RESEARCH ARTICLE

ECONOMICS

Cash for carbon: A randomized trial of payments for ecosystem services to reduce deforestation

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We evaluated a program of payments for ecosystem services in Uganda that offered forest-owning households annual payments of 70,000 Ugandan shillings per hectare if they conserved their forest. The program was implemented as a randomized controlled trial in 121 villages, 60 of which received the program for 2 years. The primary outcome was the change in land area covered by trees, measured by classifying high-resolution satellite imagery. We found that tree cover declined by 4.2% during the study period in treatment villages, compared to 9.1% in control villages. We found no evidence that enrollees shifted their deforestation to nearby land. We valued the delayed carbon dioxide emissions and found that this program benefit is 2.4 times as large as the program costs.

Land-use change—mostly deforestation—was responsible for 9% of global anthropogenic carbon emissions between 2006 and 2015, making it the second largest source of carbon emissions after fossil fuel combustion (*1*). Trees absorb carbon dioxide (CO_2) through photosynthesis and store the carbon in their biomass. When a tree is cut down, it stops absorbing CO_2, and, as it decomposes or is burned, its store of carbon is released into the atmosphere.

Curbing deforestation in low-income countries, where most deforestation occurs today, is viewed as one of the most cost-effective ways to reduce global CO_2 emissions (*2*, *3*). The reasoning is that the cost to intensify agriculture and adopt nonwood fuels and construction material in poor countries to meet growing consumption needs without clearing forests is much lower than the cost of many of the technological changes in high-income countries that reduce emissions by the same amount. Thus, having rich countries finance forest conservation projects in poor countries is a promising way to address climate change.

Indeed, the United Nations' REDD+ (Reducing Emissions from Deforestation and Forest Degradation) mechanism was created to financially reward developing countries for preservation of forests. The Paris Agreement negotiated in 2015 bolstered the role of REDD+ in international climate policy (*4*, *5*). To put REDD+ into action, one needs to identify effective on-the-ground interventions that reduce deforestation. This Research Article evaluates a promising and popular type of intervention, namely, financial incentives for forest owners to keep their forest intact. This type of policy is called Payments for Ecosystem Services (PES): Payments are made conditional on voluntary proenvironment behaviors (*6*–*8*). PES is the environmental version of conditional cash transfers, a policy instrument used widely in developing countries to incentivize families to invest in child health and education.

Despite the widespread use and growing popularity of PES, its effectiveness and cost-effectiveness are open questions. One key concern is what the PES community calls "additionality" and economists refer to as "inframarginality": Absent the payments, some participants would have engaged in the incentivized behavior anyway. If such individuals represent many of the enrollees, then the program will generate very little gain in forest cover per dollar spent. A second concern is that individuals will simply shift their tree-cutting from land covered by the PES contract to other nearby land ("leakage").

This study was a randomized evaluation of a PES intervention that was implemented in order to measure the causal impacts on forest cover. The PES program offered private owners of forest in western Uganda payments if they refrained from clearing trees. The program was designed and implemented by a local conservation nonprofit, Chimpanzee Sanctuary and Wildlife Conservation Trust (CSWCT) (*9*). The study was carried out in 121 villages with private forest owners (PFOs); we randomly selected 60 of the villages to be in the treatment group. In treatment villages, the PES program was marketed to PFOs, and they were eligible to enroll. To participate, PFOs had to enroll all of their primary forest (*10*). During the 2-year program from 2011 to 2013, enrollees who complied with the contract received 70,000 Ugandan shillings (UGX), or $28 in 2012 U.S. dollars, per hectare of forest per year, paid out in cash at the end of each year. CSWCT employed forest monitors who conducted spot checks of enrollees' land to check for recent tree-clearing. The program also offered additional payments in exchange for planting tree seedlings. The PES program is described in more detail in the supplementary materials (SM), section 1.

We measured the impact of the program on forest cover by analyzing satellite imagery. We tasked a very high resolution commercial satellite, QuickBird, to take images of the study region at baseline and endline and classified each pixel as tree-covered or not using eCognition, an object-based image analysis software product (*11*). The QuickBird pixel size is smaller than the crown of a typical mature tree. By comparing PFOs' land in treatment and control villages, we assessed how many additional hectares of tree cover the program caused. We also estimated program impacts on secondary outcomes collected via a household survey. We then analyzed cost-effectiveness by calculating the amount of CO_2 emissions delayed by the program and its monetary value, using the "social cost of carbon," and compared this program benefit to the program costs.

Study context

The study was conducted in Hoima district and northern Kibaale district in Uganda. Forests cover an eighth of Uganda's land area, and its deforestation rate between 2005 and 2010 was 2.7% a year, the third highest in the world (*12*). The pace of deforestation is even faster on privately owned land, which represents about 70% of the forest in Uganda (*13*). As in much of Africa, the main drivers of deforestation in the study region are subsistence agriculture and domestic demand for timber and charcoal (*14*). Trees are often sold by PFOs to timber and charcoal dealers and feed into a national market, with much of the end use in urban areas.

In addition to reducing atmospheric CO_2, forests increase habitat for biodiversity. Many species are threatened by deforestation in western Uganda, notably chimpanzees, an endangered species that is important for Uganda's tourism industry. Other benefits of forest conservation include watershed protection and reduced siltation and flooding.

Experimental design and data

To select study villages, we used Landsat satellite images to identify villages in the study districts with forest and then conducted a census of PFOs in these villages. We selected 121 villages with PFOs for the study and then carried out a baseline survey of PFOs in these villages (see fig. S1 for a map of the study villages).

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The sample of PFOs comprises the 1099 individuals (564 in treatment villages and 535 in control villages) who completed the baseline survey and for whom we have the Global Positioning System (GPS) coordinates of their home (see SM section 2.1 and tables S1 and S2). After the baseline survey, we conducted subcounty-level public lotteries to choose which villages were in the treatment group (see SM section 2.2).

The primary outcome is forest cover or, more precisely, tree cover. The analysis quantified the hectares of tree gain or loss; tree loss is inclusive of both parcels of forest becoming smaller in area (deforestation) and selective cutting of trees within the forest (degradation), as well as any shifting of tree-cutting from forest enrolled in the program to other nearby land. We analyzed QuickBird satellite images to measure tree cover using object-based image analysis and a change-detection technique (see SM section 2.3 and fig. S2). QuickBird is a commercial satellite with a multispectral resolution of 2.4 m and a panchromatic resolution of 0.6 m. We tasked QuickBird to image the study region at baseline and endline. The endline image was taken before the program ended, so that the imaging was done while PFOs still had a financial incentive to conserve their forest. The program had been in place, on average, 1.5 years at the time of the endline images.

In the statistical analysis, the unit of observation is either the entire village or the PFO's landholding. For the village-level analysis, which is the main analysis, we used administrative boundaries of the village from the Ugandan Bureau of Statistics. For the PFO-level analysis, we used a circle around the PFO's home that was twice as large as his or her landholding to approximate his or her land; it was not possible to collect land boundaries for each PFO (see SM section 2.4). Although the PFO-level analysis omits some of the PFOs' land and thus might underestimate the treatment effects, it is a useful complement to the village-level analysis because it allows us to examine whether there is selective enrollment based on the PFO's counterfactual deforestation and how treatment effects vary with PFO characteristics.

The second source for outcome data was the PFO survey. We conducted an endline survey similar in structure to the baseline survey to measure self-reported forest conservation behavior and socioeconomic outcomes (see SM section 2.5 for details). The third data source was the administrative records of CSWCT on program enrollment, compliance, and payments to PFOs. As a control variable, we also used preintervention Landsat-based measures of photosynthetic vegetation.

Summary statistics

The key baseline variables from the PFO survey and satellite data and the tests for balance between the treatment and control groups are summarized in Table 1. The first two columns report variable means and standard deviations by treatment status, and the third column reports the normalized difference (ND) in means [treatment mean minus control mean, divided by the pooled standard deviation]. None of the 20 variables tested has imbalance at the 10% statistical significance level, and the magnitudes of the normalized differences are all smaller than 0.2.

In levels, the mean (median) self-reported land area owned per PFO was 10.8 (5.3) hectares. The table reports the inverse hyperbolic sine (IHS), or $sinh^{-1}$, of land owned. The IHS function approximates the log function, and we use it in lieu of the log function because it accommodates zeros (*15*). The average forest area that a PFO owns was 2 ha. Thus, a typical program enrollee would earn $56 a year for compliance, which is equal to 5% (16%) of average (median) annual household income.

About 85% of PFOs report having cut trees in the 3 years preceding the baseline survey; 24% had done so to use the land for cultivation, and 71% had done so to use or sell the timber products. PFOs also report that they use trees as a source of emergency cash to pay for unexpected costs, such as hospital bills, or large expenses, such as school fees. Average revenue from timber products in the previous year was 110,000 UGX or $44. About 29% of PFOs reported earning any revenue from timber products in the past year, and among them, the mean (median) revenue was $151 ($40). These statistics indicate that foregone annual income from timber products was, on average, somewhat less than the payment that a PFO who owns 2 ha of forest could receive from the PES program, but opportunity costs also vary considerably across PFOs.

The next rows summarize variables based on the satellite imagery, at both the village and PFO level. About a quarter of the land area in the study villages is tree-covered.

Empirical strategy

We estimate the following ordinary least squares regression to quantify the impacts of the PES program:

$$\Delta TreeCover_j = \alpha + \beta Treat_j + X_{1j}\,\delta + X_{2j}\,\mu + \varepsilon_j \quad (1)$$

The outcome is the change in the area of tree cover between baseline and endline in village j. The regressor of interest is *Treat*, which equals 1 in the treatment villages and 0 in the control villages. The coefficient β is the effect of the PES program, which is hypothesized to be positive.

We control for the vector X_1, which encompasses variables related to the stratification procedure, in all specifications. X_1 comprises subcounty fixed effects, as the randomization was stratified by subcounty and the village-level variables that

Table 1. Summary statistics for treatment and control groups. Observations for treatment and control: PFOs, 564 and 535; villages, 60 and 61. Subsample means with standard deviations in brackets. The last column reports the regression-adjusted difference in means between the treatment and control subsample divided by the pooled standard deviation. None of the differences has a $P < 0.10$. The standardized difference and P values are based on a regression with subcounty fixed effects and clustering at the village level. IHS denotes inverse hyperbolic sine. Tree cover in village, percentage of village with tree cover, and percentage change in vegetation in village are at the village level. The data source for variables with Tree cover in the variable name is the baseline QuickBird satellite image. The source for percentage change in vegetation is 1990 and 2010 Landsat satellite images. The source for all other variables is the baseline survey.

	Treatment	Control	ND
Household head's age	47.499 [13.605]	47.589 [14.659]	0.003
Household head's years of education	7.715 [4.003]	7.931 [4.187]	−0.056
IHS of self-reported land area (ha)	4.062 [1.021]	4.004 [0.968]	0.053
Self-reported forest area (ha)	1.727 [3.318]	2.068 [12.413]	−0.042
Cut any trees in the last 3 years	0.845 [0.362]	0.858 [0.350]	−0.031
Cut trees to clear land for cultivation	0.236 [0.425]	0.241 [0.428]	−0.016
Cut trees for timber products	0.704 [0.457]	0.721 [0.449]	−0.037
Cut trees for emergency/lumpy expenses	0.25 [0.433]	0.292 [0.455]	−0.088
IHS of total revenue from cut trees	1.238 [2.118]	1.397 [2.248]	−0.085
Rented any part of land	0.163 [0.370]	0.198 [0.399]	−0.091
Dispute with neighbor about land	0.218 [0.413]	0.206 [0.405]	0.035
Involved in any environmental program	0.100 [0.301]	0.111 [0.315]	−0.035
Agree: Deforestation affects the community	0.539 [0.499]	0.548 [0.498]	−0.014
Agree: Need to damage environ. to improve life	0.064 [0.245]	0.043 [0.204]	0.089
Tree cover in village (ha)	134.515 [108.800]	159.18 [178.011]	−0.169
Percentage of village with tree cover	0.247 [0.122]	0.263 [0.132]	−0.122
Percentage change in vegetation in village, 1990–2010	0.036 [0.041]	0.041 [0.033]	−0.128
Tree cover in PFO land circle (ha)	4.355 [12.466]	3.845 [9.178]	0.050
Percentage of PFO land circle with tree cover	0.199 [0.161]	0.209 [0.157]	−0.044
Percentage change in vegetation in PFO land circle, 1990–2010	0.035 [0.066]	0.037 [0.058]	−0.016

we ensured were balanced in the randomization: number of PFOs, average household earnings per capita, distance to a road, and average land size (16). X_2 are additional control variables we include in our preferred specification, namely, 1990 and 2010 Landsat-based measures of photosynthetic vegetation to control for any pretrends in deforestation and dummy variables for the date of the baseline satellite image.

The regression analysis weights each observation by the proportion of the village polygon with nonmissing tree classification data. Because of cloud cover, we were missing tree classification data for a portion of the land (see fig. S3). We have, in essence, an aggregate outcome variable measured with a sampling rate that varies by observation, so to improve efficiency, we weighted by this sampling rate (17).

We also estimated the effect on tree cover with the PFO as the unit of observation, using a model analogous to Eq. 1. Because the treatment varies at the village level, we allowed for nonindependence of the error term within a village; that is, we clustered standard errors by village. In the PFO-level regressions, the coefficient on *Treat* is the estimated effect per eligible PFO or the intention-to-treat estimate. We estimated similar PFO-level regressions to assess the program's effect on enrollment, payments received, and survey-based outcomes.

Program enrollment

Of the 564 PFOs in the sample in treatment villages, 180, or 32%, enrolled in the PES program. The regression estimate of the effect of treatment on enrollment is shown in Table 2, column 1; residing in a treatment village increases enrollment by 32.0 percentage points (18).

The effect of the treatment on enrolling and conserving forest, as assessed by CSWCT through its monitoring activities, is reported in Table 2, column 2. The treatment effect is 28%, or equivalently, 88% of enrollees complied with the requirement to preserve their forest (19). The treatment effect on PES payments (column 3, total payments during the 2 years) was about 90,000 UGX or $36. Thus, per PFO who enrolled, the average payment was $113 ($36/32.0%). The proportion of total possible payment that this represents is shown in column 4. The effect of 0.238 implies that, on average, enrollees received 74% of the payments for which they were eligible. This proportion is lower than the conservation compliance rate of 88%, as several PFOs who conserved their forest received less than full payment because they did not plant the seedlings they agreed to plant or the seedlings died.

Enrollment in the program conferred option value to a PFO: He or she could sign up and decide later whether to comply. There was no punishment for enrolling but not complying. Thus, it is somewhat surprising that the enrollment rate was not higher than 32%. The endline survey asked treatment PFOs why they did or did not sign up for the program. Most nonenrollees said they were unaware of the program or did not know what it was about (see fig. S4). CSWCT's marketing efforts did not succeed in informing all PFOs about the program. The logistics of enrolling were also a barrier to enrollment (20). Together, these reasons account for three-fourths of nonenrollment. There are also more fundamental reasons for lack of enrollment. Some PFOs found the PES contract complex and difficult to understand or worried that it was a ploy to grab their land. Although it might be possible to explain the PES contract more thoroughly to PFOs when rolling out future programs, these barriers to enrollment are rooted in lack of formal property rights, concerns about corruption, and low levels of education—factors that are common in many of the low-income settings amenable to deforestation PES programs. The other main reason cited for not enrolling was simply that the PFO preferred deforesting to receiving the financial payments (21).

We also used the baseline data to assess the determinants of enrollment (see table S3). Few PFO characteristics predict enrollment, which is consistent with enrollment being largely related to marketing rather than low demand. An important test is whether enrollment is systematically higher for those with lower counterfactual deforestation. This analysis speaks to how much "additionality" to expect. We used the control group PFOs and regressed the change in tree cover on preintervention characteristics. We used the results to predict the business-as-usual change in tree cover for each treatment PFO. We found that enrollment is unrelated to predicted deforestation. The signature of especially high enrollment by those who would have anyway kept their forest intact would be a positive correlation between enrollment and the counterfactual change in tree cover; instead, we find a small, negative correlation.

Impacts of the PES program on tree cover

The main results of the Research Article—the effect of the PES program on tree cover—are presented in Table 3. All columns control for stratification controls, and those after column 1 additionally control for the area covered by photosynthetic vegetation in 1990 and 2010 and the date of the baseline satellite image.

To help interpret the treatment effects, it is useful to first understand the business-as-usual patterns of deforestation. In the control group, the average tree loss per village between baseline and endline was 9.5 log points or 9.1% of baseline tree cover. This rate of tree loss was higher than most estimates of Uganda's rate of forest loss, which were based on changes in the perimeter of the forest. The high-resolution data we used detects additional loss of trees due to selective tree-cutting within the forest, i.e., forest degradation.

The PES program caused an increase in tree cover, relative to the control group, of 5.55 ha per village (Table 3, column 1). The treatment group experienced net tree loss but considerably less than the control group. The main specification (column 2) added in further control variables, and the results were similar; the effect size was 5.48 ha, which corresponds to about 0.368 ha per PFO (22). The treatment effect is statistically significant at the 5% level. Estimates of the proportional effect size are shown in column 3. The treatment effect of 5.2 log points implies that average tree loss in the treatment group was 4.3 log points or 4.2%, which was about half of the 9.1% tree loss in the control group.

The PFO-level results are shown in the next three columns of Table 3. The treatment effect is 0.27 ha per PFO (column 5). This magnitude was smaller than the village-level estimates, which was likely due to PFOs' forest not being fully located within the circles of land around their homes that we used in the study. Note that the coefficient on *Treat* is the intent-to-treat estimate or effect per eligible PFO; the treatment-on-the-treated estimate (per enrollee) was 0.88 ha.

The treatment effect was large. It represented about half of the counterfactual decrease in tree cover, which was larger than the enrollment rate (although the difference is statistically indistinguishable from zero). The magnitude suggests that the payments were not simply inframarginal to PFOs' behavior, and, in fact, those who enrolled in the program would have deforested somewhat more than the typical PFO, absent the program. Note that enrollment was uncorrelated with deforestation predicted by baseline variables (table S3), so enrollees appeared to have higher unpredicted

Table 2. Program enrollment, compliance, and payments. All columns include subcounty fixed effects and the four village-level baseline variables used to balance the randomization: number of PFOs in baseline sample, average weekly earnings per capita, distance to the nearest main road, and average size of the reported land nearest the dwelling. Amount paid is in units of 10,000 UGX. Calculated values are means; standard errors are clustered by village. Asterisks denote significance: ***$P < 0.01$. Outcome data are from CSWCT administrative data.

	Enrolled	Enrolled and deemed to have conserved forest	Amount paid	Proportion of eligible amount paid
Treatment group	0.320***	0.282***	9.023***	0.238***
	[0.030]	[0.028]	[1.872]	[0.024]
Control group	0.009	0.009	0.417	0.007
Observations	1099	1095	1095	1095

deforestation. The program might have also reduced deforestation among nonenrollees in treatment villages, which could partly explain the large effect size. Village norms about tree-cutting or about barring others from taking trees from one's land might have changed (*23*). Also, a very small part of the tree gain could have been due to reforestation (see table S4) (*24*).

We conducted several further analyses to assess the robustness of the results, which are reported in tables S5 to S8 and discussed in SM section 3.1. The robustness checks included estimating unweighted regressions, dropping PFOs with very large landholdings, using circles around PFOs' land of different sizes, and excluding cases where the baseline QuickBird image was taken after randomization.

We also tested for heterogeneous treatment effects by PFO characteristics (table S9 and SM section 3.2). The general pattern we found was that, if a characteristic was predictive of more deforestation in the control group between baseline and endline, it was also associated with a larger treatment effect, i.e., with the intervention averting more deforestation. For example, owning more tree-covered land or having land with higher tree density at baseline was predictive of more tree loss between baseline and endline in the control group, and these characteristics were also associated with a larger treatment effect. The heterogeneity patterns are consistent with two results discussed above, namely, that program enrollment was unrelated to predicted counterfactual deforestation and, according to CSWCT, most enrollees complied with the PES contract and refrained from deforesting. As a result, the program impacts are largest for those who would have deforested the most in the absence of the program.

To assess how important the composition of enrollment was to the magnitude of the program effect, we calculated how the effect size would vary if enrollment had been representative of all PFOs, or concentrated among those who would have deforested the most absent the program, or concentrated among those who would have deforested the least (see SM section 3.3 and fig. S5). One finding was that if the 32% of PFOs who enrolled had been those with the least counterfactual deforestation, the effect size would have been 0 ha. That is, despite 32% of PFOs complying and receiving payments, all of the payments would have been inframarginal to conservation. This hypothetical scenario underscores that a key to the program's large positive impact on tree cover was that there was not selective enrollment by PFOs who would have conserved their forest anyway.

Assessing leakage and spillovers

We next assessed if PFOs simply shifted their deforestation to other land. The results discussed above are net of several forms of such leakage. First, if PFOs conserved trees on the land that CSWCT classified as forest, which was regulated under the PES contract, but cut down trees on other parts of their land or others' land in the village, the results measure the net effect because they examine the entire village, not just primary forest owned by PFOs. Second, there could be within-village leakage from one PFO to another. For example, two PFOs might agree that one will enroll, the other will not, and they will do their joint tree-clearing on the nonenrollee's land. The village-level results also account for this type of leakage because the analysis examines effects on all PFOs, not just enrollees. Third, the fact that the land circles we examined are larger than the land owned means that the PFO-level results account for increased tree-clearing on nearby land even if that land fell outside the village.

PFOs who lived near government forest reserves could engage in an additional form of leakage, namely, illegally taking trees from the forest reserves. We did not find evidence that treatment effects were larger in villages closer to forest reserves (table S10).

We also examined spillover effects of the program to control villages, which was another possible form of leakage and would have biased the results. It seems unlikely that treatment group PFOs would seek trees from the control group, given the more convenient option of procuring trees from nonenrollees within their village. More plausible is that control villages faced higher demand from timber dealers because supply dried up in treatment villages. Thus, we examined whether there was greater tree loss in control villages that were closer to treatment villages and found no evidence of such spillovers (table S10). This comparison across control villages still left open the possibility of increased tree-cutting in all control villages. The study region feeds into national timber and charcoal markets, and the PES program was relatively small, so the program seems unlikely to have had large general equilibrium effects, although such effects would likely be pertinent if the program were scaled up. The endline survey asked about visits by timber dealers, and there was no increase in visits by timber dealers in control villages relative to treatment villages (*25*).

Analyzing deforestation in the study region only allowed us to assess some forms of leakage. The gains in forest cover in the study region might have been offset by increases in deforestation outside the region. The premise of PES programs is that demand for timber products is at least somewhat elastic, so program effects will not just be undone elsewhere; people will reduce consumption or substitute toward options with a lower carbon footprint. For example, if PFOs cleared less forest for subsistence farming, their demand for food might be met by more efficient agricultural producers.

Household survey results

Results for outcomes measured in the household survey are reported in Table 4. Self-reported tree-cutting was lower among PFOs in the treatment group than in the control group (column 1) (*26*). In addition, the program caused PFOs to increase how much they patrol their land and to curtail others' access to their land to gather firewood: Complying with the program seems to have required not only reducing one's own tree-cutting but also ensuring that others do not clear trees on one's land. Treatment PFOs did not respond to the program by fencing their land, however. Note that a reduction in others' right to gather firewood and access PFOs' land was very likely a regressive effect because landowners often let poorer neighbors gather firewood or cut down very small trees for building material. Thus, the program could have had a negative impact on non-owners of forest, unless PFOs found some other way to make transfers to their poorer neighbors.

The program could have either increased or decreased PFOs' short-run income, depending on how the PES payments compared with their forgone income from clearing trees. Any increase

Table 3. Effect of the PES program on tree cover. All regressions and means are weighted by the proportion of available tree-classification data for the observation. All columns include subcounty fixed effects and the four village-level baseline variables used to balance the randomization. Columns 2, 3, 5, and 6 also control for dummy variables for the date of the baseline satellite image, and columns 2 and 3 control for 1990 and 2010 area covered by photosynthetic vegetation within the village polygon and in aggregate in PFO land circles for the village; columns 5 and 6 control for 1990 and 2010 area covered by photosynthetic vegetation within the village polygon and in the PFO's land circle. Standard errors are heteroskedasticity-robust in columns 1 to 3 and clustered by village in columns 4 to 6. Significance: *$P < 0.10$, **$P < 0.05$, ***$P < 0.01$.

	Village boundaries			PFO-level land circles		
	ΔTree cover (ha)	ΔTree cover (ha)	ΔLog of tree cover	ΔTree cover (ha)	ΔTree cover (ha)	ΔIHS of tree cover
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment group	5.549*	5.478**	0.0521**	0.245**	0.267**	0.0447*
	[2.888]	[2.652]	[0.021]	[0.110]	[0.106]	[0.023]
Control group	–13.371	–13.371	–0.095	–0.349	–0.349	–0.073
Control variables	No	Yes	Yes	No	Yes	Yes
Observations	121	121	121	995	995	995

in income was unlikely to be large because the payment levels were chosen to be of the same order of magnitude as the monetary opportunity costs. Any decrease was also likely to be small: The program was voluntary, and, although PFOs might have chosen a reduction in current income in exchange for wealth accumulation in the form of intact forest, credit constraints and impatience likely limited such behavior. We examined expenditures as a proxy for income and did not find strong evidence that it either increased or decreased, although there was weak evidence that nonfood expenditures may have increased.

Cost-benefit analysis

We next converted the program's effect on tree cover into delayed CO_2 emissions, calculated the monetary value of the delayed emissions, and compared this benefit to the program costs. (See SM section 4 for further detail.)

The program averted 0.326 ha of deforestation per eligible PFO. Here we used the estimated village-level effect on tree cover from column 2 of Table 3, converted it into the effect per eligible PFO, reduced the magnitude by 10% to account for the possibility of local leakage that we did not detect, and subtracted 0.005 ha as due to reforestation (*27*). The average carbon per hectare of forest cover in the study villages was 153.5 metric tons (MT) (*28*). The carbon stored under other forms of land use, including agriculture, is negligible compared with that from trees, so this amount also represents the change in carbon stocks from tree-clearing (*29*). To be conservative, we ignored the flow of carbon that trees absorb and assumed that the forest was close to carbon-neutral if it remains intact. A CO_2 molecule is 3.67 times as heavy as a carbon atom. Thus, averted CO_2 per eligible PFO is 183.5 MT.

The average payment per PFO is $37.80 over the 2 years of the program. Thus, the program paid $0.20 to delay each MT of CO_2 emissions. We treated this full amount as a program cost, but note that payments in excess of the amount needed to gain compliance are a transfer, not a true economic cost. There are also costs of monitoring enrollees' forests and marketing and administering the program, plus financial transaction costs to pay PFOs. We estimated that these costs were $0.26 per averted MT of CO_2. Thus, our best estimate of total program costs—incentive payments to forest owners plus program administration costs—at scale-up is $0.46 per averted MT of CO_2.

The amount paid to avert CO_2 emissions can be compared with CO_2 benefits, valued using the social cost of carbon (SCC). The middle estimate used by the U.S. Environmental Protection Agency (EPA) for 2012 is an SCC of $39 per MT of averted CO_2 (in 2012 USD). The EPA averages the SCC across three integrated assessment models; its middle estimate uses a discount rate of 3%. The SCC represents the benefit of permanently averting CO_2 emissions. The program we evaluated is a prototype of what could be a permanent program, but it was in place for only 2 years. The effects we estimated, thus, represented a delay in CO_2 emissions. To quantify the delay length, we needed to make assumptions about deforestation after the program ended, which we did not have direct data on. The base case scenario assumed that PFOs will deforest at a 50% higher rate than the control group after the program ends until they catch up on all their postponed deforestation, thus undoing the 2 years' worth of treatment effects over 4 years. The average delay in deforestation in this scenario is 3 years, if one assumes that the status-quo rate of deforestation is uniform over time. We also considered two more extreme scenarios. First, we supposed that PFOs would catch up on all their delayed deforestation immediately when the program ends, in which case the treatment effects represent a 1-year delay in deforestation. Second, we supposed that PFOs would resume their normal rate of deforestation after the program ended, rather than an accelerated rate; this is equivalent to a permanent 2-year delay in both the deforestation that would have occurred during the intervention and all later deforestation.

We assumed that the average lag between when trees are cleared and the carbon is emitted into the atmosphere is 10 years (*30*). Finally, the value of delayed CO_2 emissions depends on the discount rate and the growth rate of the SCC. We used the EPA's median assumptions of 3% for the discount rate and 1.9% for the growth of the SCC (*31*, *32*).

Combining these assumptions, the discounted benefit of delaying a MT of CO_2 through the program is $1.11 in the base case. This benefit is 2.4 times as large as the program costs. If, instead, PFOs caught up on their backlog of avoided deforestation the moment the program ended, the benefit-cost ratio would fall to 0.8. If PFOs resumed deforesting at their normal rate after the program ended, not at an accelerated rate, then the benefit-cost ratio rises to 14.8. This last scenario of a permanent 2-year delay in deforestation is the relevant one for extrapolating to a longer-lasting program: If the program effects we observed were to persist with a permanent program, the net present cost to permanently avert a MT of CO_2 would be $2.60, much less than the SCC. Note that this was a very tentative extrapolation.

The cost-benefit analysis for several scenarios is summarized in Table 5. Under most assumptions, the social benefit of the delayed CO_2 emissions exceeds the program costs. It is important to note that the cost-benefit numbers have a wide confidence interval due to estimation error in our

Table 4. Effects of the PES program on secondary outcomes. All columns include subcounty fixed effects and the four village-level baseline variables used to balance the randomization. Columns 1, 4, 5, and 6 control for the baseline value of the outcome. Baseline data on the outcomes in columns 2 and 3 were not collected. IHS denotes inverse hyperbolic sine. For observations where the baseline outcome is missing, the value is imputed as the sample mean, and the regression includes an indicator variable for whether the baseline value is imputed. Standard errors are clustered by village. Significance: *P < 0.10, **P < 0.05, ***P < 0.01.

	Cut any trees in the past year	Allow others to gather firewood from own forest	Increased patrolling of the forest in last 2 years	Has any fence around land with natural forest	IHS of food expend. in past 30 days	IHS of nonfood expend. in past 30 days
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment group	−0.140***	−0.170***	0.109***	0.036	0.065	0.156**
	[0.034]	[0.033]	[0.039]	[0.033]	[0.074]	[0.066]
Lee bound (lower)	−0.161***	−0.185***	0.094**	0.007	−0.029	0.053
	[0.034]	[0.033]	[0.039]	[0.033]	[0.070]	[0.064]
Lee bound (upper)	−0.104***	−0.148***	0.132***	0.055	0.144*	0.215***
	[0.033]	[0.032]	[0.039]	[0.034]	[0.075]	[0.064]
Control group mean	0.453	0.427	0.378	0.667	2.524	4.363
Control group SD	[0.498]	[0.495]	[0.485]	[0.472]	[1.177]	[1.354]
Observations	1018	9767	984	1020	1020	1020
Observations (Lee bounds)	994	957	965	998	998	998

statistical analysis, the need to make several assumptions, and the large uncertainty in the SCC, which is derived from highly complex models of climate and the economy. Another way to benchmark the program is against other environmental programs. The PES program is less expensive per MT of averted CO_2 than many policies in the United States for which similar cost-benefit analyses are available, such as hybrid and electric car subsidies; those policies cost 4 to 24 times the CO_2 benefits they generate (*33*, *34*). Note that there could be ways to reduce CO_2 more cost-effectively than PES, but for which we lack rigorous impact evaluations and cost-effectiveness analyses.

The cost-benefit analysis does not incorporate behavioral responses besides tree-cutting in the study area, such as how charcoal consumption in urban areas responds and what fuel sources consumers substitute toward. Analyzing these consumption responses is highly relevant to the ultimate impact on carbon emissions but is beyond the scope of this study. In addition, whereas our analysis does not detect general equilibrium effects, such effects could be important if PES programs were implemented at large scale.

Beyond averted CO_2, there are also other benefits of the program. For example, the program redistributes from the wealthy to the poor. Although PFOs are not poor relative to their neighbors, they are poor in global terms. Another benefit that we cannot quantify with our study design is increased biodiversity. On the opposite side of the ledger, the program caused PFOs to curtail their neighbors' access to their land to gather firewood. A do-no-harm version of PES might want to include small cash transfers to poor, non–forest-owning individuals in the community.

Discussion

The main contribution of our analysis is to provide rigorous evidence on an important policy to mitigate climate change, which is one of the most (if not the most) significant environmental challenges we face. Scholars have noted the need for high-quality evaluations of PES (*35*, *36*). The most widely studied PES program is Costa Rica's Pago por Servicios Ambientales (PSA). A review of the literature on PSA concluded that "studies give widely divergent results" (*37*). Several case studies report large, positive impacts on forest cover but likely suffer from omitted variable bias. Quasi-experimental studies of PSA that use covariates to match enrollees and nonenrollees generally find smaller effects, with the specific effects varying based on the cohorts and regions analyzed (*38*, *39*). Mexico's PES program has also been studied with quasi-experimental methods (*40*–*42*). Our study, by randomly assigning who is eligible for the PES program, improves on the existing studies, which are subject to concerns about bias in the estimates due to self-selection into the program or targeting by program administrators based on unobservables.

This study also adds to the literature on PES in four other ways. First, we use satellite images with very high resolution, which enables us to detect selective tree-cutting in addition to clear-cutting. In many parts of the world, thinning of forests leads to significant forest loss. Second, we study a region with a high rate of deforestation and forest degradation; a recent literature review of forest-related PES concluded that "the places in which avoided deforestation has been measured tend to have low deforestation rates," and "analyses from countries where deforestation risk is high but institutional strength is low [are] essential" (*43*). Third, we investigate and address leakage more thoroughly than most previous studies (*44*). Finally, our cost-benefit analysis allows policymakers to assess the cost-effectiveness of the PES program in comparison to other options for reducing global carbon emissions.

Table 5. Cost-benefit analysis. The costs of the PES program compared with the social benefit of delayed CO_2, both measured per MT of averted CO_2. The base case assumes (i) an average 3-year delay in deforestation (treatment effects undone over 4 years), (ii) no further treatment effects during the 0.5 years between endline QuickBird data collection and program end, (iii) average time from tree-cutting to CO_2 emissions of 10 years, and (iv) a monitoring rate of 2 spot checks per monitor per day. Row 2 modifies (i) to assume a 1-year delay in deforestation (treatment effects undone immediately when the program ends). Row 3 modifies (i) to assume the averted deforestation and all subsequent deforestation are delayed by the 2-year duration of the program. Row 4 maintains the base case assumptions but uses the treatment effect estimated using PFO land circles instead of village boundaries. Row 5 modifies (ii) to assume the treatment effects accumulate at the same rate in the final months as we observe in the period before endline data collection. Rows 6 and 7 modify (iii) to shorten and lengthen the gap between tree-cutting and emissions. Row 8 modifies (iv) to assume one spot check per monitor per day. See SM section 4 for further details.

Scenario	Benefit per MT of CO_2 ($)	Cost per MT of CO_2 ($)	Benefit-cost ratio
1. Base case: Program effects undone over 4 years	1.11	0.46	2.4
2. Program effects undone immediately	0.37	0.46	0.8
3. Deforestation resumes at normal rate (permanent delay)	0.74	0.05	14.8
4. Base case except using effect size from PFO-level analysis	1.11	0.63	1.8
5. Program effects accumulate for final 6 months	1.11	0.34	3.2
6. Average time until emissions is halved to 5 years	1.17	0.46	2.6
7. Average time until emissions doubled to 20 years	1.00	0.46	2.2
8. Monitoring rate of 1 spot check per day per staff person	1.11	0.53	2.1

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9. The PES program was funded by the Global Environment Facility, through the United Nations Environment Programme, via a grant to the Ugandan government, which subcontracted the design and implementation of the program to CSWCT.
10. PFOs could not enroll after the initial enrollment period, and once they deforested in violation of the contract, they were no longer eligible for payments.
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18. Five PFOs in control villages enrolled; they resided in control villages but owned forest in treatment villages. Sampling and assignment to treatment were based on the village of residence, but CSWCT allowed a PFO to enroll any forest located in a treatment village.
19. CSWCT data on monitoring and payments are missing for four enrollees.

20. CSWCT collected the signed contracts on a particular day in each village, and if a PFO was unaware of this process or absent that day, they missed, or at least thought they had missed, their chance to enroll.
21. In focus group discussions we conducted with a nonrepresentative sample of 49 nonenrollees in 10 treatment villages, fear of losing one's land to CSWCT and preferring to cut down one's forest for money were the most common reasons cited for not enrolling.
22. The unreported coefficient on 2010 vegetation is positive, and the coefficient on 1990 vegetation is very similar in magnitude and negative, both with $P < 0.01$. These patterns indicate that, first, the Landsat vegetation variable provides additional information on baseline tree cover, and, second, vegetation loss in the two decades before the study is predictive of tree cover loss over the study period. We have, on average, 9.1 PFOs per village in the sample, but not all PFOs are in the sample. Sixty-three percent of program enrollees are in the sample, which is equivalent to 14.9 PFOs per village given an enrollment rate of 32%.
23. The endline survey data indicate that some nonenrollees thought that they were enrolled in the program. They might have avoided deforestation based on an incorrect belief that they would be paid if they did so. These PFOs might have misunderstood the procedure to enroll, or CSWCT might have made a mistake in not registering their contract, or the endline data might have mismeasured their beliefs about their enrollment status. These PFOs did not receive any monitoring or payments according to CSWCT. Self-reported enrollment in the endline survey was 5 percentage points higher than official enrollment.
24. On average, 0.1 ha were set aside for reforestation per treatment group PFO, with 10 surviving trees per PFO. Seedlings would not have grown large in 2 years, but if each had a crown area of 5 m^2 (likely an overestimate), then reforestation would explain 0.005 ha of the treatment effect on tree cover.
25. Deforestation is not significantly correlated with a PFO's expectations about the program's future (table S10). PFOs were informed that the program would last for 2 years.
26. Attrition from the endline survey was higher in the treatment group than in the control group; in particular, treatment group PFOs who did not take up the program were less likely to participate in the survey, which likely biases upward the magnitude of the treatment effects. We therefore present Lee bounds on the treatment effects (*46*). Lee bounds make a monotonicity assumption about how treatment status affects selective attrition.
27. The average duration from baseline to endline was 1.5 years, but the payment data we use are for 2 years of compliance. To be conservative, we ignore that the amount of averted deforestation likely accumulated during the remaining half year of the program.

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SUPPLEMENTARY MATERIALS

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Materials and Methods
Figs. S1 to S5
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Science

Cash for carbon: A randomized trial of payments for ecosystem services to reduce deforestation

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Ecosystem protection payments pay off

Trees take up a lot of CO_2, so one approach to reducing the rate of increase in atmospheric CO_2 levels is to reduce the cutting down of trees. Jayachandran *et al.* evaluated a program in which forest owners in Uganda were paid to not cut down their trees. Encouragingly, payments did reduce deforestation, and owners did not compensate by cutting down trees in neighboring forests. Furthermore, even in a scenario where cutting resumed after payments ceased, the beneficial delay in CO_2 release from cut trees, as quantified by the social cost of carbon, outweighed the monetary cost.

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