SEs are clustered at the village level. Models 1, 2, 4, and 5 are Tobit models bounded between 0 and 1. Model 3 is a linear probability model. The observations were weighted per the inverse sampling probabilities. FE—fixed effects; rel.—relative.**P* < 0.10, ***P* < 0.05, ****P* < 0.01.

PES did not affect intrinsic motivation to engage in pro-environmental behavior, self-efficacy with respect to environmental conservation, or the perception of benefits and disbenefits derived from forests.

Fig. 1 *B*–*D* illustrate the distribution of the intermediate outcomes (i.e., behavioral drivers) differentiated by the treatment and control villages. First, we assessed a self-reported intrinsic motivation score based on the responses to seven statements. On average, respondents showed a relatively strong intrinsic motivation to plant native trees (M = 0.785, SD = 0.222), which provides scope for motivational crowding out. Without baseline data, however, we cannot rule out that intrinsic motivation was not present before the PES program was implemented. However, we deem the preintervention presence of intrinsic motivation to be likely because we observed it in the control group in the follow-up survey. The average intrinsic motivation score was slightly higher in the treatment (M = 0.787, SD = 0.217) than in the control (M = 0.783, SD = 0.228) villages. This difference is, however, not statistically significant in the regression analysis (see Table 1, Model 4). For perceived self-efficacy, we observed a slightly higher score in the control (M = 0.705, SD = 0.148) than in the treatment (M = 0.688, SD = 0.147) villages, but this difference was not statistically significant in the corresponding model (see Table 1, Model 5). Lastly, we observed slightly higher perceived forest benefits in the control (M = 0.546, SD = 0.078) compared to the treatment (M = 0.541, SD = 0.086) villages—a difference that was, again, not statistically significant in the regression analysis (see Table 1, Model 6).

All three behavioral drivers (intrinsic motivation, self-efficacy, and perceived forest benefits) are positively correlated with pro-environmental behavior (i.e., the choice of native seedlings). Intrinsic motivation has the strongest correlation if all factors are jointly assessed.

We next report on the different types of robustness tests that we conducted. For the choice of native seedlings to indeed be a proxy for pro-environmental behavior, it had to be positively correlated with elicited behavioral drivers. That is, the stronger the intrinsic motivation, perceived self-efficacy with respect to environmental conservation, and perceived forest benefits, the more likely a respondent would be to choose a package with native seedlings. To test this, we employed four Tobit regression models, with the share of chosen native seedlings as the dependent variable (Table 2). The first three models found that each intermediate outcome was significantly correlated with the number of native seedlings chosen. We found that a 0.1 increase in the intrinsic motivation score was associated with a 0.17 increase in the share of native seedlings (Model 1), whereas a 0.1 increase in the self-efficacy score increased the share of native seedlings by 0.12 (Model 2). Finally, a 0.1 increase in the forest benefit score increased the share of native seedlings by 0.14 (Model 3). When evaluating the joint effect of the three variables on the main outcome, only the intrinsic motivation score remained statistically significant (Model 4). The intrinsic motivation score also explained the greater variance in the outcome (measured as adjusted R^2) compared to the other explanatory variables. Both intrinsic motivation and self-efficacy beliefs were also positively correlated with the self-reported planting of native trees (*SI Appendix*, Table S37), whereas they did not correlate with the self-reported planting of nonnative trees (*SI Appendix*, Table S38).

Next, we focus on potential spillover effects between the treatment and closely located control villages that could have resulted in an underestimation of the true treatment effect. This could explain the lack of significant results. In Table 3 and Model 1, we restricted our sample to the control villages and included the number of treatment villages within a 5-km radius as the explanatory variable. In Model 2, we specified a treatment variable with three levels, and the control villages with five or fewer treatment villages within a 5-km radius formed the base group. Relative to this, we estimated the effect of being located in a control village with more than five treatment villages in the vicinity, as well as the effect of being located in a treatment village. In both models, we found no

Table 2. Tobit regression results for the intermediate outcomes explaining pro-environmental behavior

	Model 1	Model 2	Model 3	Model 4
	Native seedlings (rel.)			
Intrinsic motivation (rel.)	1.700***	–	–	1.711***
	[1.386, 2.014]			[1.370, 2.053]
Self-efficacy (rel.)	–	1.162***	–	0.331
		[0.635, 1.689]		[−0.219, 0.881]
Forest benefits score (rel.)	–	–	1.388***	0.531
			[0.437, 2.339]	[−0.413, 1.475]
Constant	−0.851***	−0.172	−0.245	−1.315***
	[−1.317, −0.386]	[−0.627, 0.284]	[−0.805, 0.316]	[−1.894, −0.736]
Enumerator FE	Yes	Yes	Yes	Yes
Stratification controls	Yes	Yes	Yes	Yes
Subcounty FE	Yes	Yes	Yes	Yes
N	748	743	718	709
Clusters	119	119	118	118
F-statistic	28.787	34.908	10.389	16.505
P value	0.000	0.000	0.000	0.000
Pseudo R-squared	0.110	0.041	0.038	0.121

Note: 95% CIs in brackets. SEs are clustered at the village level. The Tobit models are bounded between 0 and 1. The observations were weighted per the inverse sampling probabilities. FE—fixed effects; rel.—relative.
$^*P < 0.10$, $^{**}P < 0.05$, $^{***}P < 0.01$.

evidence for spillover effects across the villages (see *SI Appendix*, section D for more details).

For all the presented regression models, we report additional robustness checks in *SI Appendix*, sections C and D.3. This included omitting the enumerator fixed effects and controls that were used during treatment randomization as the stratification variables. All results were robust in terms of these alternative specifications.

Conclusions

Overall, our findings indicate that the PES scheme that reduced deforestation in Western Uganda by half during its 2 y of implementation (40) did not impact pro-environmental behavior and nor did it affect underlying intrinsic motivation, self-efficacy beliefs, or the perception of forest benefits 6 y after the incentives had been terminated. These findings contribute to the increasing knowledge base on motivation crowding under PES. Additionally, our study offers a unique methodological approach to assessing crowding effects by combining a real-world PES program, implemented as an RCT with incentivized, experimental measures at the individual level, and measures of underlying behavioral drivers.

While some studies have indicated that PES can affect the underlying behavioral drivers of conservation behavior (22, 23), our findings do not support the notion that PES crowd out intrinsic motivation and consequent pro-environmental behavior in the long term. Our results are also in line with the emerging, yet limited, literature on the long-term effects of terminated real-world PES on the initially targeted behavior. With a few exceptions (30), the findings of these studies indicate that, in the worst case, behavior reverts to its level prior to the PES but does not backfire (25–29, 31). However, we also did not observe positive crowding-in effects, as other experimental studies have suggested (15, 17, 19, 20, 24, 41).

We believe that a detailed microlevel approach as presented here, with an experimental measure of pro-environmental behavior complemented by survey items to measure underlying behavioral drivers, has inherent advantages. It allows us to obtain a more nuanced understanding of human responses to conservation instruments compared to solely focusing on the outcome that the policy or program initially targeted (e.g., measuring deforestation levels using remote sensing). Both approaches should be seen as complementary and should ideally be combined in future studies.

Table 3. Tobit regression results testing for spillover effects

	Model 1	Model 2
	Control villages	All villages
No. of treatment villages within a 5-km radius	−0.015	–
	[−0.055, 0.025]	
Treatment	–	−0.066
		[−0.260, 0.129]
Control (>5 treatment villages within a 5-km radius)	–	−0.199
		[−0.440, 0.042]
Constant	0.364	0.472**
	[−0.315, 1.042]	[0.025, 0.919]
Enumerator FE	Yes	Yes
Stratification controls	Yes	Yes
Subcounty FE	Yes	Yes
N	389	751
Clusters	61	119
F-statistic	6.293	36.541
P value	0.000	0.000
Pseudo R-squared	0.053	0.033

Note: 95% CIs in brackets. SEs are clustered at the village level. The Tobit models are bounded between 0 and 1. The observations were weighted per the inverse sampling probabilities. FE—fixed effects.
$^*P < 0.10$, $^{**}P < 0.05$, $^{***}P < 0.01$.

The specific contexts of the studied PES scheme are worth discussing. First, the sample used in this follow-up included 128 PES recipients (~35% of the 363 eligible forest owners at baseline, with an overall enrollment rate in the RCT of 32%). While the enrollment may seem relatively low, other randomized PES programs have reported even lower participation rates of between an estimated 10% (42) and a median of 14% (43). Second, the respondents in our study privately own land but they do not derive their main income from forests. Nonetheless, forests provide them with important resources for their livelihoods, including building materials and energy, and are a source of emergency funding. Third, the payments to the forest owners were fixed and solely area-based. Differentiated payments, or the selective termination of contracts, could have affected fairness perceptions, potentially changing the underlying behavioral drivers. Fourth, the payment levels in the studied PES scheme were sufficiently high to motivate the land users to reduce deforestation. Prior research has indicated that incentives that are too low might backfire (44) and crowd out intrinsic motivation, even though lab-in-the-field experiments with resource users receiving PES have not confirmed this (16).

As noted above, one key difference to the existing crowding literature is that we estimated the intention-to-treat effect, which comprises the overall effect of the PES on those who have been offered PES and not only on those who enrolled in the PES program. The majority of nonparticipants stated that they were either not aware of the program, did not know how to sign up to participate in the program, or perceived the contracts to be too complicated during the program's sign-up phase (40, 45). Over the course of the PES implementation, most nonparticipants, however, learned about the program and the payments involved, which may have affected their underlying motivation and future propensity to engage in forest conservation. Therefore, the causal estimate from this study has to be understood as the aggregate effect of offering PES to forest owners at the community level. From a policy perspective, this aggregate treatment effect seems highly relevant because, in most—if not all—PES programs that target private landowners, some decide to participate and others do not. However, we cannot rule out that potentially opposing crowding effects occur at the subgroup level. Those PES programs that are perceived as supportive are also more likely to lead to crowding in of intrinsic motivation, whereas programs that are perceived as controlling are more likely to induce crowding out (9). However, oftentimes, it is the context of the intervention that may be seen as supportive or controlling, and this may only be known to the participants who interact with the program staff. Simultaneously, specific mechanisms may only be at work among nonparticipants. For example, nonparticipants may be frustrated because they missed out on payments, leading to crowding out. Frameshifting may also only occur among nonparticipants who primarily see monetary rewards instead of nonmonetary recognition for conservation efforts that participants experience through personal interactions. Therefore, future studies should aim to estimate separate crowding effects for those who do and do not enroll in a PES program, as well as identify the potential mechanisms responsible for subgroup crowding effects.

The studied PES program was implemented for a relatively short period (2 y), and the follow-up was conducted 6 y after the incentives had ended. As such, our study is best understood as a lower-bound estimate of crowding effects. While our study has a relatively large sample size compared to existing empirical studies, we cannot rule out that our study may have been inadequately powered to discover small treatment effects that could be anticipated with a PES program that was terminated 6 y ago and where around one-third of the eligible landowners participated. This concern echoes the recent debate about the statistical power of empirical research in the social sciences in general, and environmental economics specifically (46). However, from a practitioner's perspective, the question is also whether such small effect sizes would be of economic relevance.

Future research should ideally focus on PES programs that are implemented for a longer period, and they should conduct short-, medium-, and long-term follow-up surveys. However, most PES programs are not implemented using a randomized treatment assignment, so researchers usually need to rely on quasiexperimental methods for causal inference. To facilitate this, program implementers and researchers are best advised to collect baseline data on relevant pro-environmental behavior and underlying behavioral drivers before initiating any PES activities.

Materials and Methods

Intervention and Study Design. This study was based on an RCT of a PES intervention implemented between 2011 and 2013 in Western Uganda (Hoima and the Kibaale District), which compensated forest owners for conserving intact and rehabilitating degraded forests by planting trees. PES were assigned to 60 randomly selected villages (i.e., the treatment villages), while the 62 remaining villages served as the control group. Overall, 133 landowners in the treatment villages participated in the program. This corresponds to an enrollment rate of 32% among eligible forest owners. On average, landowners enrolled 2.06 ha of forests and received US$113 over the entire duration of the program. Notably, Jayachandran et al. (40) found that the intervention successfully reduced deforestation rates from 9.1% in the control villages to 4.2% in the treatment villages within the intervention period. Furthermore, a smaller share (14.9%) of eligible households participated in the reforestation component. On average, 0.10 ha were reforested per eligible household, and 31 trees were planted, of which 9.8 survived. A detailed account of the RCT implementation can be found in Jayachandran et al. (40). Moreover, a follow-up remote-sensing study found that the treated communities did not catch up with control villages through higher deforestation after the program ended (26).

At the end of the PES program in 2013, 85% of eligible forest owners in the treatment villages were aware of the program. At the end of our survey in 2019, we collected information about whether and how the respondents in the treated villages recalled the PES scheme. Overall, 39.6% of landowners in the treatment villages could name the exact organization that had implemented the program, indicating that many respondents still remembered details about the PES program 6 y after its termination. This number was higher among the respondents of households that had received PES (57.9% vs. 30.2%). Among those who could recall the PES scheme and had enrolled in it, 35.5% perceived that they had benefited "a lot," 57.4% perceived that they had benefited "a little," and only 7% perceived that they had "not benefited at all" from the program. Additionally, among those who recalled the program and participated, money was perceived to be the most important benefit of the program (37.9%), followed by information (29.8%) and seedlings (29.7%).

Data Collection. The data for the present study were collected 6 y after the PES program was terminated (October/November 2019). Overall, 753 households from 58 treatment villages (n = 363) and 61 control villages (n = 390) were interviewed. Two villages had to be excluded from the study because we did not receive permission to conduct interviews from the village leaders due to land-tenure security concerns. The study protocols (including the survey instruments) received both national (Ugandan) and international ethical approval prior to the data collection, from the Mildmay Uganda Research Ethics Committee (#0707-2019) and the Innovations for Poverty Action Institutional Review Board (#15032), respectively. Each respondent confirmed their prior, free, and informed consent by providing their signature or thumbprint. Prior to this, the enumerators and respondents jointly went through a document describing the scope of the study, the rights of the respondents, and the confidentiality of the personal information provided. This study was also preregistered at the American Economic Association RCT registry prior to receiving the data from the data collection team. The preanalysis plan and the replication package, including the dataset and survey material, is publicly available.

We calculated the minimum detectable effect size for the main outcome (i.e., the experimental measure of pro-environmental behavior). With a conventional Type-I error rate of 0.05 and a Type-II error rate of 0.8, and by taking estimates for the intracluster correlation from the whole sample and the SD of the outcome from the control group, this study was sufficiently powered to detect differences in our main outcome of 0.09 (a SD of 0.23) and above as statistically significant.

Sampling Strategy and Attrition. A subset of households covered by the initial RCT study were randomly sampled in three stages. First, four households in each village were randomly sampled to ensure that households from all the villages were sampled and to maintain sufficient statistical power. Second, a specific number of remaining households were sampled across the villages. During this step, we stratified the sampling per forest size at the baseline to increase the number of households that still owned a forest. Third, if a sampled household could not be interviewed, either because it could not be tracked (e.g., due to migration) or because it refused to be interviewed, the enumerators replaced the household with another random household from the same village. A detailed description of the sampling strategy can be found in *SI Appendix*, section B.1. To account for different sampling probabilities, depending on the size of the village and the amount of forest owned at the baseline, all reported statistics include sampling weights derived from 100,000 Monte Carlo simulations (see *SI Appendix*, section B.2 for more details).

The attrition rate was 17%–out of 910 sampled households (including the replacements), 157 could not be interviewed. The attrition was slightly higher in the treatment (19%) than in the control (15%) villages, even though this difference was not statistically significant at $X^2(1, n = 910) = 2.2.43$, $P = 0.134$ (see *SI Appendix*, section H for more details). We did, however, find significant differences between the attrition households in the treatment and control villages, based on the baseline characteristics (*SI Appendix*, Table S48). Nonetheless, the baseline variables did not affect the likelihood to attrite in the treatment and control villages differently (*SI Appendix*, Table S47). We have also reported on Lee Bounds (47) to ensure that our null results were not purely driven by selective, nonrandom attrition (*SI Appendix*, Table S49).

Sample Characteristics. On average, the respondents were 51.6 y old (SD = 14.7) and had 6.8 y of formal education (SD = 4.5). Of the respondents, 25% were female, and 85.8% were the head of their household, 13% of these being female-headed. The households owned on average 7.6 ha of land (SD = 14), with around half of the sample (51.8%) owning land covered by natural forest, corresponding to an average forest size of 0.53 ha across the respondents (SD = 1.16). Slightly fewer households (40.7%) owned a planted woodlot, which typically consists of eucalyptus or pine trees, and the average size of the woodlots was 0.345 ha (SD = 2.05). More detailed sample characteristics per treatment status, and for the baseline, can be found in *SI Appendix*, Tables S14 and S15, respectively.

Survey Instruments. The main outcome (i.e., pro-environmental behavior) was elicited through an incentivized experiment. The respondents could choose one of six unique seedling packages, each containing 20 seedlings. One respondent from each village was randomly chosen to receive the seedlings. The seedlings were then delivered from a local tree nursery to the winning household or to a predetermined pickup point in the village. Additionally, the seedling packages differed in their compositions (i.e., in the share of native vs. alien tree species included). Musizi trees (*Maesopsis eminii*) and Muvule trees (*Milicia excelsa*) were included as species native to the area, while hybrid eucalyptus species were included as alien species. Notably, these hybrid varieties were similarly difficult to acquire in the research area than the native species. Because eucalyptus trees generally grow faster, they are more economically viable, but this is at the expense of higher water consumption and the depletion of soil nutrients. Additionally, eucalyptus plantations provide limited wildlife habitats. More details on how the main outcome was elicited are provided in *SI Appendix*, section A.1. We took the proportion of native seedlings in the chosen packages as the outcome, bound between 0 and 1, with 1 indicating a stronger pro-environmental choice.

The intrinsic motivation to engage in pro-environmental behavior was elicited through seven statements answered on a 5-point Likert scale. The responses were then summarized on a scale ranging between 0 (weak intrinsic motivation) and 1 (strong intrinsic motivation). These statements were adapted from Moros et al. (41) to reflect the activity of planting native instead of eucalyptus trees (see *SI Appendix*, section A.2 for details). Each statement captured one type of motivation, as distinguished in the self-determination theory of Ryan and Deci (34).

Perceived self-efficacy and locus of control were measured using 10 statements answered according to the level of the respondent's agreement on a 5-point Likert scale (see *SI Appendix*, section A.3 for details). The sum of all the answers was then aggregated and normalized between 0 and 1. A higher score indicates greater perceived self-efficacy (i.e., the belief that one can positively impact the environment) and an internal locus of control (i.e., the belief that most environmental problems are caused by human activities).

Perceived forest benefits and disbenefits were measured by asking the respondents to list the benefits and disbenefits that were generated by four hectares of natural forest (see *SI Appendix*, section A.4 for details). Here, the answers were assigned to predefined categories. The sum of the categories was then calculated and normalized between 0 and 1, with a higher score indicating more perceived benefits.

The general environmental attitudes of the respondents were measured using a slightly adapted New Ecological Paradigm Scale (48) that consists of five subscales. As specified in the preanalysis plan, only subscales with adequate internal validity (measured with a Cronbach's alpha greater than 0.6) were to be summarized and analyzed. Unfortunately, all the subscales had Cronbach's alpha values below 0.25, indicating that the internal validity of the measure was compromised (see *SI Appendix*, section A.5 for details). Therefore, we have not reported the results for this intermediate outcome.

Data, Materials, and Software Availability. Anonymized Data, Experimental and Survey Material, Analysis Scripts have been deposited in Open Science Framework (OSF) (49).

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Author affiliations: [a]School of Business Administration and Economics, Osnabruck University, Osnabruck 49076, Germany; [b]Utrecht School of Economics, Utrecht University, Utrecht 3584, The Netherlands; and [c]School of Economics, Marburg University, Marburg 35032, Germany

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