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Effects of Exclusion from a Conservation Policy: Negative Behavioral Spillovers from Targeted Incentives

Francisco Alpízar*
Anna Nordén*, ‡, §
Alexander Pfaff††
Juan Robalino*

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*Environment for Development Center for Central America, CATIE, 7170 Turrialba, Costa Rica; Phone +506 2558-2215; Fax +506 2558-2625; falpizar@catie.ac.cr; robalino@catie.ac.cr
‡Department of Economics, University of Gothenburg, Sweden; anna.norden@economics.gu.se
§Department of Physical Geography and Ecosystem Science at Lund University
††Duke University, Sanford School of Public Policy, Durham, NC 27708 USA; alex.pfaff@duke.edu

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negative behavioral spillovers from targeted incentives

Abstract

A critical issue in the design of incentive mechanisms is the choice of whom to target. For forests, the leading schemes: [i] target locations with high ecosystem-service density; [ii] target additionality, i.e., locations where conservation would not occur without the incentive; or, at least effectively, [iii] reward previous private choices to conserve forest. We use a field experiment to examine the changes in contributions to forest conservation when we introduce each of those three selection rules. For individuals who are selected, we find that targeting additionality (rule [ii]) is the only scheme to increase contributions. However that selection rule intentionally excludes those who contributed most previously and it is the only one to generate significant 'behavioral leakage', i.e., negative spillovers or falling contributions by those who are excluded (and face no price or income changes). Our results demonstrate a tradeoff in targeting and a challenge for optimal policy design.

Keywords

incentives, payment for ecosystem services (PES), targeting,
spillovers, behavioral economics, field experiment

1. Introduction

Forests provide many ecosystem services − sequestering carbon, providing species habitat and supporting watershed function. Concern about forest losses (deforestation and forest degradation) has led communities, governments, and international organizations to try harder to protect forests (Barton et al. 2009, Klemick 2011). Among the intervention options, compensation that alters the relative price of land uses, such as payment for ecosystem services (PES), has increased in both theoretical and practical popularity over the last decade (Pattanayak et al. 2010, Ferraro 2011).

Any compensation mechanism requires a decision concerning whom to target for payment. For instance, who should get paid to reduce emissions from forest land arises in global discussion of climate-change mitigation. Representatives of locations featuring low rates of deforestation have suggested paying for all services generated by standing forest. In contrast, an additionality or impact focus suggests paying only when blocking deforestation risks, i.e., increasing services (see, e.g., Pfaff and Sanchez 2004, Muñoz et al. 2008, Wünscher et al. 2008, Pfaff et al. 2009). Others have focused on co-benefits, such as protecting species habitat or groups' livelihoods, and suggest targeting species or group locations − often without explicit comment on additionality.

From global efforts to provide incentives for REDD (Reduced Emissions from Deforestation and forest Degradation) down to local watershed initiatives, such targeting has consequences for efficiency and perceived fairness. We examine whether perceived fairness also affects efficiency. For instance, without fairness considerations it may seem inefficient to pay for all standing forest including areas with low deforestation rates; globally, an application of this rationale could be the exclusion from new REDD incentives of a country like Costa Rica, which halted net forest loss. However, while there may well be an efficiency gain from paying only sites with deforestation, it could be offset if upon exclusion those who voluntarily restricted deforestation stopped doing so.

Such shifts in motivation are our focus in examining what we will call 'behavioral leakage'. We distinguish that from standard 'leakage' − an often expressed concern − where the payments that shift land uses' relative prices drive changes in behavior which, in turn, shift other prices. For instance, payments can lower local labor demand or crop supply and thus shift wages and prices. Our experimental approach lets us focus upon motivations by eliminating such price changes.

Within the realm of motivations, we distinguish exclusion from important literatures which consider individuals' reactions when they are faced with a new incentive (see more in Section 2). Behavioral economics considers 'motivation crowding': shifts in motivations and thus decisions when new incentives are introduced. Another literature, including in psychology, considers shifts when incentives are first introduced and then removed. Neither literature considers the situation we study, in which people may react to *not receiving* a newly established incentive, i.e., they may react despite unchanged prices and incomes. We study exclusion from a targeted forest incentive.

To examine whether exclusion has impacts, and whether the rationale for exclusion matters, we employ a field experiment that for the participants was framed and functioned as an actual intervention to solicit contributions that support forest conservation. We examine changes in contributions, by selected and excluded individuals, when we introduce each of three selection rules for targeting an incentive. Specifically, we conducted a survey of Costa Rican landowners concerning their land uses and agricultural practices. Participants were paid for their time. After being paid, participants were asked if they would like to contribute to forest conservation by making a monetary contribution to a governmental program called Bosque Vivo, a public program that acts to conserve forest ecosystems in Costa Rica using funds from private donors. Each participant was paid twice during the survey, never having known money was forthcoming. Each person faced the same decision in our first round, and then we randomized selection rules.

Specifically, in our second round of paying for their time and then soliciting a contribution we randomly assigned four conditions: [i] 'environmental benefit'[1] or incentive offered to those in locations judged to have highest potential gains for Bosque Vivo, for reasons that we explained; [ii] 'additionality' or incentive offered to those with initial contributions below a low threshold; [iii] 'reward' or incentive offered to those with initial contributions above a high threshold; and [iv] 'control' or no incentive, to observe how contributions change across rounds without policy. To isolate the effects of our treatments, we employ a difference-in-difference (DiD) approach that compares the change in behavior over rounds for each selection rule to the change in the control.

For additionality and for behavioral leakage, we find significant effects for only one rule. Further, it was the same rule [ii], i.e., the focus on additionality or impact, that had both impacts[2]: for those selected to face the incentive, only this rule increased contributions relative to control; and for those excluded from incentives, only this rule decreased contributions relative to control.

Given no price change for the excluded, the latter result is suggestive of 'behavioral leakage' or a negative shift in the motivations of those who felt they were making pro-social contributions (recall, targeting of additionality explicitly excludes those who previously had contributed a lot). In total, our results indicate a tradeoff within the optimal designs for targeted incentives, while providing support for arguments on both sides of ongoing debates about targeting additionality.

The rest of the paper is organized as follows. Section 2 provides some additional description of literature on leakage and motivations. Section 3 describes the design of our field experiment. Section 4 presents our results and then Section 5 summarizes and discusses some implications.

[1] The labels that we provide here for these rules are simple characterizations that we feel are descriptive. We did not use them with the participants so as not to generate any signals about expected behavior (script available on request).

[2] Consistent with indications from a lab experiment with students at University of Costa Rica (Alpízar et al. 2013).

2. Relevant Literature

2.1 Selection in Incentive Programs

Two well-known national programs to pay for ecosystem services are in Costa Rica and Mexico. Targeting, and more specifically the lack of targeting of additionality[3], has been raised for both. That is not surprising, given that not all policymakers are in agreement regarding the objectives for guiding any efforts at targeting. There are various approaches that could guide enrollment.

A common top-down approach is to target land with the highest ecosystem-service benefits for society per unit of standing forest (Wu and Babcock 1996, Smith and Shogren 2002), such as land with highly valued species or land near rivers upstream of cities. For any given level of additionality, this clearly makes sense. The latter criterion has been used in Mexico, for instance, at a relatively rough resolution. However, if there were no additionality, this targeting would not raise service provision. It could lead away from additionality for, e.g., species in isolated forests.

A common bottom-up approach is voluntary enrollment, for instance 'first come, first served' as utilized, to first order, for early payments in Costa Rica. Those who do not plan to deforest in any case due to poor returns on highly sloped lands, e.g., are likely to enroll disproportionately. This 'targets' forest facing low clearing risk (Stoneham et al. 2003, Goeschl and Lin 2004, Ferraro 2008) and effectively makes numerous transfers that 'reward' those with standing forest even if it was their own preferred land use − although it is important to consider that the reasons for such a preference could include not only poor returns but also a desire to contribute to the public good. In any case, such a plan is likely to yield a low impact of payments, since no new deforestation is halted due to payments (Pfaff et al. 2007; Alix-Garcia et al. 2012, Robalino and Pfaff 2013).

[3] Pfaff et al. (2007), Alix-Garcia et al. (2012) and Robalino and Pfaff (2013) show that most of the parcels that have been selected for payments in Costa Rica and Mexico were going to remain forested even without those payments.

Given such results and a desire for impact, a recommendation to increase impact has been to target threat, i.e., make an effort to enroll land that faces a threat of being deforested or degraded (Pfaff and Sanchez-Azofeifa 2004, Muñoz-Piña et al. 2008, Wünscher et al. 2008). By design, this focus on impact or additionality excludes from payment those who have conserved the forest. That makes sense if the force driving private forest conservation is a lack of profit in agriculture. But if a significant driver of private forest is a pro-social desire to contribute to public goods, such exclusion may backfire: excluded individuals might feel they are being punished, instead of appropriately rewarded for acting pro-socially in the absence of the monetary incentive program; and they may react to any incentive program that excludes them by no longer maintaining forest.

The following excerpts from a recent discussion in *RESECON*[4] illustrate this concern. In talking about the recommendation to increase additionality via PES design, one expert argues:

> *"A conscientious landowner whose land is in permanent woods and/or pasture land (i.e. not being logged nor plowed) doesn't get any incentive in the first place although he is contributing a positive externality. Because we (the society) expect him to do so?"*

and another says:

> *"I have to admit that my issue with this design feature is not really (or mostly) a straightforward economic one. Rather, it doesn't seem right/fair to me that someone whose practices are damaging gets paid to change his ways, when someone who has sacrificed his own gain to make his operation more environmentally benign is just taken for granted. If payments are one-off incentives to make permanent changes to practices, I have somewhat less of a problem, but a long-term flow of payments to reward the changes is a slap to those who have made these changes on their own initiative."* [5]

These comments highlight simply that economists considering such issues recognize a possible tradeoff between impact on selected forest land and fairness to those who have not been selected (see also, for instance, Wu et al. 2001 for consideration of the distributional impacts of targeting). We examine whether perceived unfairness can affect net impact by shifting forest motivations.

[4] RESECON, Land and Resource Economics Network, is an environmental-economics listserve (resecon.org).
[5] RESECON 2013/01/24 New Forest Trends' Ecosystem Marketplace Report "State of Watershed Payments 2012".

2.2 Shifting Motivations

2.2.1 Incentives Shifting Motivations

In psychology, many have examined the effects on behavior when an incentive for an action is first introduced and then later removed (see a comprehensive review in Deci et al. 1999). Based on this, although not necessarily requiring the removal of the incentive, in behavioral economics 'motivation crowding' has been identified as a potential issue in a range of settings. It is a change in how an individual frames a specific decision, and a change which could shift such a decision. Specifically, a shift in the decision framing, or the motivations brought to bear upon the decision, results from the introduction of a policy such as a public incentive for something that was purely private. One widely cited example (Gneezy and Rustichini 2000) finds a fall in compliance with rules for on-time daycare pickup when a fine for lateness is created (an environmental analog in Colombia is provided by Cardenas et al. 2000). Contributions to a public good also are seen to change when incentives are introduced (related theory in Frey 1994, Frey and Jegen 2001 and further related empirics in Frey and Oberholzer-Gee 1997 and Mellström and Johannesson 2008).

Neither of those literatures applies to our situation in which people react to *not receiving* an incentive. A strand of literature that could suggest impacts on those *excluded* from the incentive concerns the desire for social approval (for economic models include Akerlof 1980, Hollander 1990, Bénabou and Tirole 2006, Ellingsen and Johannesson 2008, Andreoni and Bernheim 2009). If private forest conservation, e.g., were driven partly by the desire to be perceived by others as environmentally friendly, conservation payments or 'bribes' could spoil the clarity of private signals (Ariely et al. 2009) and those not selected to the program would not receive a payment that could overcome a reduction in signaling value. They might, then, lower their conservation.

2.2.2 Fairness & Motivations

Behavioral-and-experimental economics also provides alternative potential explanations for reactions to being *excluded* from a policy such as within the 'behavioral leakage' that is our focus. One well-established empirical result is that people treat nicely those who treated them fairly but wish to punish those who did not (Rabin 1993). This fits well with documentation of reciprocity (see, for instance, Kahneman et al. 1986, Fehr and Gächter 2000, Falk and Fischbacher 2006). Fairness preferences are also suggested by (relatively) equal division of resources in games and costly punishment for proposing unequal divisions (Fehr and Schmidt 2006, Dawes et al. 2007).

For our case, those excluded from a forest incentive might feel they were not treated fairly, perhaps in particular if they were excluded because of behaviors they felt were publicly minded. How might offended agents punish any unfairness? If an unfair rule concerns forest conservation, then agents affected by it might feel they can punish by reducing their forest conservation efforts.

3. Field Experiment

Our experiment is like a Dictator Game where a player, the dictator, is given a sum of money to allocate between himself and another player, the receiver (Kahneman et al. 1986, Forsythe et al. 1994, Hoffman et al. 1996). In our experiment the recipient is a charity organization, following Eckel and Grossman (2003) and Carpenter et al. (2008). In our field experiment, subjects consented to participate in a survey.[6] Face-to-face surveys are commonly used in Costa Rica, including in the national census, and participants did not know this survey had an experimental component. Thus ours is a natural field experiment in the language of Harrison and List (2004).

[6] List 2008 discusses informed consent in the social sciences. We did not ask for consent to be in an experiment, as we wanted to observe behavior in as realistic as setting as possible. The experiment certainly met all ethical norms: anonymity; no additional risk; and while no direct research benefits, direct payments and ideally societal benefit.

The survey was an hour-long questionnaire about land use and socioeconomic characteristics of landowners in Costa Rica. About half an hour into it, enumerators announced a short break and subjects were informed about and received a payment in appreciation of their time and effort.[7] Then subjects were invited to consider a voluntary, anonymous contribution to a public program called Bosque Vivo that conserves forest ecosystems in Costa Rica using private contributions. Its purpose and nature was described in the survey. Subjects were given private time to decide how much to contribute using the money they had just received. The subject marked the amount contributed on an envelope with only the survey number as identifier, then put the contribution in the envelope and sealed it.[8] Subjects could only use the payment just received, so the maximum amount that could be contributed each time was 5,000 colones, in multiples of 500 colones.

The survey recommenced, ending half an hour later when a second payment was announced and second invitation to contribute issued. Subjects were not aware of the second payment when they made their first contribution. Each of the two payments amounted to 5,000 colones (10 USD) − and each was paid in notes and coins in order to allow for flexibility in contribution in multiples of 500 colones (1 USD). The payments were given to the subjects in sealed envelopes. As a reference, a day of farm labor would earn workers an average of 10,000 colones (20 USD).[9]

Our first round of contributions provides baseline pro-environmental behavior in the absence of any incentive.[10] Our second round introduced the treatments, i.e., the incentive to contribute

[7] Even though the payment was framed as money paid for time and effort, subjects are not expected to act as if this money was earned since participation in surveys is seldom paid for in Costa Rica. Some studies have found that earned money leads to lower contributions compared to windfall money (e.g. Cherry et al. 2002, Carlsson et al. 2012). However, since we use a difference-in-difference approach, such effect has no implication for our results.
[8] Since the enumerator could observe the subjects' contributions, an experimental demand effect might be driving contributions (e.g. Hoffman et al. 1996, Zizzo 2010). However, the DiD approach should take care of such concerns.
[9] We used relatively high stakes but as we use a difference-in-difference approach, stake has no implication for our results (note also that Kocher et al. (2008) did not find a significant stake effect for contributions to public goods).
[10] We tried to move away from "seed money", or matched-funds experiments (List and Lucking-Reiley 2002), and towards PES schemes by framing the incentive as money paid back to the respondent (the framing of incentives for

based on a randomly assigned selection rule. A limited budget was used as justification for using selection rules, i.e., for the fact that we would not be providing the incentive to everyone.[11] To control for the effect of making repeated contributions, which by itself could lead the second round to differ from the first, a subsample received round two without any incentive program − and hence no mention at all of selection rules. We refer to this treatment as 'control' treatment.

Three alternative selection rules were randomly assigned. For two rules, selection drives off the subject's past behavior. In the 'additionality' treatment, subjects who had contributed 1,000 colones or less in the baseline round were selected for the incentive, and the rest were excluded. The purpose of this targeting is to increase contributions from those who on their own give little. In a standard dictator game, average giving is around 20% of the endowment (see Camerer 2003), suggesting that below 20% − which for us is 1,000 of 5,000 colones − is relatively low giving. In the 'reward' treatment, in contrast, subjects who contributed 2,500 colones or more were selected. This rule rewards relatively high prior giving, noting that 2,500 colones is 50% of their payment.

We also wanted to compare selection based on past behavior with selection based solely on exogenous characteristics outside of the landowner's control. Thus, our 'environmental benefits' treatment selected people in locations with high environmental benefit from additional forest. Specifically, we indicated in the script that Bosque Vivo was prioritizing the area of Nicoya and in the script explained that this was due to the region's water quality problems during dry seasons. Given that choice, regardless of their contribution levels in the first round, for this 'benefits rule' we selected to receive the new incentive only the people who were located in the Nicoya area.

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prosocial behavior can matter (Eckel and Grossman 2003)). Subjects contribute whatever amount they choose then the research team paid the subjects chosen by the selection rule half of the contributed amount. For example, if the subject had decided to contribute 1,000 colones to Bosque Vivo in a given ground, that exact amount was put in a sealed envelope addressed to Bosque Vivo, and a new payment of 500 colones was made in favor of the subject.

[11] For the exact wording, the original script in Spanish (and/or the English translation) is available upon request.

Subjects were not aware that others had been selected, or excluded, on a variety of bases. Each learned only about the selection rule applied to her/him, and whether he/she had qualified. In this fashion, then, we interviewed 357 landowners between November 2011 and January 2012. Two databases set the sampling frame: i) all 2011 applications received before May 2011 by the National Forestry Financing Fund (FONAFIFO), the authority responsible for PES in Costa Rica; and ii) a farm census by the Ministry of Livestock and Agriculture (MAG) during 2006 and 2007. We focus on four of Costa Rica's nine regions − namely Limon, Guapiles, San José, and Nicoya − and balance the datasets by including 25% of observations from each region of the FONAFIFO dataset along with about 40 random observations per region from the much larger MAG dataset. Appointments were made by phone before visiting respondents' homes. Most of those who were contacted were interested in participating. As logistics made it impossible to meet the preferences of all landowners regarding date and time, about 50% of all those contacted were interviewed.

4. Results

To identify the effect of exclusion *per se* on the change in contributions across our two rounds, we require a difference-in-differences (DiD) approach (following Ashenfelter and Card 1985). The reason is that contributions could change across rounds for reasons other than introduction of the incentive. For instance, at the point our second payment-and-solicitation round is announced, subjects may feel they already contributed (diminishing altruism), maybe lowering contributions, or feel richer (as at that point they have been paid twice), making the second contribution larger. Alternatively, that subjects had to disclose, by marking the amount contributed on the envelope, might trigger 'experimental demand', i.e., “changes in behavior by experimental subjects due to cues about what constitutes appropriate behavior” (see Zizzo 2010:75 or Hoffman et al. 1996).

Thus, we start with the within-subject change in contributions from Round 1 to Round 2 for each subsample to which a selection rule was applied. We then compare those differences to the change in contributions across rounds observed in the subsample that faced the control treatment, i.e., where nobody was selected for incentives. More specifically, we make the comparison twice for each selection rule, comparing separately to the control the selected and excluded individuals. We are expecting, if anything, rising contributions for the selected but falling for the excluded.

We further refine these comparisons based on our expectation of considerable heterogeneity in making such contribution decisions. That leads us to want to try to compare the same 'type' without and with an incentive. Specifically, those who contribute in the first round what we have effectively defined as a little (20% or less, ≤ 1000 colones) and those who contribute a lot (50% or more, ≥ 2500 colones) may have sufficiently different relevant preferences to be called 'types'. With that in mind, the best controls differ by selection rule, as we want controls to be the same type as those excluded or selected under the selection rules. For instance, to compare with those excluded under the 'additionality' rule, i.e., those who contributed over 1,000 in the first round, from among all controls we will compare to those who contributed over 1,000 in the first round.

4.1 Effects of Exclusion

The top half of Table 1 shows average contributions by round for those excluded by each rule, in the first three columns. Contributions by those excluded fell significantly for the Additionality rule which excludes the high contributors in Round 1, but not for the Reward rule which excludes the low contributors in Round 1. They fell also for the Benefits rules, which excludes by region; in Round 1, those excluded had contributed almost as much as those excluded by Additionality. However, recall that contributions may fall in Round 2 just because of prior giving in Round 1.

The bottom half of Table 1 confirms that without any selection contributions do indeed fall and, further, how much they fall seems to depend on Round 1 contributions. When focusing upon control-group individuals who gave a lot in Round 1, to have groups similar to those excluded by the Additionality and Benefits rules[12], we see that contributions fell significantly across rounds. However, when focusing on low Round 1 contributors similar to those who were excluded under the Reward rule, not only do the control contributions not fall significantly but in fact they rise.[13]

Comparing the changes in contributions across rounds for the excluded groups, for each rule, to changes in contributions across rounds for their similar control groups (i.e., using DiD) yields a simple first estimate of the effects of exclusion.[14] Only for Additionality is the effect significant, with exclusion of high Round 1 contributors − on the basis of their high Round 1 contributions − lowering average contributions by 456 colones more than occurs for the controls who gave a lot in Round 1 (one-tailed t-test p-value=0.07[15]). Being excluded by the Reward rule, i.e., excluded for low Round 1 contribution, does not have negative spillovers (one-tailed t-test p-value=0.30). Neither does being excluded by region based on density of environmental benefits, with the drop in contributions for the excluded being fairly close to the controls (one-tailed t-test p-value=0.38).

It is notable that despite having excluded a group with relatively high Round 1 contributions, Benefit's exclusion based just on exogenous characteristics − not Round 1 contribution choice − appears to have less of an effect in discouraging Round 2 contributions than does Additionality.

[12] Comparing the Round 1 contributions levels for all columns inTable 1, there are no significant differences between the excluded and similar controls (two-tailed t-test additionality rule p-value=0.8925; reward rule p-value=0.7160; environmental benefits rule p-value=0.7659; environmental benefits with initial contribution>1000 p-value=0.8925)
[13] Leaving magnitude aside, the share of subjects reacting negatively upon exclusion based upon the reward rule is significantly smaller than under the additionality and environmental benefits rules (chi2 t-test p-value<0.01 reward rule compared with additionality rule and p-value=0.03 for reward rule compared with environmental benefits rule).
[14] Non-parametric tests (i.e., Wilcoxon test) are used when making within-subject comparisons, while between-subjects comparisons are tested using parametric statistical tests (t-test). However, to account for the small sample size, we also run non-parametric tests (i.e., Mann-Whitney test) and find similar results to those discussed here.
[15] One-tailed t-tests are used to test our hypothesis of possible negative behavioral spillovers upon exclusion.

Table 1's fourth column tightens that comparison a bit further by considering a subset of those in the third column, i.e., those excluded based on region who also gave over 1,000 in Round 1. That group is arguably a bit more similar, in terms of 'type', to those excluded under Additionality. Thus, the lack of a significant reduction in average contribution (one-tailed t-test p-value=0.30) comparing Benefit-excluded individuals to similar controls suggests that the reason for a greater effect under Additionality is *the reasoning provided for having excluded the higher contributors*: excluding them because they contributed more differs from excluding them based just on region.

Table 2 extends Table 1 with more attention to controlling for Round 1 contribution levels. The first column for each rule includes Round 1 contributions linearly as an independent variable explaining the change in contributions across rounds, while the second column for each rule uses dummy variables for Round 1 contribution levels, as one way to control in a non-linear fashion. For high-initial-contributions groups (columns 1,3,4), Round 1 contribution is significant, while for low initial contributions excluded by the Reward rule, Round 1 contributions do not matter.

Table 2 confirms the findings within Table 1 concerning the effects of exclusion. Again, only for Additionality do we find a significant effect. All coefficients are negative, just as in Table 1, and indeed the magnitudes of the coefficients are quite similar to the final row within Table 1. As noted, among higher initial contributors the higher the Round 1 contribution the greater the fall in contributions across rounds. Further, the coefficient for the effects of exclusion by Benefits is larger when we employ a filter of initial contributions over 1,000 to match the range excluded by Additionality. However, the estimated exclusion effect is still smaller than for Additionality and is not statistically different from zero, while the estimated Additionality effect is significant. Again, this suggests that the effect is due not only to Additionality excluding higher contributors but also to the effect on motivations of being excluded based expressly on those contributions.

4.2 Effects of Selection

The top half of Table 3 shows average contributions by round for those selected under each rule, in its first three columns. For those selected under Additionality given low Round 1 contributions, contributions rise considerably. In contrast, those selected under Reward for having contributed a lot in Round 1 lowered their contributions considerably. For selection based on region Benefits, overall there was little change. However, focusing on those who gave little in Round 1 − to study a group more similar to those selected under Additionality − the Benefits contributions increased.

The bottom half of Table 3 confirms that without any selection rule, i.e., for the controls, the level of Round 1 contributions strongly influences the change in contributions across the rounds. When focusing upon individuals who gave little in Round 1, to have controls similar in 'type' to those selected by Additionality[16], we see that contributions if anything rose a little across rounds. However, when focusing on high Round 1 contributors similar to those who were selected under the Reward rule, not only do the control contributions not rise but in fact they fall significantly. For the controls similar to those selected by Benefits, i.e., people in Nicoya, whose contributions in Round 1 are between those selected for Additionality and Reward, contributions fall just a bit.

Comparing the changes in contributions across rounds for the selected groups, for each rule, to changes in contributions across rounds for their similar control groups (i.e., using DiD) yields a simple first estimate of the effects of selection. Only for Additionality is the effect significant (one-tailed t-test p-value=0.04) with an increase in contributions across rounds that is 735 colones greater than for the similar controls. Selections under Reward and Benefit yielded positive effects as well but those effects are not significant (one-tailed t-test p-value=0.28 and 0.30 respectively).

[16] Comparing the Round 1 contributions levels for all columns inTable 1, there are no significant differences between the selected and similar controls (two-tailed t-tests for additionality rule p-value=0.283; reward rule p-value=0.378; environmental benefits rule p-value=0.683; environmental benefits with initial contribution>1000 p-value=0.859).

As in Table 1, the fourth column of Table 3 examines a subset of the Benefits rule based on Round 1 contributions. However for these cases of selection, in contrast to the cases of exclusion, we might well expect the same results from providing the incentive to low Round 1 contributors. The reason is that here we study *low* Round 1 contributors and *selection* − not the same situation where potentially publicly spirited behavior was rejected, with the potential to shift motivations (recall from above that for *rejecting high* contributions, the reason provided appeared to matter). The last row of Table 1's fourth column supports that logic, since the estimated selection effect for the low-initial-contributions subset of those selected under Benefits react as for Additionality.

Table 4 extends Table 3 − as Table 2 did Table 1 − with controls for Round 1 contributions (verifying the significant negative effect of initial contributions upon changes in contributions). Table 4 confirms the findings of Table 3, with no significant effects of selection under either the Reward or the Benefits rules but a significant positive effect of the selection under Additionality. This time, unlike for exclusion, the effect is actually larger when using regressions for controls. Also, the subset − for comparison to Additionality − of those selected under Benefits no longer is significant for regression, given few observations, but the positive coefficients are as in Table 3. In sum, then, selecting low initial contributors for incentives does appear to raise contributions.

4.3 Additionality Rule's Net Effects

Significant effects − falling contributions if excluded and rising contributions if selected − only arose from the Additionality rule. Thus, we consider net effects of incentives for Additionality. Figure 1 shows those effects cancel out, using two versions of the net effects plus associated tests. The upper graphic finds that if one person were selected and one excluded from our incentive, the net effect would be an increase of 279 colones in contributions to forest conservation, but that is

not statistically significant (two-tailed t-test p-value=0.546). This is just one way to confirm what is perhaps already clear in Tables 1 and 2, i.e., negative behavioral spillovers are worth tracking since they could well be of a magnitude which could influence the evaluation of a selection rule.

From a policy perspective when evaluating selection for an incentive the cost of the incentive also should be taken into account. Figure 1's second graphic conveys the results with that cost subtracted from the average gain per person selected. The net effect is then a loss of 89 colones, but that too is insignificant (two-tailed t-test p-value=0.832). This result suggests that targeting locations where private contributions are not likely to occur in the absence of policy could indeed achieve what analysts suggest in terms of additionality for those selected, however such an effect could be neutralized if exclusion of those who privately contributed convinces them not to do so. That should inspire search for how to maintain additionality while avoiding shifting motivations.

5. Conclusion

Our results suggested that behavioral spillovers – e.g., when those excluded from a new incentive choose to reduce contributions to forest conservation despite no changes in the prices they face or their incomes − are important to consider when targeting conditional incentives (such as Payment for Ecosystem Services (PES)) which are intended to increase the contributions to public goods. It is striking that *not being paid* can have significant impact on change over time in contributions.

We find that this is significant if incentives target past low contributors to public goods (e.g., those doing more deforestation), i.e., for selection rules that exclude the highest past contributors. This implies tradeoffs within PES programs. Targeting those who require incentives to contribute may increase those individuals' contributions to forest conservation. However, if the others who contributed more in the past felt they were providing public goods and are offended by exclusion,

such targeting may decrease contributions among the group excluded from the new incentive. We found such behavior. Further, we found that the *rationale for exclusion* matters for this spillover: excluding because of high past contributions differs from exclusion of high contributors based on external reasons such as region. Further research is, however, needed to generalize this finding. That is worthwhile, as this suggests the potential to try to target impact while framing selection in terms of exogenous variations in parcel type, or some other way that would not trigger spillovers.

The three selection rules we considered relate directly to the ongoing discussion of whether selection rules for forest-conservation incentives should be based more on additionality that in the past (e.g., focus on efficiency by paying those who would not conserve without the incentive), or instead rewards (e.g., focus on fairness by paying those who are already conserving forest), or instead environmental benefit based on location (e.g., pay those in prioritized areas due to high environmental benefits). As private forest conservation is at least effectively a form of voluntary contribution to a public good, a new incentive targeting those who have shown little pro-social or pro-environmental inclinations could well spoil the motivation of those who do like to contribute.

Various extensions of such analyses could probe further into the nature of such effects. More time periods could allow for dynamics, which is realistic for the many programs that have lasted. Considering the effect of changing the fraction of applicants who are selected into the incentive, and perhaps changing whether the participants are aware of that fraction, also seems worthwhile.

Our current findings of "behavioral leakage", i.e., negative conservation spillovers to those excluded from a targeted incentive (who face no price or income changes), suggest a tradeoff in incentives design. To maximize gains for the selected while minimizing losses for the excluded seems an important challenge. Our results offer first insights but also motivate further research.

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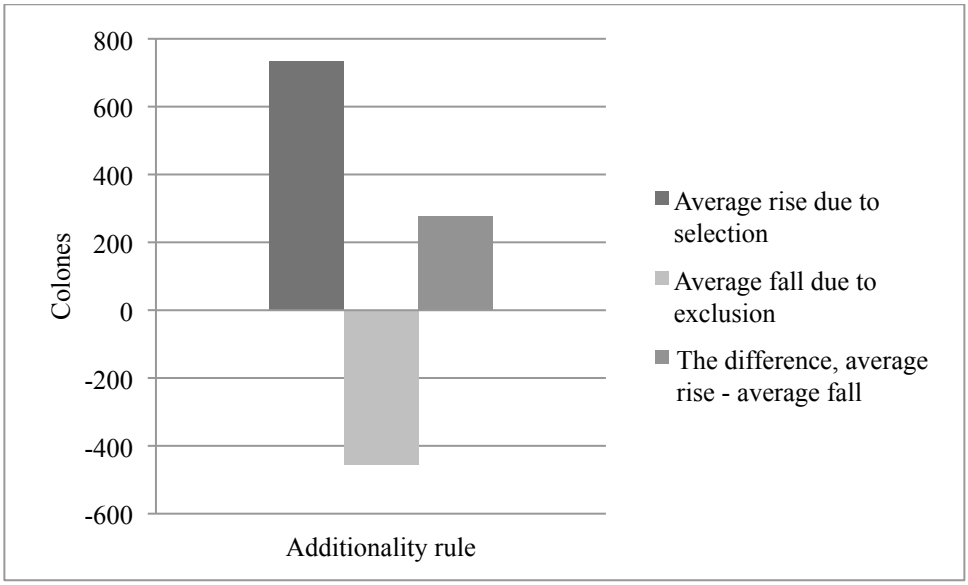
Figure 1

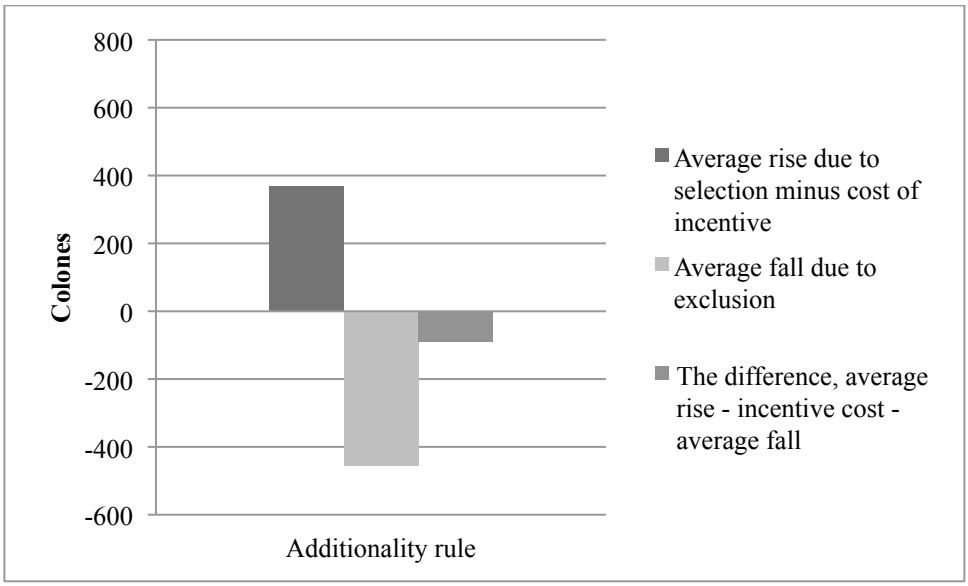
Additionality Rule's Effects

Changes In Contributions



Note: The net effect above is not significant (two-tailed t-test p-value=0.546).

Accounting for Incentive Cost



Note: The net effect above is not significant (two-tailed t-test p-value=0.832).

Table 1

Contributions By Round, Excluded and Similar Controls

	Additionality (select ≤ 1000) (exclude>1000)	**Reward** (select ≥ 2500) (exclude < 2500)	**Benefits** (select Nicoya) (exclude other)	**Benefits** (exclude other & filter for >1000)
EXCLUDED				
Round 1	4070 (71 obs)	1000 (37 obs)	3339 (62 obs)	4234 (47 obs)
Round 2	2824 (71 obs)	1081 (37 obs)	2468 (62 obs)	2862 (47 obs)
R2 - R1	-1246***	+81	-871***	-1372***
Similar[1] Controls				
Round 1	4040 (71 obs)	932 (37 obs)	3228 (57 obs)	4366 (41 obs)
Round 2	3250 (71 obs)	1108 (37 obs)	2474 (57 obs)	3220 (41 obs)
R2 - R1	-790***	+176	-754***	-1146***
Excluded − Similar Controls	**-456***	**-95**	**-117**	**-226**

[1] "Similar" = same Round 1 range or/and same region.

***=significant at 1%, **=significant at 5%, *=significant at 10%, according to a Wilcoxon test for the within-subject comparisons (H_0: Round 2 = Round 1) and a one-tailed t-test for the between-subject comparisons (H_0:Excluded – Similar Controls ≥0) to test the hypothesis that there is negative effect, i.e., behavioral leakage, upon exclusion from the incentive.

Table 2

Effects of Exclusion (excluded versus similar controls)

Dep.Variable = change in contributions	**Additionality (select ≤ 1000) (exclude > 1000)**		**Reward (select ≥ 2500) (exclude < 2500)**		**Benefits (select Nicoya) (exclude other)**		**Benefits (exclude other and filter for >1000)**	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Exclusion[#]	-443* (0.067)	-446* (0.068)	-91 (0.312)	-65 (0.365)	-67 (0.420)	-90 (0.397)	-280 (0.253)	-221 (0.310)
Initial Contribution								
linear	-0.45*** (0.000)	----	-0.06 (0.634)	----	-0.44*** (0.000)	----	-0.41** (0.020)	----
0-500	----	selected	----	omitted	----	omitted	----	filtered
500-1000	----	selected	----	-239 (0.293)	----	omitted	----	filtered
1500-2000	----	omitted	----	-94 (0.687)	----	omitted	----	omitted
2500-3000	----	-690 (0.182)	----	selected	----	-681 (0.339)	----	-67 (0.939)
3500-4000	----	-1091 (0.161)	----	selected	----	-226 (0.868)	----	323 (0.832)
4500-5000	----	-1441*** (0.000)	----	selected	----	-1756*** (0.000)	----	-1138* (0.055)
constant	1025** (0.048)	240 (0.518)	228 (0.185)	277 (0.128)	680* (0.061)	226 (0.497)	655 (0.427)	-323 (0.590)
# obs	133	133	74	74	119	119	88	88
R^2	0.1190	0.1198	0.0069	0.0197	0.1998	0.1772	0.0647	0.0245

[#] A one-tailed t-test is used to test a negative effect, i.e. behavioral spillover from exclusion.

***=significant at 1%, **=significant at 5%, *=significant at 10%

Table 3

Contributions By Round, Selected and Similar Controls

	Additionality (select ≤ 1000) (exclude>1000)	**Reward (select ≥ 2500) (exclude<2500)**	**Benefits (select Nicoya) (exclude other)**	**Benefits (select Nicoya & filter for ≤1000)**
SELECTED				
Round 1	605 (19 obs)	4380 (54 obs)	2778 (27 obs)	625 (8 obs)
Round 2	1500 (19 obs)	3546 (54 obs)	2852 (27 obs)	1375 (8 obs)
R2 - R1	+895*	-833***	+74	+750*
Similar[1] Controls				
Round 1	440 (25 obs)	4540 (50 obs)	2583 (30 obs)	667 (9 obs)
Round 2	600 (25 obs)	3510 (50 obs)	2517 (30 obs)	667 (9 obs)
R2 - R1	+160	-1030***	-66	0
Selected − Similar Controls	**+735****	**+197**	**+140**	**+750****

[1] "Similar" = same Round 1 range or/and same region.

***=significant at 1%, **=significant at 5% , *=significant at 10%, according to a Wilcoxon test for the within-subject comparisons (H_0: Round 2 = Round 1) and a one-tailed t-test for the between-subject comparisons (H_0:Selected – Similar Controls ≤0) to test the hypothesis that there is a positive effect, i.e. additionality, upon selection for the incentive.

Table 4

Effects of Selection (selected versus similar controls)

Dep.Variable = change in contributions	**Additionality (select ≤ 1000) (exclude > 1000)**		**Reward (select ≥ 2500) (exclude < 2500)**		**Benefits (select Nicoya) (exclude other)**		**Benefits (select Nicoya and filter for ≤ 1000)**	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Selection[#]	910** (0.011)	943*** (0.009)	172 (0.302)	200 (0.275)	170 (0.275)	106 (0.344)	716** (0.046)	833** (0.020)
Initial Contribution								
Linear	-1.06** (0.008)	----	-0.16 (0.385)	----	-0.15** (0.043)	----	-0.82* (0.087)	----
0-500	----	omitted	----	excluded	----	omitted	----	omitted
500-1000	----	-1087*** (0.006)	----	excluded	----	omitted	----	-1000** (0.027)
1500-2000	----	excluded	----	excluded	----	omitted	----	filtered
2500-3000	----	excluded	----	omitted	----	-737* (0.051)	----	filtered
3500-4000	----	excluded	----	200 (0.705)	----	-1245* (0.088)	----	filtered
4500-5000	----	excluded	----	-261 (0.519)	----	-455 (0.133)	----	filtered
constant	625** (0.042)	638** (0.036)	-319 (0.708)	-861** (0.036)	326 (0.215)	245 (0.259)	543 (0.197)	667* (0.092)
# obs	44	44	104	104	57	57	17	17
R^2	0.2203	0.2305	0.0110	0.0125	0.0780	0.1223	0.3330	0.4237

[#] A one-tailed t-test is used to test a positive effect, i.e. additionality from selection.

***=significant at 1%, **=significant at 5%, *=significant at 10%