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Essays on Behavioral Economics and Policies for Provision of Ecosystem Services

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Till Farmor och Farfar

Contents

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Anna Nordén

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Summary of the thesis

Forests provide key ecosystem services such as clean water, timber, habitat for fisheries, carbon sequestration, pollination, and biodiversity. However, many of these services are being lost or degraded at a furious pace, brought about by human activity. For instance, deforestation and forest degradation are measured to account for around 12% of all CO_2 emissions, making it the second largest anthropogenic source of carbon dioxide to the atmosphere after fossil fuel combustion (Werf et al. 2009). This has led communities, governments, and international organizations to increase their efforts to protect forests. Among such efforts, the use of monetary incentives to promote or reward private behavior that is associated with environmental objectives is becoming an increasingly popular policy instrument (Pattanayak et al. 2010, Ferraro 2011). For instance, payments for ecosystem services (PES) programs aim to increase the provision of ecosystem services by offering direct compensation to landowners for the opportunity costs of more environmentally friendly land management practices (e.g., low impact agriculture or conservation of natural ecosystems).

PES programs have been widely promoted as more cost-effective and institutionally less demanding than traditional conservation policies such as establishment of protected areas. Yet despite this, the few rigorous impact evaluations done so far show that the impact of PES programs has been modest (for a recent review see Pattanayak et al. 2010). This raises concerns that "easy fixes," like PES, may not solve the planetary problems we are facing. Further, PES may suffer the fate of many interventions that stumble in reaching their objectives because people do not always behave as expected. Cardenas et al. (2000), for instance, experimentally show that introducing an incentive to reduce timber extraction from common forest land led to more forest extraction compared with a case with no incentive. This so-called crowding-out effect has been found in studies, both in psychology and economics, where external incentives sometimes lead to less pro-social behavior[1] once the voluntary act is shifted to a market-based relationship (for a review of experimental as well as nonexperimental studies see Bowles 2008). At present, few attempts to understand the behavioral issues of forest conservation polices are undertaken.

This five-paper thesis attempts to contribute to the understanding of people's behavioral responses to forest conservation policies. The first paper examines determinates of the impact

[1] Pro-social behavior, understood as behavior that transcends the narrow definition of a selfish *homo economicus* to include concern for others at a cost to oneself.

of payment for ecosystem services (PES) and the role of behavioral aspects. The second and third papers experimentally examine behavioral responses to incentives for voluntary contributions to forest conservation, where some stakeholders are excluded in favor of others. The fourth paper investigates the relationship between participation in PES programs and type of payment (i.e., cash or in-kind). The fifth and last paper examines the effect of introducing fixed entrance fees on voluntary donations to a protected area.

Paper 1: Incentives, Impacts, and Behavioural Issues in the Context of Payment for Ecosystem Services Programmes: Lessons for REDD+ (Published 10 April 2013 in *Globalization and Development: Rethinking Interventions and Governance*, A. Bigsten (Ed.), Routledge Press)

Payment for environmental services (PES) aims to increase the provision of public goods and internalize environmental externalities by offering direct compensation to landowners for the opportunity costs of more environmentally friendly land management practices (e.g. low impact agriculture or conservation of natural ecosystems). Being promoted as more cost-effective and institutionally simpler than traditional environmental conservation policies, mainly small-scale PES schemes have spread prolifically across developing countries in the last decade. Despite their popularity, there are few rigorous impact evaluations of existing PES programmes, and the ones that have been done have generally shown modest impacts. Here we use a conceptual framework of PES additionality, i.e. a programme's ability to deliver outcomes that would not have occurred in its absence, to overview the main issues raised regarding the impacts of PES programme. We also show that PES impacts can be highly affected by information asymmetries and behavioural responses to the introduction and design of payment schemes. We draw upon these lessons to give policy advice to the design of REDD+ programmes.

Paper 2: Incentivizing versus Rewarding Good Behavior: Insights on the Use of Monetary Incentives

Payments conditional upon a socially desired behavior, such as blood donations, leaving armed forces, or provision of ecosystem services, are growing in popularity. Due to financial limitations and the need to show results, many of these incentive schemes are selective,

resulting in the exclusion of some stakeholders in favor of others. In this paper, we study the possibility of the stakeholders excluded from the monetary incentive reducing their pro-social behavior. We use a laboratory experiment to investigate this and hypothesize that alternative selection rules, i.e., who gets paid and why, affect the overall contributions to a public good differently. Our results show that incentivizing those who acted less pro-socially (i.e., contributed below a certain threshold) before the incentive was introduced resulted in increased contributions to the public good by this group. On the other hand, that very same selection rule excludes those who acted more pro-socially (i.e., contributed over a certain threshold) before the incentive was introduced, and this resulted in decreased average contributions by this group, decreasing the net effect on overall contributions. These results set up an efficiency-fairness tradeoff for designing selective conditional payments to promote pro-social behavior: Targeting those who require incentives to contribute may increase payment response beyond what would have happened in the absence of the incentive program, but it may also give rise to the unexpected consequence of negative spillovers.

Paper 3: Unintended Consequences of Targeting Forest Conservation Incentives: Behavioral Insights into Incentive Design

Ongoing concerns about species and water quality, plus growing attention to carbon, have generated significant interest in the use of incentives to promote forest conservation, e.g., payment for ecosystem services (PES) or Reduced Emissions from Deforestation and forest Degradation (REDD) payments. A key challenge in the design of such compensation mechanisms is the choice of whom to pay. Experts and practitioners debate whether the selection of whom to pay should be based on: i) additionality, by paying those who would not conserve without the incentive; ii) rewards, by paying those who are already conserving forest; or iii) location-based environmental benefit, in which case payments would go to those in prioritized areas. In this paper, we use a field experiment to test the hypothesis that these different selection rules imply different effects on contributions to forest conservation by those selected for the incentive and unintended negative effects on contributions by those excluded from the incentive. Our results suggest that it is only a focus on additionality that leads to decreased average contributions to forest conservation by unpaid individuals, thereby limiting the total gains expected from such focus. These results should be considered in the design of conservation incentive programs in general and PES schemes in particular.

Paper 4: Payments in Cash or in Kind for Ecosystem Services: Stated Preferences of Costa Rican Landowners

This paper investigates landowners' preferences for type of payment, cash or in kind, for the provision of ecosystem services. A choice experiment analysis focusing on the effect of different levels of cash and in-kind payments on participation in a payment for ecosystem services (PES) contract is provided. We use an educational in-kind payment in the form of days of practical training offered free of charge to the recipients. The results indicate a positive correlation between participation in a PES contract and the magnitude of the cash payment—higher cash payments increase the probability of participation—while participation seems uncorrelated with the magnitude of the in-kind payment. We also find that both in-kind and cash payments increase the likelihood of participation in shorter PES contracts (i.e., 5 years), while in-kind payments have no significant effect on participation in longer contracts (i.e., 15 years). Higher levels of cash payment seem to be what is needed to increase the likelihood of participation in longer contracts. In addition, we investigate heterogeneity in preferences for type of payment, which can help policymakers better target payment types to specific groups of landowners.

Paper 5: Do Entrance Fees Crowd Out Donations for Public Goods? Evidence from a Protected Area in Costa Rica

In this paper, we investigate how different levels of entrance fees affect donations for a public good, a natural park. To explore this issue, we conducted a stated preference study focusing on visitors' preferences for donating money to raise funds for a protected area in Costa Rica given different entrance fee levels. The results reveal that there is incomplete crowding-out of donations when establishing an entrance fee.

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Paper I

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Incentives, Impacts, and Behavioural Issues in the Context of Payment for Ecosystem Services Programmes: Lessons for REDD+

*Anna Nordén, U. Martin Persson and Francisco Alpízar**

Payment for environmental services (PES) aims to increase the provision of public goods and internalize environmental externalities by offering direct compensation to landowners for the opportunity costs of more environmentally friendly land management practices (e.g. low impact agriculture or conservation of natural ecosystems). Being promoted as more cost-effective and institutionally simpler than traditional environmental conservation policies, mainly small-scale PES schemes have spread prolifically across developing countries in the last decade. Despite their popularity, there are few rigorous impact evaluations of existing PES programmes, and the ones that have been done have generally shown modest impacts. Here we use a conceptual framework of PES additionality, i.e. a programme's ability to deliver outcomes that would not have occurred in its absence, to overview the main issues raised regarding the impacts of PES programme. We also show that PES impacts can be highly affected by information asymmetries and behavioural responses to the introduction and design of payment schemes. We draw upon these lessons to give policy advice to the design of REDD+ programmes.

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Introduction

The last decade has witnessed a rapid increase in the popularity—both in theory and practice—of *payments for ecosystem services* (PES) as an environmental policy tool in developing countries (Pattanayak et al. 2010, Ferraro 2011). An early review by Landell-Mills and Porras (2002) found approximately 200 incipient PES schemes in developing countries, and the numbers have only increased since then (Pattanayak et al. 2010). Although often small in scale, a few countries have established nationwide PES schemes: Costa Rica has its *Pagos por Servicios Ambientales* (PSA) programme, which since its inception in 1997, has made payments for forest conservation (primarily) on nearly half a million hectares of land; China has its *Sloping Lands Conservation Programme* (SLCP), which has thus far contracted 12 million hectares for reforestation in an attempt to stem soil erosion; and Mexico with its *Pago de Servicios Ambientales Hidrológicos* (PSAH) programme, which compensates beneficiary communities for preserving 600,000 hectares of forest (Pattanayak et al. 2010).

By directly compensating resource users for the opportunity costs of ecosystem service provision, PES has been touted as institutionally simpler and more cost effective than other, more indirect, conservation policies (Pattanayak et al. 2010, Ferraro 2011). Despite the claim that PES programmes are cost effective, few rigorous evaluations of the environmental and social impacts of PES programmes have been conducted. A recent review of the few credible assessments available concluded that programmes generally show poor performance in terms of delivering additional ecosystem services, and that 'we do not yet fully understand either the conditions under which PES has positive environmental and socioeconomic impacts or its cost-effectiveness' (Pattanayak et al. 2010:268).

The fact that a large share of annual greenhouse-gas emissions are a result of changes in land use and the degradation of ecosystems has led policymakers participating in international climate deliberations to discuss how best to incentivise efforts to reduce emissions from deforestation and forest degradation (REDD+). The result is a campaign in favour of payments for the protection and management of forests. Although many details still need to be finalised, the basic premise of REDD+ is to compensate developing countries for reduced greenhouse-gas emissions from land use change — most notably from tropical deforestation — or for the conservation or enhancement of

forest carbon stock, with financing coming either from global carbon markets or from international funds.

Given its focus on performance-based payments, REDD+ is viewed as a multilevel PES scheme, where national and subnational PES programmes will be key tools for REDD+ implementation (Bond et al. 2009, Angelsen 2010, Pattanayak et al. 2010). If experience with PES programmes is to serve as a blueprint for REED+ implementation, it is important to understand the determinants of PES programme success and how outcomes are affected by information asymmetries and behavioural responses.

For the purposes of this book, it is important to recognise that most PES schemes implemented thus far have been local (e.g., county or municipality) or national initiatives, although, at times, these efforts have been supported by multilateral organisations. In the case of REDD+, funding for policy interventions comes from multilateral or bilateral initiatives; examples of the former include UN-REDD and the World Bank's Forest Carbon Partnership Facility and Forest Investment Program, while Norway is the main funder of bilateral REDD+ activities (for an overview of existing REDD+ funding initatives see, for example, Westholm 2010). REDD+ is therefore understood here as a nationally implemented, but externally motivated and funded, intervention.

In this chapter, we describe a key issue raised in the literature regarding PES impact and efficiency: the extent to which PES programmes are capable of delivering desired outcomes beyond what would have occurred in their absence (referred to as *additionality*). This is a key issue for REDD+, because, unlike many past payments schemes, countries will only be eligible for payments if they reduce greenhouse-gas emissions from land-use change below an established baseline.[1] The aim of this chapter is to offer guidance to policymakers regarding the circumstances under which PES is an appropriate policy choice and how PES programmes can be designed to maximise impact and minimise unwanted spillover effects.

We do so by first drawing upon a conceptual model of PES additionality (Persson and Alpízar 2011), showing that programme efficiency can be highly constrained by information asymmetries regarding agents' decisions to comply with programme conditions in the absence of payments. Second, we extend the conceptual model by drawing upon results emerging from the behavioural economics literature regarding

how agents respond to economic incentives in general, including specific information resulting from economic experiments that examine behavioural response in relation to PES.

Accounting for behavioural responses is important because an economic agent's reaction to a PES intervention is the result of a complex decision-making process that is only affected by the payment itself to a limited extent. For instance, monetary incentives could crowd in or out intrinsic motivations for protecting the environment. Additionally, participation in a PES programme might be due to peer pressure, learning, or simply inertia, e.g. neighbour and signalling effects, all of which are combined with the actual reaction to the payment.

Moreover, using both the model and insights from the behavioural economics literature, we discuss the ability to overcome the information asymmetries that limit programme efficiency through programme design, primarily by improving payment targeting.

Understanding the determinants of PES additionality: introducing a conceptual framework

Payment for ecosystem services is a policy that aims to increase the provision of ecosystem services and protect the natural-resource base by paying landowners for good agricultural practices or complete conservation of natural vegetation on their lands. If carefully designed and implemented, payments should provide sufficient incentives to adopt land-use management practices that reduce downstream pollution (e.g. reduced use of pesticides), avoid deforestation and increase carbon sequestration.

PES programmes thus identify an activity or behaviour on which payments will be conditioned, define the population eligible for payments (e.g. landowners in a given area) and then decide who will ultimately be the programme's beneficiaries, either by negotiating with service providers (small-scale PES schemes) or by choosing among applicants (national PES). Two central features of PES schemes are, thus, as follows: (1) their voluntary nature — households or landowners freely choose whether to apply for or accept payments or not[2] — and (2) conditionality — once an agent has received payments, meeting the conditions is mandatory, although the extent to which this is monitored, if at all, varies widely across PES programmes (Wunder et al. 2008).

Following Persson and Alpízar (2011), given the voluntary nature of PES programmes, potential participants can be divided into four categories (see Figure 8.1):

A — those who apply for payments, but will meet the programme conditions with or without them;

B — those who apply for payments and will not fulfil the conditions without payments;

C — those who do not apply for payments but will meet the conditions regardless; and

D — those who do not apply and will not meet the conditions.

Applies for payment?	*Will meet PES conditions in absence of payment?*	
	Yes	*No*
Yes	**A:** $U_C(M_C, N_C) > U_{NC}(M_{NC}, N_{NC})$ $U_P(M_P, N_P) > 0$	**B:** $U_C(M_C, N_C) < U_{NC}(M_{NC}, N_{NC})$ $U_P(M_P, N_P) > U_{NC} - U_C$
No	**C:** $U_C(M_C, N_C) > U_{NC}(M_{NC}, N_{NC})$ $U_P(M_P, N_P) < 0$	**D:** $U_C(M_C, N_C) < U_{NC}(M_{NC}, N_{NC})$ $U_P(M_P, N_P) < U_{NC} - U_C$

Figure 8.1: *Conceptual categorisation of potential PES payees based on their counterfactual compliance and application decisions. Agents will fall into categories A, B, C and D, depending on the utility (U) they would derive from meeting the programme conditions (C), the utility from non-compliance (NC), and the utility from PES participation (P), each of which is shaped by both monetary (M) and non-monetary (N) factors.*

The only instance in which the programme will induce a direct change in behaviour is by focusing on type-B landowners. Consequently, the direct impact of the programme, in terms of additionality, can be measured as the share (or number) of payments going to type-B applicants. This, in turn, will be determined by the following three factors: (1) the share of the population of eligible payees that will not comply with programme conditions in the absence of payments (what we call counterfactual compliance or [B+D]/[A+B+C+D]); (2) the degree to which agents who would meet programme conditions in absence of payments self-select into the programme (what we call selection bias, defined as the ratio between A/[A+C] and B/[B+D]); and (3) the

extent to which one is able to identify agents who will not meet conditions in absence of payments and use this information to target payments to them.

In the case where payments are not targeted based on the predicted risk of non-compliance, for example, deforestation risk (which is the case in the majority of existing PES schemes), additionality is strictly determined by (1) and (2); thus, (1) the higher the counterfactual compliance level, the lower the expected additionality, and (2) the more that selection bias causes compliers to self-select into the programme, the lower the additionality. Figure 8.2 illustrates these insights with results from a stylised multi-agent model of PES, simply being a numerical implementation of the conceptual framework presented in Figure 8.1.[3]

Figure 8.2 clearly shows that PES additionality and, consequently, cost-effectiveness will be seriously constrained in cases where counterfactual compliance with programme conditions is high. Another way to frame this result is in terms of information asymmetries (Pattanayak et al. 2010); to effectively implement PES, the regulator needs to target payments to those who would not meet the programme conditions in the absence of payments. However, this information is not available to the regulator but only to the PES applicants. The smaller the share of the eligible population that will not comply in the absence of payments, the larger this problem of hidden information is. Consequently, one cannot make general claims about the cost-efficiency of PES programmes; cost efficiency will be highly affected by the context in which a PES programme is implemented.

Consistent with the basic insights from the conceptual model, evaluations show that the additionality of forest conservation in the Costa Rican and Mexican PES programmes, where baseline compliance is over 99 per cent (annual deforestation rates are near or below 0.5 per cent), has been low (on the order of 1 or a few per cent; see Figure 8.2) (Pfaff et al. 2008, Robalino et al. 2008, Muñoz-Piña 2010, Alix-Garcia et al. 2012). Moreover, evaluations of these programmes also suggest that additionality has been reduced owing to self-selection bias; i.e. landowners with plots having a low risk of deforestation represent a disproportionally large share of applicants (Hartshorn et al. 2005, Muñoz-Piña et al. 2008, Pfaff et al. 2008).

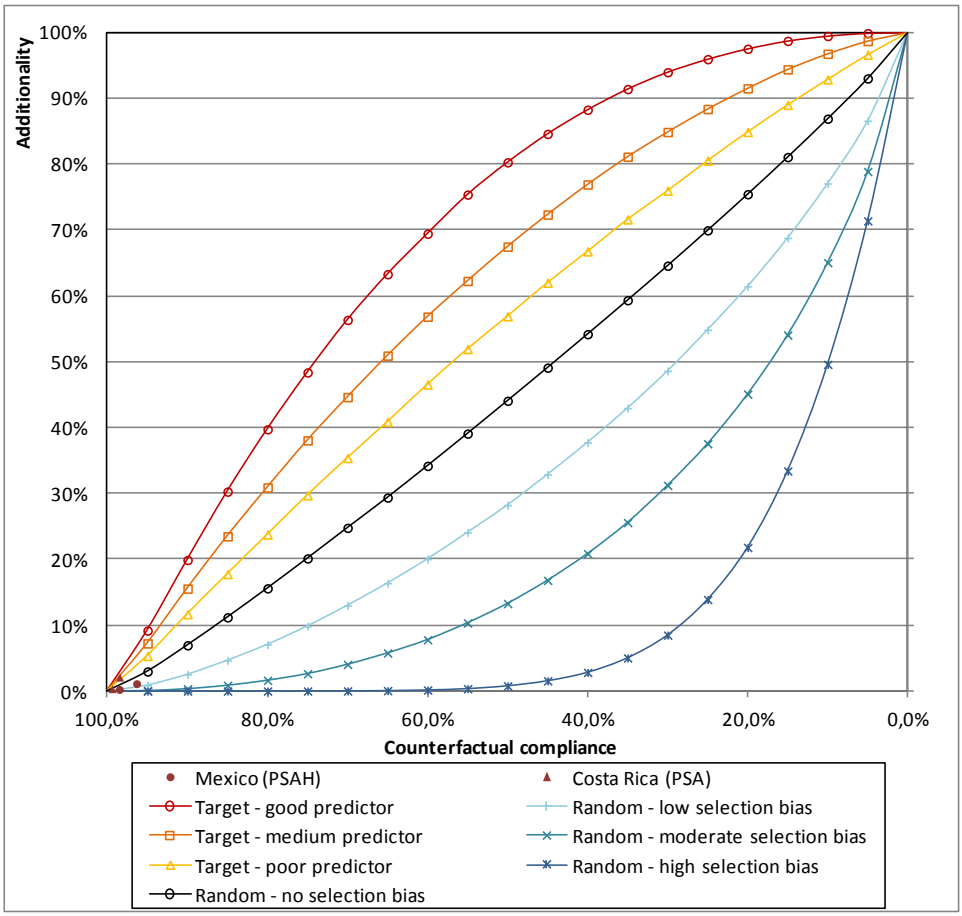

Figure 8.2: *Results from a stylised multi-agent model of PES additionality, showing how additionality depends on counterfactual compliance (the share of potential payees that would have met programme conditions in the absence of payments), negative selection bias (those who already meet conditions self-select into the programmes at higher rates than others), and targeting based on imperfect predictors of counterfactual compliance. In the absence of selection bias and targeting, additionality roughly equals counterfactual non-compliance; negative selection bias reduces programme impacts, whereas targeting increases it. Good, medium, and poor targeting predictors correspond to cases where 10, 30, and 60%, respectively, of the variance in the variable that determines agents' compliance decisions is determined by factors observable to the regulator. Low, moderate, and high selection bias correspond to cases where the correlation between the variables that determine agents' decisions regarding compliance and participation are set to -0.3, -0.6, and -0.9, respectively. See (Persson and Alpízar 2011) for a full description of the model. The results from impact evaluations of the national PES programmes in Costa Rica (Pfaff et al. 2008, Robalino et al. 2008) and Mexico (Muñoz-Piña 2010, Alix-Garcia et al. 2012) are also shown.*

Selection bias can occur for two reasons, the first being that agents who will comply with the programme conditions in the absence of payments have a lower threshold for applying for payments than those who will not comply, as, in the latter case, the payments must cover the opportunity costs of compliance (see Figure 8.1). The second reason for selection bias may be that the factors that affect whether agents meet the programme conditions also affect the decision of whether to apply for payments.

In the Costa Rican case, selection bias seems to be primarily explained by the first mechanism: Ortiz et al. (2003) find that PES is only profitable on marginal lands with zero opportunity cost of conservation. Through interviews with both PSA participants and nonparticipants, Arriagada et al. (2009:355) find that the most common reason for enrolling land in PSA is a 'lack of more profitable land use alternatives due to land characteristics', and the second most common reason for not enrolling land — after lack of information — is that the payments are too low.[4] In the Mexican case, however, poor programme design seems to be the chief reason for negative selection bias: land in the two quintiles with the highest estimated deforestation risk constitutes only 18 per cent of forest land eligible for conservation payments (Muñoz-Piña et al. 2008).

Although the basic insights from the conceptual model provide a good understanding of the main determinants of the direct impacts of a PES scheme, the introduction of a PES scheme in itself introduces a change in institutions that might alter the way landowners enter into the typology in Figure 8.1. For example, the mere existence of a payment scheme may affect landowners' intrinsic motivations to conserve natural ecosystems (motivational crowding) or change the value of land belonging to non-participants (leakage/spillovers). Moreover, targeting payments to certain landowners may alter the decisions made by those who do not receive payments. Understanding and ultimately quantifying these indirect effects are important for evaluating the full impact of a PES programme but requires a deeper understanding of the factors shaping landowner decisions.

Beyond direct monetary incentives: spillovers from the introduction and design of ecosystem payments

There is an extensive literature on the factors affecting landowner attitudes towards environmental conservation and decisions regarding participation in PES-like systems (see reviews by, e.g., Siebert et al. 2006, Knowler and Bradshaw 2007). However, the latter studies almost exclusively focus on farmers' participation in agro-environmental schemes (AES)[5] in the EU, USA, and Australia; exceptions are studies from Costa Rica (Zbinden and Lee 2005, Wünscher 2008, Arriagada et al. 2009), Mexico (Kosoy et al. 2008), and China (Mullan and Kontoleon 2009). In the following, we will briefly review the findings from this literature, framing it with the conceptual model of PES additionality introduced in the previous section.

Under what circumstances will landowners fall into the different categories (A, B, C, and D) identified in the conceptual model? Starting with the decision of whether to comply with the programme conditions in the absence of payments, if we assume that agents maximise their expected utility[6], we would expect landowners to comply if the (expected) utility they derive from doing so is larger than the (expected) utility from not complying. Studies analysing the factors affecting farm and forestry management decisions and PES participation have shown that these are not exclusively shaped by monetary incentives (e.g., Lynne et al. 1988, Siebert et al. 2006, Knowler and Bradshaw 2007). It is therefore, useful to make the distinction between monetary and non-monetary factors affecting the utility of compliance and PES participation.

Here, we will express the utility of complying with programme conditions or not as $U_C(M_C, N_C)$ and $U_{NC}(M_{NC}, N_{NC})$, respectively, where M_C is the land-use revenue under compliance (e.g. incomes from non-extractive forest uses), N_C is the non-monetary (intrinsic) value of compliance (e.g. concerns for environmental issues, culture/spirituality reasons, signalling as environmental stewards), M_{NC} is the land-use revenue when not complying (e.g. incomes from agriculture or cattle ranching), and N_{NC} is the non-monetary (intrinsic) value of non-compliance (e.g. professional pride or traditional values of being a farmer or rancher). An agent will comply with PES conditions in the absence of payments only if $U_C > U_{NC}$ (see Figure 8.1).

Similarly, the decision of whether to apply for, or accept, PES payments will be determined by the utility of participation, which will also be shaped by monetary (M_P)

and non-monetary (N_P) factors, $U_P(M_P, N_P)$. An agent will apply for, or accept, payments if the utility from doing so is positive and offsets the opportunity cost of participation, i.e. if $U_P > \max[0, U_{NC} - U_C]$. The monetary incentive for participation will depend on the payment level, the costs of participation (e.g. protection costs such as firebreaks in the case of forest conservation), and transaction costs (e.g. the costs of information gathering and contract establishment). The non-monetary benefit of PES participation may be either positive or negative as a result of, for example, pride of being recognised as an environmental steward or mistrust towards the regulator.

Consequently, a land-user's decision of whether to apply for PES payments and conserve a patch of forest or to convert it to other uses will be shaped by both the economic returns to each option and the intrinsic well-being the landowner derives from having his or her land in either use or from participating in the scheme. As noted above, the empirical evidence suggests that monetary concerns are the primary motives for conservation and PES participation decisions, with the most common reason for enrolling land in PES in both developed- and developing-country settings being that the opportunity cost of doing so is low (a lack of profitable alternatives, fits with existing management plans), whereas a common reason for not enrolling land is that the payments are too low (Wilson and Hart 2000, Arriagada et al. 2009).

However, there is ample evidence that non-monetary factors also play a significant role in shaping these decisions. Studies across developed and developing countries on the adoption of environmentally friendly farming technologies in the absence of PES-like policies find that non-monetary factors — e.g. environmental awareness and concerns — increase conservation efforts (Lynne et al. 1988, Knowler and Bradshaw 2007). Similarly, Chouinard et al. (2008) find evidence of farmers in the US being willing to forgo some profit in the service of conservation objectives.

Examining participation in payment schemes, Siebert et al. (2006), surveying 160 studies of PES participation across six EU member states, find that social factors (farm continuity, work satisfaction, public legitimacy) play a significant role in land-users' decisions. Similarly, Wilson and Hart (2000) find that more than half of the participants in sixteen PES schemes, in ten European countries, stated environmental concerns as important reasons for participation. Finally, risk considerations may also affect PES participation decisions (Lynne et al. 1988, Ferraro 2001, Anderson 2006, Wünscher et

al. 2011). Wüncher et al. (2011) find that factors related to risk perception (risk aversion, age, trust in the regulator) affect farmers' attitudes towards enrolling land in the Costa Rican PES programme.

Unfortunately, the interaction between the monetary and non-monetary attributes of a conservation policy is seldom studied. A common assumption is that utility is separable in the monetary and non-monetary arguments (Bowles 2008), such that a change in the level of one (e.g. the introduction of an economic incentive through a PES) does not affect the other. However, a growing body of both theoretical and experimental evidence shows that separability does not always hold, which can change both people's perceptions of a task as well as the outcomes of policy interventions, in sometimes unexpected ways (Frey and Jegen 2001, Bowles and Polania Reyes 2009). As a consequence, the introduction of conservation payments will not unequivocally raise the provision of ecosystem services.

In the next subsections, we discuss different ways in which indirect monetary and non-monetary motivations may affect the impact of introducing payments for ecosystem services. We begin by making a distinction between price spillovers and behavioural spillovers. Price spillovers are the side effects of introducing payment schemes that result from the change in relative prices that is caused by setting a portion of the available land aside for conservation. Behavioural spillovers are side effects from the PES scheme that affect land-use decisions by changing the personal motives for protecting a natural ecosystem, even in the absence of price changes.

Price spillovers: leakage from conservation efforts

In typical neoclassical economic models of land-use change, one would expect that, as conservation efforts (e.g. payments or protected areas) reduce forest clearing, timber and crop prices would rise, which could lead actors, those who lost outputs or others, to increase production elsewhere. Such 'leakage' (or 'slippage') would have negative effects on the programme outcomes, as deforestation is simply spatially shifted. In a theoretical framework, Sohngen and Brown (2004) used Bolivia as a global timber market supplier to estimate the potential negative effect of forest-based carbon projects. Their model suggests that leakage could range from 2 to 42 per cent, depending strongly on assumptions.

Some recent empirical evidence for the Mexican PSAH programme found evidence of an approximately 4 per cent reduction in programme efficiency due to leakage, as a result of increased deforestation both on property belonging to the programme recipients and within markets with high levels of programme participation (Alex-Garcia et al. 2012). Wu et al. (2001) use an analytical framework and show that ignoring the effects of conservation strategies on prices can reduce the environmental gains.

Conceptually, as the prices of agriculture commodities and forest-extracting activities increase, the monetary revenue of non-compliance activities (M_{NC}) increases, potentially having a negative effect on the programme's outcomes through leakage in the following ways: (1) turning As into Bs, threatening the outcomes of the PES by making compliance conditional on payment; (2) turning As or Cs into Ds, increasing non-compliance.

Although spillovers commonly are assumed to arise owing to changes in the relative value of land, it is important to add that non-pecuniary incentives might also be behind the observed leakage. For example, signalling could also play a role, where, by committing to protect one hectare of land, the landowner might feel entitled to clear another hectare somewhere else, without losing his/her green image. Alternatively, owners of large forests might be willing to commit to conservation on part of their land, in exchange for lenient tax treatment and environmental policy enforcement on the rest of their land.

Moreover, Robalino and Pfaff (2012) show that neighbours' land-use decisions are significantly affected by each other. Using highly explicit spatial deforestation information, they show that neighbouring deforestation significantly increases the probability of deforestation. Such positive spatial interaction is good news, as policies promoting conservation in one area could potentially increase conservation in neighbouring areas. However, as mentioned above, the ways in which agents react to a policy will depend on how the policy is perceived.

Effects on the intrinsic valuation of compliance: motivation crowding and preference formation

In a public-goods experiment (Cardenas et al. 2000, Cardenas 2004), subjects from a village in rural Colombia had to decide how much time to spend gathering firewood in

the forest, presenting the villagers with the social dilemma of maximising their own payoffs and those of the community, as higher levels of wood extraction were posited to increase soil erosion and damage local water quality. As other public-goods games have shown, the villagers exhibited some other-regarding preferences by making decisions that were neither privately nor socially optimal, but somewhere in between. In a second round of the experiment, a regulation (an imperfectly enforced time quota on firewood gathering, coupled with a fine for noncompliance) was introduced. Although this initially reduced resource use, after a while, as participants realised that some participants where violating the quota and not getting caught, the time spent in the forest rebounded to its earlier value. The monetary incentive introduced by the regulation, therefore, did not succeed in increasing the provision of the public good, but simply replaced — or crowded out — the subjects' intrinsic motivations for limiting resource use.

This experiment illustrates some general insights from the behavioural economics literature. First, agents tend to strike a balance between monetary and non-monetary interests. Second, external interventions can have unanticipated effects on the latter. With respect to PES, when an monetary incentive in the form of PES payments is introduced, voluntary compliance with the programme conditions becomes more of a market-like interaction, possibly affecting the non-monetary (intrinsic) motivation to comply with the programme conditions (cf. Heyman and Ariely 2004). The effects this may have on programme impacts can be both positive and negative, depending on how the monetary and social preferences interact.

The payment and the intrinsic motivation could be either (1) complements, implying that the introduction of a monetary incentive for conservation will increase the intrinsic motivation for the same, commonly called *crowding in*, or (2) substitutes, where the intrinsic values, as in the experiment above, are *crowded out* by a monetary incentive (Frey and Jegen 2001, Bowles and Polania Reyes 2009). In our conceptual model, motivation crowding would imply that, as we introduce (or change the level of) a payment, and therefore the level of U_P, we will also affect the value of N_C and therefore U_C.

Crowding out would lower U_C, potentially having a negative effect on programme impacts in the following way: (1) by turning As into Bs, which has a negative effect if

the resulting Bs cannot be paid and by threatening the outcome of the PES in the long run by making compliance conditional on payments; and (2) by turning As, Bs, or Cs into Ds, which in all three cases increases non-compliance.

Conversely, the crowding in of social and environmental values, raising U_C, could potentially have a positive effect on programme compliance by (1) turning Ds into As, Bs, or Cs, and (2) by turning Bs into As. The latter will tend to increase compliance when not all PES applicants can be paid and would have a positive long-term effect on preferences for environmental conservation.

Moreover, as individuals are also motivated partly by how they are perceived by others, forest conservation may well be partly motivated by preferences for being perceived as a pro-environmental person. This so-called image or signalling motivation captures the rule of opinion in utility, i.e. the desire to be liked and respected by one's peers. Agents therefore, attempt to signal characteristics that are defined as 'good', based on social norms and values, in the search for social approval (for economic models incorporating social approval, see, for example, Bénabou and Tirole 2006, Andreoni and Bernheim 2009, Ellingsen and Johannesson 2008).

Studies have found that such image motivation could be crowded out when an incentive is introduced. In an experimental study, Ariely et al. (2009) found a negative relationship between payments and image motivation, where payments had a tendency to crowd out the motivation to signal socially preferable behaviour. A possible explanation for this result is that the signalling effect becomes unclear, as some individuals are behaving in accordance with social norms simply owing to the incentive. Introducing a PES payment conditional on forest conservation may then crowd out the green-image motivation, as it becomes impossible to distinguish between those conserving the forest because of the payment and those that would have done so in any case.

There is limited empirical evidence of motivation-crowding effects regarding PES (Boon et al. 2010, Kits et al. 2012). Kits et al. (2012) used an experimental approach to investigate motivational crowding in the case of conservation auctions, i.e. market-based instruments that, similar to PES, are intended to encourage landowners to adopt environmentally friendly management practices. Their results reveal a statistically significant (though small) reduction in the voluntary provision of environmental quality

(via monetary donations to an environmental charity) when the conservation action is removed, suggesting motivational crowding out.

In a stated-preference survey among Danish forest owners, Boon et al. (2010) find a small crowding-out effect, with 2 per cent of the sample of forest owners being willing to set aside less of the forest for conservation under a payment scheme than without it (although this minor negative effect is overwhelmed by the large positive effect on the willingness to conserve forest land among the rest of the sample). There are a few studies that find that participation in PES in the EU induces some changes in preferences, suggesting crowding in, but overall attitudes towards environmental conservation seem to be unaffected by participation in PES schemes, leading Burton et al. (2008: 18) to conclude that 'the schemes act as a facilitator for the expression of existing attitudes rather than agents of attitude change'.

In a comparative study of three PES schemes in Latin America, Kosoy et al. (2008) find that payments often do not cover the farmer's opportunity cost of participation, with participation partly being explained by participants feeling that the PES supports them as environmental stewards. Although this does not constitute proof of crowding in — merely that considerations other than purely pecuniary ones play a role — it does indicate that there is no strong effect in the opposite direction.

A small but positive crowding effect from a voluntary, positive economic incentive, such as a PES, is consistent with the psychological notion that crowding in occurs when agents feel that external interventions are supportive, whereas crowding out occurs when agents feel that interventions are designed to control their behaviour (Frey and Jegen 2001) (cf. results from Cardenas et al. 2000 discussed above). However, as will be discussed below, a positive effect may be contingent on participants perceiving the payment scheme as fair (Bowles and Polania Reyes 2009), something that may be affected by measures to increase programme impacts through, for example, increased targeting.

One reason for the lack of evidence on motivation crowding in PES programmes — apart from the fact that few empirical studies on this effect have been carried out — is that crowding out of intrinsic motivation may only become evident when the payments cease. There is anecdotal evidence from Mexico of landowners threatening to cut down their forests if they do not continue to receive PES (Kaimowitz 2008). The major

concern here may, therefore, be that there is little evidence to date supporting the notion that these programmes affect the long-term preference formation of participants, which implies that, to maintain the environmental gains from PES, payments must continue indefinitely. Consequently, if the incentives for deforestation increase in the future — owing to increased demand for land for food, feed, and biofuel — PES may have done little to buttress the support for forest conservation, and whatever gains that have been made may be lost (Persson 2012).

Fairness versus efficiency: the potential for, and limits to, targeting payments to increase additionality

Past experience clearly shows that failing to target risk has constrained the impacts on forest conservation efforts, both in the national PES programmes in Costa Rica and Mexico and regarding protected areas, the locations of which have been found to be biased towards areas unlikely to face land-conversion pressures (Andam et al. 2008, Joppa and Pfaff 2009). However, the results also show that programme design does matter: the Costa Rican PSA system achieved greater additionality in the 2000-2005 period thanks to new selection rules that reduced bias towards low-risk landowners (Robalino et al. 2008).

Several studies acknowledge the need for improved PES targeting to counteract adverse selection and increase additionality (Pattanayak et al. 2010, Ferraro 2011). However, although targeting has the theoretical potential to substantially increase the efficiency of PES programmes, there are a number of obstacles that may hinder its success in practice. The foremost is, of course, the reason that PES may perform poorly in the first place: the issue of asymmetric information. As landowners know more about their ecosystem service's vulnerability and opportunity costs than do PES regulators, they are able to extract informational rents from PES buyers in the form of payments for non-vulnerable ecosystem services or payments well above the full costs of protection (including direct costs, transaction costs, and opportunity costs).

Programme officials, therefore, need to rely on imperfect information regarding the risk of non-compliance, and the gains from targeting will be highly dependent on how well those risks can be predicted (see Figure 8.2). Additionally, any gains from targeting should offset the administrative costs of gathering the information necessary to

implement targeting strategies, which, in practice, can be quite expensive (Engel et al. 2008, Ferraro 2008).

A simpler, and less expensive, way to target payments that does not require information about non-compliance risk for individual applicants is by restricting a PES programme to an area with a higher incidence of baseline non-compliance. Both in the Amazon and in South East Asia, annual deforestation rates may exceed 5 per cent in some areas (Mena et al. 2006, Miettinen et al. 2011), implying non-compliance rates — and, consequently, potential PES additionality — that are an order of magnitude higher than those prevailing at national levels.

However, the gains from both geographic and individual targeting may be offset through increased market-based leakage, with the deforestation pressure simply shifting to areas or individuals not targeted by the programme. Moreover, targeting almost inevitably causes PES policies to treat certain groups of potential participants differently. It follows that selection rules, differentiated payments and other targeting strategies can introduce questions about the equity and fairness of PES programmes and, consequently, the risk of what one can call behavioural spillovers.

Economic theory regarding fairness suggests that people in general are inequity averse; i.e. agents experience disutility, not only from being worse off than others, but also from being better off (Fehr and Schmidt 1999, Bolton and Ockenfels 2000). The evidence reveals, not simply that inequity aversion is distaste for inequity, but that agents are willing to incur some private cost to avoid it.

In the context of PES, a narrow focus on efficiency will most likely suggest targeting payments to those acting in their own self-interest to shift their behaviour. This would imply paying agents that are, in the words of Wunder (2007: 53), 'if not outright environmentally nasty, then at least at the edge of becoming so'. Although potentially efficient, when we consider only programme beneficiaries, this sort of selection rule might lead those not being paid, owing to their perceived low risk of non-compliance to feel unfairly treated. A possible result of this may be that they retaliate by deforesting their land, justified by the feeling that they were not rewarded but punished for their previous environmental stewardship (Lindhjem et al. 2011).

Alpízar et al. (2012a) examine such behaviour in a laboratory setting by testing selection rules in a modified dictator game at the University of Costa Rica, including

over 400 students in total. The basic structure of a dictator game is that the dictator receives an endowment to divide between himself and a receiver. Their results concern a three-period game, in which all subjects initially play a dictator game where the receiver is a green public fund used to protect forests in Costa Rica (i.e. before the policy is created). When the second round is played, some players qualify for conditional payments, i.e. a reduced cost for contributions in the third period. One rule for who qualifies is a lottery. Another is that the people who contributed more (specifically above a threshold) in the second period received payments, and a third is that the people who contributed less (specifically below a threshold) within the second period received payments. The authors find that those not selected for payment reduce their contributions significantly depending on the selection rule. In particular, they find the greatest behaviour spillovers when payments go to the non-contributors, a result that is strengthened further by a natural field experiment performed by Alpízar et al. (2012b). This presents an efficiency dilemma for PES: targeting those who require incentives to contribute might lead to the desired response, but may also produce undesirable behavioural responses from those not selected for payment.

Returning to the conceptual model in Figure 8.1, targeting only those that comply with the programme's conditions (Bs) may generate behavioural responses that could decrease the utility of compliance (U_C), as the non-monetary motivations (N_C) for complying for those that are not selected for payment might decrease. This would have a potentially negative effect on the impact of PES by tuning those who comply with the programme without payment, and do not receive a payment in this case (As and Cs), into landowners who would only comply if paid (Bs or Ds), potentially producing a negative effect if Bs cannot be paid, or in the long run if the payments will not continue indefinitely.

Although far from providing an exact estimate of behavioral spillovers in the real world, these results shed light on an aspect of PES design that is frequently a source of disagreement between experts and practitioners: targeting payments to those who would otherwise not contribute to conservation efforts makes sense from an efficiency perspective — experts argue, but excluding those that indeed would protect nature in the absence of payments is hard to justify from an fairness perspective—practitioners claim. Alpízar et al. (2012a,2012b) show that both of them are right; agents that were selected

for their low contributions (for efficiency reasons) contributed more while those that were excluded from payment for their relatively high contributions contributed less.

The results from these experiments confirm the notion that, not only is there a trade-off between efficiency and fairness considerations in PES programmes, but that excessive emphasis on efficiency may cause negative spillover effects, undermining the very aim of the programme. In other words, behavioural responses and preferences for equity and fairness may very well place bounds on the additionality that can be achieved in PES programmes.

Policy discussion and conclusions: what is the proper role for PES in national REDD+ implementation?

In this final section we ask the following question: in light of the evidence presented above, what is the role for PES in implementing REDD+? The first important conclusion emerging from the conceptual framework is that the meagre performance of the national PES programmes in Costa Rica and Mexico is not primarily a result of poor policy design (although this has also diminished policy impact); rather, the fact that the level of additionality is on the order of 1 per cent or less simply reflects that non-compliance with the programme conditions (i.e. deforestation rates) are in the same order of magnitude.

The mirror argument of this, however, is that PES is potentially a cost-effective policy for inducing increased reforestation (which is a component of the plus in REDD+), given that baseline reforestation rates in many tropical countries are low. In fact, this seems to be the experience emerging from the Costa Rican PSA programme (Daniels et al. 2010), although a much smaller share of payments have been gone to reforestation contracts than forest conservation.

The first conclusion is further strengthened by the second: efforts to increase additionality by targeting payments are likely be hampered by spillovers. If PES schemes are targeted to areas with high incidences of forest clearing (e.g. agricultural frontiers) to boost additionality, deforestation pressures may simply shift to other areas (price spillovers/leakage/slippage), or landowners in other areas may start clearing forests because they feel that they are being punished instead of rewarded for protecting their forests prior to the intervention (behavioural spillovers). The same mechanisms

will be at work if payments are targeted to individual applicants based on non-compliance risk. Note also that, even in the absence of these offsetting effects, deforestation risks are inherently difficult to predict (especially if one can only rely on non-manipulative predictors to reduce the risk of moral hazard), and, consequently, the absolute gains from targeting may still be limited (see Figure 8.2).

These results have significant implications for the role that PES can play in implementing REDD+ in countries with tropical forests, given that the essence of REDD+ is performance-based payments. That is, only if a country can show that it has reduced emissions from deforestation and degradation below a given level will it be eligible for receiving REDD+ funds or selling REDD credits. This implies that, if a country selects a nationwide PES scheme as its main REDD policy and that scheme exhibits an additionality of 1 per cent, the payment level either has to be set at 1/100 of the international carbon price (which will most likely provide little incentive for forest conservation), or the country has to provide co-funding for payments to the 99 per cent of landowners who will be paid for doing what they would have done in the absence of the intervention (something that few developing countries' budgets are likely to accommodate).

This does not mean that there is no place for PES in implementing REDD+. It does, however, imply that a nationwide PES programme most likely will not be the main instrument actually realising reduced deforestation and forest degradation, but that this requires a broader set of policies.

This point is not new; others have noted that PES alone will not be sufficient to address the multitude of factors putting pressure on tropical forests, including policies aimed at reducing the profitability of forest conversion (e.g. agricultural policies) and direct regulations strengthening forest protection (e.g. protected areas, land-use planning) (Angelsen et al. 2009, Angelsen et al. 2012). Others have also noted that the institutional prerequisites for PES implementation are lacking in most settings where deforestation is rampant (e.g., Wunder 2009). Despite this, many seem to concur with Seymour and Angelsen that '[i]n the medium to long term, PES schemes are likely to be the implementation instrument of choice' (Angelsen et al. 2009: 299). Our results indicate that this conclusion may need to be rethought: even in the longer run, effective forest conservation needs to rely primarily on other policy instruments.

There are several benefits to implementing PES as a part of a larger set of policies aimed at forest conservation. If the main aim of PES is no longer cost efficiency, broader policy aims, such as poverty alleviation and fairness, can more easily be accommodated, as they no longer necessarily compromise the overall efficiency of the intervention. Other policies and measures at the national level, addressing the causes of deforestation and providing alternative livelihood options, may also reduce the risk of leakage that might otherwise occur in a standalone PES programme (Sunderlin and Sills 2012).

However, if the role of, and motivations for, PES change from cost-effective conservation policy to benefit sharing and increasing the legitimacy of other conservation policies, policymakers need to contemplate whether a PES scheme is the best option for meeting these objectives and be aware of the broader effects that the introduction of a payment scheme may have. Especially as this chapter has highlighted that the introduction of PES may have unintended effects on landowners' intrinsic motivations for forest conservation and, therefore, undermine, rather than build, long-term support for forest conservation. This effect would be unfortunate for two reasons. First, REDD+ is only intended to be a temporary solution; in the longer term, international REDD+ financing will cease, and when this occurs, it is important that the institutions and policies implemented as part of REDD+ can be sustained. The question is whether tropical countries will be willing, and able, to continue large-scale PES schemes when external financing vanishes (this has been, and continues to be, a concern in Costa Rica where the PSA programme was initially funded, to a large extent, by a World Bank grant).

Second, owing to increases in population, incomes and climate policy-induced biofuel demand, demand for land is poised to rise rapidly in the future, putting further pressure on the world's remaining tropical forests (Hertel 2010, Persson 2012). A key aim of REDD+ is, therefore, to help build institutions and policies that are resilient and buttress long-term support for tropical forest conservation.

Notes

[1] However, establishing a baseline to ensure that estimated emission reductions are truly additional is far from straightforward (Persson and Azar 2007).

[2] An exception is the Chinese SLCP, where some involuntary enrolment has been reported (Bennett 2008). Sommerville et al. (2009: 2) argue that, although participation in a PES scheme is voluntary, 'service providers do not necessarily have the choice whether or not to provide the service, such as in cases where land-use change is illegal." However, such restrictions are seldom (if ever) perfectly enforced, and landowners may choose to deforest, even if such an action is illegal. It is estimated that roughly 85 per cent of all tropical deforestation occurs illegally.

[3] The model generates a random sample of 10,000 agents, each representing a potential PES recipient and characterised by the following: (1) the loss in utility from complying with PES conditions ($U_{NC} - U_C$ in Figure 8.1); and (2) the utility derived from PES participation (U_P in Figure 8.1). If the former is positive, agent *i* will not meet the conditions in the absence of payments. Similarly, agent *i* will only apply for payments if the utility derived from doing so is positive and covers the associated opportunity cost (see Figure 8.1). Both agent characteristics are assumed to be normally distributed, with expected means and variances chosen such that a given level of counterfactual compliance with the programme and share of agents applying for payments is achieved. To model selection bias, the two characteristics are set to be correlated, with a correlation coefficient s in the interval [-1,1], such that, if $s < 0$, there is negative selection bias, and, if $s > 0$, there is positive selection bias. The results presented are averaged over 1,000 runs. See Persson and Alpízar (2011) for a full description of the model.

[4] Similarly, in a developed-country setting, Wilson and Hart (2000) found that ranking fourth and fifth among the reasons for enrolling land in AESs in ten European countries were that the requirements either fit well with existing management plans or did not require any changes in them at all. Conversely, the two main reasons for not enrolling land were that it did not fit with farm management plans or that the offered payments were too low.

[5] The difference between AES and PES is largely semantic, and we will therefore refer to both types of schemes as PES here.

[6] Note that this does not presuppose that these perceptions are rational or that the resulting decisions are privately or societally optimal.

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Paper II

Incentivizing versus Rewarding Good Behavior:

Insights on the Use of Monetary Incentives

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Abstract

Payments conditional upon a socially desired behavior, such as blood donations, leaving armed forces, or provision of ecosystem services, are growing in popularity. Due to financial limitations and the need to show results, many of these incentive schemes are selective, resulting in the exclusion of some stakeholders in favor of others. In this paper, we study the possibility of the stakeholders excluded from the monetary incentive reducing their pro-social behavior. We use a laboratory experiment to investigate this and hypothesize that alternative selection rules, i.e., who gets paid and why, affect the overall contributions to a public good differently. Our results show that incentivizing those who acted less pro-socially (i.e., contributed below a certain threshold) before the incentive was introduced resulted in increased contributions to the public good by this group. On the other hand, that very same selection rule excludes those who acted more pro-socially (i.e., contributed over a certain threshold) before the incentive was introduced, and this resulted in decreased average contributions by this group, decreasing the net effect on overall contributions. These results set up an efficiency-fairness tradeoff for designing selective conditional payments to promote pro-social behavior: Targeting those who require incentives to contribute may increase payment response beyond what would have happened in the absence of the incentive program, but it may also give rise to the unexpected consequence of negative spillovers.

Keywords: monetary incentives, conditional payments, economic experiments, behavioral economics.

JEL Classification: C91, D03

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1. Introduction

The use of monetary incentives to promote or reward private behavior that is associated with social and/or environmental objectives is becoming an increasingly popular policy instrument. When the objectives are related to social outcomes (schooling, vaccination, blood donations, etc.), payments are usually called conditional cash transfers (e.g. Fizsbein and Schady 2009). In the environmental literature, such payments are called performance-based payments, with payments for ecosystem services (PES) and more recently REDD payments (Reduced Emissions from Deforestation and Forest Degradation) being the more popular terms used (for a recent review see Pattanayak et al. 2010). In all these cases, incentives (cash or in-kind) are offered conditional on investments in social or environmental capital. Due to limited funds and the need to show results, the implementing authority has to define who qualifies for the incentive, given a set of criteria. Individuals then decide to apply or not, and the authority is requested to choose some and exclude others. In this paper, we refer to such programs as selective incentive programs.

There are a number of real world examples of selective incentive programs. For instance, since 2006 the Colombian government has provided cash payments and free social services, like education, health care, and psychosocial assistance, to individuals who leave the groups involved in armed conflicts (Denissen 2010). Poor communities with low participation in the armed conflicts but high unemployment and limited education opportunities are excluded from the incentive program, which could create incentives for young people to join the paramilitary groups.

In addition, as part of the climate change negotiations, payments for REDD are advised to be designed such that those unlikely to deforest or degrade their forests are excluded from the payment. If only lack of profit from degrading or clearing were the explanation for conserving forest, then perhaps there would be no negative reactions from those excluded from payment. Yet if the decision to conserve is based on pro-social motives too, then being excluded from the incentive program precisely due to pro-social behavior could lead to anger and forest degradation or even, in extreme cases, to deforestation. There is anecdotal evidence from Mexico of landowners threatening to cut down their forests if they do not continue to receive payments for the ecosystem services provided (Kaimowitz 2008).

This problem of excluded individuals potentially decreasing their efforts to achieve social and/or environmental objectives as they feel that they are being punished instead of rewarded

for their high likelihood of acting pro-social in the absence of the monetary incentive program is often not considered in the design of selective incentive programs. The underlying assumption is that excluded individuals will not react to the exclusion, especially if their income and market prices are not changed.

In this paper, we explore this assumption by investigating responses of paid and unpaid individuals to a monetary incentive aimed at encouraging contributions to a public good, and test whether the effects are different under alternative selection rules, i.e., who gets paid and why.

Studies, both in psychology and economics, have shown that monetary incentives sometimes lead to less pro-social behavior once the voluntary act is shifted to a market-based relationship (Frey 1994, Deci et al. 1999, Frey and Jegen 2001). However, this so-called crowding out effect is restricted to individuals offered the incentive. In this paper, we study the possibility of the stakeholders excluded from the monetary incentive reducing their pro-social behavior[1]. Further, it is important to differentiate between negative spillovers of incentive programs that operate through changes in income or prices and spillovers that occur even in the absence of those changes. The former are certainly expected reactions. For example, conservation payments might successfully reduce the amount of land in agriculture, leading to an increase in the value of arable land. This might be enough to change the behavior of some of the excluded landowners, who might then be tempted to degrade their forest or even deforest (e.g., for theoretical work see Wu et al. 2001, for empirical work see Alix-Garcia et al. 2012).

The spillovers that occur in the absence of changes in income and prices are the focus of the present paper. We refer to such negative reactions as *behavioral spillovers*. Behavioral economics suggests various non-neoclassical motivations for such unintended effects—including emotional and distributional concerns outside of "the typical" utility functions, e.g., envy, spite, and inequity aversion (Straub and Murnighan 1995, Pillutla and Murnighan 1996, Fehr and Schmidt 1999, Bolton and Ockenfels 2000, Goel and Thakor 2005, Dur and Glazer 2007). Our aim is, however, not to distinguish among the motivations for potential negative reactions of the unpaid agents but to document behavioral spillovers. If such spillovers appear, and if they shift based on selection rules, then those designing selective incentive

[1] Pro-social behavior, understood as behavior that transcends the narrow definition of a selfish *homo economicus* to include concerns for others at a cost to oneself.

policy may wish to tradeoff direct effects on the paid with the behavioral spillovers of the unpaid.

In reality, both market spillovers and behavioral spillovers are expected to occur simultaneously. To be able to control for other factors that may influence a change in pro-social behavior and to test for potential differences in the level of behavioral spillovers depending on the selection rule in place, we conducted a laboratory experiment. Our experiment has the basic structure of the well-known dictator game in which a player, i.e., the dictator, is given a sum of money to allocate between himself/herself and another player (i.e., the receiver) (e.g., Kahneman et al. 1986, Forsythe et al. 1994, Hoffman et al. 1996). In contrast to the standard dictator game where the recipient is somebody in the room, in our experiment the recipient is a governmental conservation program called Bosque Vivo. The objective of Bosque Vivo is to conserve key forest ecosystems in Costa Rica and as such can be seen as a public good. In this regard, our experiment follows the experience of Eckel and Grossman (2003) and Carpenter et al. (2008).

In our experiment, subjects made three repeated allocation decisions, and the basic design of the dictator game changes gradually to simulate a situation where first there is no existing institution (Round 1), and then an institution in the form of a regulator is introduced (Round 2) and made operative by choosing the selection rule by which the receivers of an incentive for contributions to the public good is decided (Round 3). The following selection rules were tested[2]: [1] *additionality rule* – select those with low (i.e., below a threshold) contributions to the public good in the second round; [2] *reward rule* – select those with high (i.e., above a threshold) contributions to the public good in the second round; and [3] *random rule* – select those who won a lottery. There were also sessions where no incentive was introduced, i.e., the control treatment. To explore behavioral spillover we use a difference-in-difference approach: The effect of each selection rule is related to the allocation in Round 2, and this difference is compared with a situation where no selection rule or incentive is introduced in Round 3.

Our results for over 400 university students from the University of Costa Rica provide empirical evidence of motivation to act pro-socially being hampered upon exclusion from a monetary incentive, even without any change in prices or income. However, this behavioral spillover is found to be highly dependable on the reasoning behind exclusion, i.e., the

[2] The labels given here for these rules are intended to be descriptive. We did not use them with the participants, in order to not generate any signals about expected behavior. The specific script used is presented in the Appendix.

selection rule. As expected, incentivizing those who acted less pro-socially before the incentive was introduced resulted in increased pro-social behavior among these individuals, i.e., increased contributions to the public good. However, this selection rule excluded those who acted more pro-socially before the incentive was introduced, which reduced pro-social behavior in this group and decreased overall contributions. In contrast, neither rewarding past pro-social behavior (i.e., contributions over a certain threshold) nor randomly selecting subjects for payment leads to negative behavioral spillovers from exclusion.

These results set up an efficiency-fairness tradeoff for designing selective conditional payments to promote pro-social behavior. Targeting those who require incentives to contribute may increase payment response beyond what would have happened in the absence of the incentive program, but it may also give rise to the unexpected consequence of behavioral spillover. These results are strengthened by similar results from a field experiment with almost 350 Costa Rican landowners (Alpízar et al. 2013). Moreover, we find that the mere introduction of an authority, whose payoff depends on the outcome, does not affect contributions to the public good.

Do note that this unexpected consequence of behavioral spillover from selective incentives is found in a context with a high degree of pro-social concerns, i.e., contributions to an environmental fund protecting and restoring tropical forest in Costa Rica. Therefore, since the presence of pro-social concern varies considerably depending on the context, any general conclusions would require further research to investigate behavioral spillover in cases where the behavior is less motivated by pro-social concerns.

The remainder of this paper is organized as follows. Section 2 describes the experimental design, the modified dictator game, and the sample. Section 3 presents our findings. In Section 4, we discuss our findings and conclude with the implications for the design of selective incentive policies.

2. The experimental design

The basic structure of our experiment is a dictator game where a player, i.e., the dictator, is given a sum of money to allocate between himself/herself and a governmental conservation program called Bosque Vivo, following the experience of Eckel and Grossman (2003) and Carpenter et al. (2008). The objective of Bosque Vivo is to conserve key forest ecosystems in Costa Rica and as such can be seen as a public good.

In our experiment, subjects make three repeated allocation decisions. In the first round, we obtained information about the subject's type, i.e., whether the dictator shares a large or small amount with the recipient. Right before the second round, a regulator was randomly selected from the pool of subjects at a rate of one regulator for circa ten dictators. The regulator remained anonymous to the dictators throughout the game. The subjects kept their roles through the rest of the experiment. The presence of the regulator is an important element of realism in our experiments. Incentive programs like the ones described in the introduction require an institutional framework, be it a government agency, a non-governmental institution or an international organization, that administers the funds and chooses who receives the incentives. In practice, these institutions become the face of the program, and its success obviously depends on the success of the incentive program in achieving the desired target of pro-social behavior. In our experiment, the regulator's payoff therefore depended on the total contributions to Bosque Vivo by the subset of subjects he/she is regulating. The payment to the regulator comes from a source exogenous to the game to keep the funds available to the dictator and recipient constant between rounds. This payment structure is known by all dictators, and gives them a mechanism to "punish" or "reward" the regulator via payoffs. If the dictator wanted to punish the regulator, this could be done by lowering contributions, but such an act would also result in lower giving to the receiver, i.e., Bosque Vivo. This is precisely the type of trade-off that excluded individuals would face in reality. For example, the performance of blood banks is highly dependent on people's voluntary blood donations. If an individual for some reason is not happy with the way the institution works, he/she might decide not to donate blood, which also comes at a social cost.

The role of the regulator was explained before the dictators made their second allocation decision. However, the regulator played no role in this round, except that his/her payoff depended on the total contributions. In the third and last round, the regulator became operative and chose the selection rule that decided which of the dictators would receive an incentive to share an amount of money with the recipient.

Treatments - Selection rules

In each round, dictators received 10 tokens, each worth 1000 colones[3], which had to be allocated between himself/herself and the recipient (Bosque Vivo). As noted above, the main task of the regulator was to choose which selection rule to apply in assigning incentives to some but not all dictators. Three selection rules were tested. The first rule, which we here—not in the script—call *additionality rule*, selects subjects that contributed less than three tokens (2,000 colones or less) in Round 2, and the rest were excluded. Those selected qualified for an incentive of 50% of their contribution in Round 3 to be paid back to them with funds external to the initial allocation of funds.[4] This rule is intended to encourage additional contributions to Bosque Vivo from those who gave little in the previous round. In standard dictator games, average giving is around 20% of the endowment (see Camerer 2003 for a review), hence our threshold. The *additionality rule* is the standard recommendation given to incentive programs aiming to increase contributions on top and beyond what would happen in the absence of the incentive—a concept called additionality (e.g., Rawlings and Rubio 2005, Angelsen 2008).

The second rule, here called the *reward rule*, selects subjects that contributed more than five tokens (6,000-10,000 colones). In other words, those who contributed more than 50% of their endowment in Round 2 were rewarded with the same incentive as described above.

These two selection rules are based on past behavior, either rewarding those who gave a lot or encouraging those who gave little to give more in the future. The third rule—here called the *random rule*—selects subjects for the incentive randomly with a 50% chance, thereby delinking selection from past behavior. This allows us to test whether such selection leads to more or less behavioral spillovers than selection rules based on past behavior.

[3] The exchange rate at the time of the experiment was 500 colones= $1, i.e., each dictator received 20 US dollars, which is a substantial amount for a Costa Rican university student. For example, 20 US dollars covers around five lunches at the university cafeteria. High stakes were used to increase the saliency of the experiment. However, we note that Kocher et al. (2008) did not find a significant stake effect for contributions to public goods.

[4] In the framing of our experiment, we made sure that subjects perceived the incentive as a payment linked to their past action and not as seed money or matching funds (e.g., List and David Lucking-Reiley 2002). Payments were given to the selected dictators, and not the receiver. Although in practical terms the effect on public good funding is similar, our approach brings us closer to the practical implementation and design of incentive programs (see Eckel and Grossman 2003 on the importance of framing of charitable giving).

All selection rules were available in all sessions of the experiment. Before the subjects made their contribution decision in the third round, the selection rules were explained and each subject privately learned about the selection rule applicable in his/her case, and whether he/she qualified for the incentive. The subjects were also instructed to carefully read the text that explained why they were selected or rejected for the incentive. Note that since all actions were visible to the experimenter at all times, there may have been an "experimenter effect" on subject behavior (e.g., Hoffman et al. 1996, Zizzo 2010). However, our difference-in-difference approach should take care of such concerns.

Importantly, we also conducted sessions where Round 3 included a regulator, but no incentive or selection rule. This control treatment provides us with further means of comparing the difference between Rounds 2 and 3 with and without selection rules, thereby comparing not only one rule with another, but also a given rule with no rule.

To explore behavioral spillover we use a difference-in-difference (DiD)[5] approach: The effect of each selection rule is related to the allocation in the second round, and this difference is compared with a situation where no selection rule or incentive is introduced in Round 3. This DiD approach results in an across-subjects comparison of changes in behavior resulting from the introduction of the incentive and the selection rule. It also controls for the expected decrease in sharing observed in repeated games and the effect that introducing another receiver (i.e., the regulator) into the game may have on contributions to the public good (see recent results from meta-regressions by Engel 2011). In addition, to control for learning/order effects, which occur when prior experience with one task affects the behavior in sequential tasks, some sessions were conducted with only two rounds of the basic dictator game, i.e., Round 1.

[5] Since the work by Ashenfelter and Card (1985), the use of difference-in-difference methods has become very widespread. In the simplest setup, outcomes are observed for two groups for two time periods. The first group is the group exposed to a treatment in the second period but not in the first. The other group, the control group, is not exposed to the treatment in either period. When the same units within a group are observed in each time period, the average gain in the control group is subtracted from the average gain in the treatment group.

Pay-offs

At the beginning of the game, subjects were instructed that one of the three rounds would be randomly selected for payment. This was to avoid an income effect since if paid for each round, subjects would become richer and presumably increase giving over rounds.

The dictator's payoff was determined by the dictator's own decision with the following payoff function: $\pi_{dictator} = 10 - g_{ir}$, where g_{ir} is giving by dictator *i* in round *r*. If selected for the incentive in Round 3, the pay-off to the dictator was given by $\pi_{Sdictator} = 10 - g_{ir} + P$, where $P = \frac{g_{ir}}{2}$, i.e., the cost of giving is cut in half. Funds to cover this payment were provided by the experimenter.

The pay-off to Bosque Vivo (the recipient) in round *r* is decided by the total contributions of each dictator *i* as follows: $\pi_{recipient} = \sum_{i=1,...N}^{N} g_{ir}$, where *N* is the total number of dictators in a given session. The total amount donated to the recipient in the drawn round was transferred directly via the internet.

The regulator's pay-off was made endogenous as we wanted to give the dictators the possibility to punish their regulator directly, albeit at some social cost. Therefore, the regulator's payoff function depends on the total giving of the dictators in his/her group:

$$\pi_{regulator} = \frac{\sum_{i=1,...n}^{n} g_{ir}}{n}$$

where *n* is the total number of dictators under the rule of a given regulator and g_{ir} is the contribution of each dictator *i* in round *r*. Funds to pay the regulator were provided by the experimenters, leaving the initial endowment of dictators and recipients unaltered between rounds. Do note that introducing a regulator in Round 2 with an endogenous pay-off function increases the prospective benefits of donating compared with Round 1, as higher giving to the recipient also resulted in a higher payoff for the regulator. By focusing on a comparison between Rounds 2 and 3, we control for this effect.

After the second and third rounds, regulators received a notification regarding their pay-off, while dictators received a notification of gratitude for their participation. In the end of each session, the actual contributions to Bosque Vivo were made on site via the internet and the subjects were paid their earnings individually upon signing a valid receipt.

Experimental procedure

We conducted a paper and pen experiment with students at the University of Costa Rica (Costa Rica). The students were paid a fixed amount of 2,500 colones[6] for participating in the experiment. We recruited the subjects by distributing flyers and then had them sign up on a participation list before each session on a first-come, first-serve basis. In each session there was room for a maximum of thirty subjects. All subjects had to show their university identification to ensure that they had not participated before.

The laboratory experiment was applied to 438 students at two campuses of the University of Costa Rica in March-June 2011 and June 2012. We conducted 16 sessions, each lasting 1-1.5 hours and involving 15-30 students.

The instructions were given orally using a Power Point presentation to make the instructions clear and easy to follow.[7] Before the session started, students were informed that the session would take about one hour. Subjects were asked for their informed consent and were given the option to leave the room. The decisions were anonymous to other players. At the end of the experiment, the subjects were asked to complete a questionnaire.

3. Results

To investigate the presence of behavioral spillovers, we use a difference-in-difference (DiD) approach. This approach accounts for the multiple-round structure of our experiment, where contributions could rise or fall across rounds for reasons other than the introduction of an incentive. The approach also takes care of potential differences in the two subsamples of students in both campuses[8] by strictly focusing on differential treatment effects and not absolute contributions. Therefore, we look at the within-subject change in behavior from one round to another for the subsample where a given treatment was applied and compare it with the change in behavior observed in the subsample that faced the control treatment, i.e., where a regulator was introduced but no incentives were handed out.

[6] At the time of the experiment, 500 colones = $1.

[7] The English translation of the script is presented in the Appendix. The original version of the script, in Spanish, is available upon request.

[8] We find a significantly higher average contribution of students from the capital city compared with students from a more rural setting (p-value<0.01; Mann-Whitney test).

In effect, the DiD approach compares two subsamples: one subsample consisting of individuals who had been selected/excluded from the incentive due to a certain selection rule and another subsample consisting of individuals in the control treatment. Since the control treatment subsample consisted of both high and low givers, one could suspect that these two different types of givers would behave differently between the rounds (cf., Alpízar et al. 2013 where such pattern was found and hence tailor made control groups were constructed). This is important to test for as the *additionality rule*[9] excludes those who donated more than two tokens, whereas the *reward rule* excludes those who contributed less than six. We tested whether high and low givers in the control treatment behaved significantly different, and found no significant differences in their contributions between the second and third rounds in the control treatment.[10] This allows us to always use all observations in the control treatment to compare them with the alternative treatments.

To analyze the full complexity of the data, we will follow three complementary strategies. We will: i) look at the DiD of behavior of excluded and selected individuals when subjected to the three selection rules; ii) explore differences in the share of excluded subjects reacting positively or negatively to the selection rules, and also the average size of the loss or gain in each case; and iii) use an econometric framework to explore differences in reactions to the rules accounting for the level of initial contribution, for both excluded and selected individuals. Finally, we will look at the overall effect of each selection rule.

A first stage in our analysis is to determine whether the mere introduction of a regulator[11] creates incentives to change behavior in the games. If such an effect exists, it could be a result of anticipated incentives, or a rejection of regulatory power per se. Comparing Round 1

[9] In this paper, additionality refers to outcomes that would not have occurred in the absence of the incentive program. Accordingly, the additionality rule refers to a rule that encourages contributions that would not have occurred in the absence of the incentive, as expected based on observed past behavior.

[10] Looking at the control treatment rounds, when compared with the rest of the players, we find no significant difference in the behavior of high givers, i.e., those who would have been excluded under the additionality rule, (p-value=0.79; Wilcoxon test). The result is similar for low givers, i.e., those who would have been excluded under the reward rule, (p-value=0.68; Wilcoxon test).

[11] As we let the regulators choose the selection rule, the number of observations in each treatment is a reflection of the preferences of the regulators. Accordingly the number of observations for each selection rule is unbalanced. We find that 41% of the regulators selected the additionality rule, 41% the reward rule, and 18% the random rule. We also find that regulators who contributed more than five in the first round, i.e., before their appointment, tend to prefer the reward rule (p-value=0.07; chi-square test).

without and Round 2 with a regulator, we find a small but significant decrease of 0.08 tokens (80 colones) (p-value=0.05; Wilcoxon test). To separate the regulator effect from an order/learning effect, we analyze the data collected in a control game where neither regulator nor incentive was introduced, i.e., Round 1 was played twice. We find no order effect (p-value=0.30; Wilcoxon test), and when controlling for the order effect by taking the difference-in-difference between the regulator and no regulator treatments, we find no significant regulator effect (p-value=0.80; Mann-Whitney test). However, we do find a redistribution of contributions, with some staying neutral and others increasing or decreasing their contribution. To control for this potential regulator effect, Round 2 is used as our baseline for the rest of the analysis.

To recapitulate, the treatments entail announcing the selection rule chosen by the regulator and privately informing each subject whether or not he/she qualifies for the incentive; the subject then decides how much to contribute. The additionality and reward selection rules are based on past contributions, so the share of excluded dictators for each treatment is endogenous: For the additionality rule 42% were excluded, for the reward rule 87% were excluded, and for the random rule 52% were excluded. The subjects did not know the share of excluded (or selected) dictators at any point in time.

3.1 Behavioral spillovers upon exclusion.

Table 1 presents the average contributions for all excluded subjects when subjected to a given treatment in two subsequent rounds. It also shows the differences between these rounds and the DiD estimation, i.e., the treatment effect when compared to the control.[12] We find that subjects in the control treatment, i.e., where no incentive was introduced, on average did not significantly change their contributions over the second and the third rounds (p-value=0.47; Wilcoxon test). Similarly, for subjects excluded under the three selection rules, we find no significant average change in contributions between the rounds (additionality rule p-value=0.20, reward rule p-value=0.32, and random rule p-value=0.14; Wilcoxon test).

[12] Table A1a in the Appendix presents the results where the control consisted of tailor-made control groups, constructed for each rule to represent those (in the control treatment subsample) who would have been excluded from the incentive had the rule applied to them. The results show that the magnitudes of the behavior spillover are similar in both tests, even though the values are insignificant due to small sample size.

To isolate the treatment effect we look at the DiD results where the difference in the control treatment is subtracted from the differences under each selection rule. Excluded subjects under the additionality rule, i.e., those who were initially giving large contributions, on average reacted negatively, with a significant overall reduction of 0.62 tokens (620 colones) (p-value=0.07; one-tailed Mann-Whitney test).[13] This negative reaction, which happens in the absence of changes in income or prices, we call behavioral spillover. This behavioral spillover is also significantly different compared with both the reward rule and the random rule (p-value=0.10 and p-value=0.06 respectively; Mann-Whitney test). Importantly, we find no significant behavioral spillover for the reward rule (p-value=0.42; one-tailed Mann-Whitney test) or the random rule (p-value=0.11; one-tailed Mann-Whitney test).

Table 1. Average contributions and treatment effects of **excluded** subjects for each selection rule.

	Control treatment	**Additionality rule**	**Reward rule**	**Random rule**
		Excluded Round 2 contribution > 2	Excluded Round 2 contribution<6	Excluded Lost lottery
Round 2	1.58 tokens (99 obs)	5.20 tokens (50 obs)	2.40 tokens (107 obs)	1.69 tokens (26 obs)
Round 3	1.70 tokens (99 obs)	4.70 tokens (50 obs)	2.62 tokens (107 obs)	2.00 tokens (26 obs)
Difference in contributions	+0.12 tokens	-0.50 tokens	+0.22 tokens	+0.31 tokens
Behavioral spillover Treatment effect (DiD)		**-0.62 tokens***	**+0.09 tokens**	**+0.19 tokens**

***=significant at 1%, **=significant at 5% , *=significant at 10%, according to a Wilcoxon test for the within-subject comparisons (H_0: Round 2 contribution=Round 3 contribution) and a one-tailed Mann-Whiney test for the between-subject comparisons (H_0:DiD≥0) to test the hypothesis that there is a negative effect, i.e., negative behavioral spillover, upon exclusion from the incentive.

As always, average information hides particular patterns of behavior, which we think are important to truly understand behavioral spillovers from the different selection rules. Therefore, in Table 2 we analyze the heterogeneity in responses for excluded subjects. To

[13] Even though we use non-parametric tests to account for the small sample size, t-tests were also used and the results remain basically unchanged.

begin with, we observe a large share of individuals reacting either positively or negatively to the fact that they are asked to contribute to the public good a third time (i.e., the control). The distribution of reactions between the excluded and the control is, however, not significantly different for any of the treatments (chi2 p-value=0.27)[14]. Further, we find no significant difference in the distribution of reactions between the selection rules.[15]

However, looking only at the share of negative responses, we find a significantly larger share of negative reactions to exclusion from the incentive under the additionality rule; i.e., excluding those who contributed more than two tokens (2,000 colones). This is true both compared with the share of negative reactions under the control (chi-square p-value=0.05) and compared with the reward and the random rule (chi-square p-value=0.05 and p-value=0.06 respectively). The magnitude of the average loss is also significantly larger under the additionality rule, with an average decrease of 2.33 tokens (2,330 colones) compared with the loss of around 1.5 tokens (1,500 colones) in the control and under the other selection rules (p-value=0.02; Mann-Whitney test).

Table 2: Percentage of subjects reacting positively or negatively to **exclusion** from the incentive depending on alternative selection rules and average size of loss or gain in tokens.

	Control treatment	Additionality rule	Reward rule	Random rule
Pos. reaction	25% (25 out of 99) +1.52 tokens	26% (13 out of 50) +1.31 tokens	27% (29 out of 107) +2.00 tokens	39% (10 out of 26) +1.50 tokens
Neg. reaction	21% (21 out of 99) -1.24 tokens	36% (18 out of 50) -2.33 tokens	22% (23 out of 107) -1.52 tokens	15% (4 out of 26) -1.75 tokens
No reaction	54% (53 out of 99) 0 token	38% (19 out of 50) 0 token	51% (55 out of 107) 0 token	46% (12 out of 26) 0 token

[14] This p-value refers to a comparison between all treatments including the control. Comparing the distribution of reactions in each treatment with the control we get the following: The control to the additionality rule, chi2 p-value=0.11; the control to the reward rule, chi2 p-value=0.95; the control to the random rule, chi2 p-value=0.40.

[15] Comparing the distribution of reactions between the selection rule treatments, we get the following: the additionality rule to the reward rule, chi2 p-value=0.13; the additionality rule to the random rule, chi2 p-value=0.16; the reward rule to the random rule, chi2 p-value=0.49.

Turning to the positive side, those who reacted positively to exclusion under the reward rule, i.e., those who contributed less than six tokens (6,000 colones) in the previous round, significantly increased their contributions by an average of 2 tokens (2,000 colones), which might be a result of the information about the socially desired behavior contained in the reward rule (p-value=0.08; Mann-Whitney test).

Finally, we run a simple OLS to explore whether observed differences in behavior were a reflection of our treatments, even after controlling for the type of contributor revealed in Round 1. First we use a linear specification that uses absolute contributions in Round 1 as explanatory variable, but in a third regression we also allow for a non-linear relation, by using dummies to trace the behavior in Round 1. Regression 1 in Table 3 is a replication of the result in Table 1, so we find a significant decrease in contributions due to exclusion based on the additionality rule. The significance of this effect holds in the other two econometric specifications, even after controlling for type of player.

Our results show that being excluded under the additionality rule reduces contributions by on average 1.01 tokens (1,010 colones) when controlling for linear initial contributions and by 0.89 tokens (890 colones) when controlling for increasingly larger initial contributions. Further, higher contributions in the first round give a significant, yet small, increase in the difference in contribution between the second and third rounds.

Table 3: Regression results for **excluded** subjects compared with the control. Dependent variable is difference in contribution between Round 3 and Round 2.

Coefficients	Regression 1 Without controlling for other variables (p-value)	Regression 2 Controlling for contributions in round one – linear (p-value)	Regression 3 Controlling for contributions in round one – non-linear (p-value)
Intercept (i.e., behavior in the control treatment)	0.12 (0.40)	-0.09 (0.58)	0.01 (0.93)
Additionality rule	-0.62****** (0.01)	-1.01******* (<0.01)	-0.89******* (<0.01)
Reward rule	0.09 (0.64)	-0.04 (0.85)	-0.01 (0.98)
Random rule	0.18 (0.55)	0.16 (0.61)	0.19 (0.55)
Contributions in Round 1		0.13***** (0.01)	
Baseline $0 \leq g \leq 2$			
Dummy if $2 < g \leq 5$			0.36***** (0.06)
Dummy if $g > 5$			0.60*(0.09)
Number of observations	282 obs.	282 obs.	282 obs.
R-square	0.04	0.06	0.05

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

3.2 Reaction to receiving the incentive.

For the selected subjects, the averages and the DiD treatment effect under each selection rule treatment are presented in Table 4.[16] As expected, the incentive significantly increases contributions over rounds for all rules except the reward rule. This insignificant change in contribution for selected subjects under the reward rule (p-value=0.25; Wilcoxon test) is expected since this rule carries very little room for additionality as it selects those who already contributed a high amount, i.e., strictly more than 5,000 colones. Using the DiD estimate with respect to the behavior of subjects in the control treatment, we find that the reward rule achieves no significant change; in other words, funds spent incentivizing subjects using the reward rule yield no change in contributions.

For subjects selected under the random rule, i.e., those who were lucky to be selected, we find a significant average increase in contributions of 1.33 tokens (1,330 colones) between the second and third rounds (p-value<0.01, Wilcoxon test). Moreover, when compared with the control treatment, the random rule yields a strongly significant increase—the largest of the three rules—in contributions (p-value<0.01; one-tailed Mann-Whitney test).

For subjects selected under the additionality rule, i.e., those who acted less pro-socially in Round 2, we find a significant change in contributions between the rounds, with an average increase of 0.90 tokens (900 colones) (p-value<0.01; Wilcoxon test). The DiD estimate is significant; subjects under the additionality rule on average reacted positively, with an average increase of 0.78 tokens (780 colones) (p-value<0.01; one-tailed Mann-Whitney test). Note that the observed performance of the additionality rule and the reward rule for selected subjects in this experiment is precisely the justification used by economists to recommend the former and discourage the latter in the design of incentive programs.

[16] Table A1b in the Appendix presents the results when the control consisted of tailor-made control groups, constructed for each rule to represent those (in the control treatment subsample) who would have been selected for the incentive had the rule applied to them. The results show that the magnitudes of the additionality and the significance are similar in both tests.

Table 4. Average contributions for control and each selection rule for **selected** subjects.

	Control treatment	**Additionality rule**	**Reward rule**	**Random rule**
		Selected Round 2 contribution < 3	Selected Round 2 contribution > 5	Selected Won lottery
Round 2	1.58 tokens (99 obs)	0.87 tokens (70 obs)	6.81 tokens (16 obs)	3.13 tokens (24 obs)
Round 3	1.70 tokens (99 obs)	1.77 tokens (70 obs)	7.31 tokens (16 obs)	4.46 tokens (24 obs)
Difference in contributions	+0.12 tokens	+0.90 tokens***	+0.50 tokens	+1.33 tokens***
Additionality Treatment effect (DiD)		**+0.78 tokens*** **	**+0.38 tokens**	**+1.21 tokens*** **

***=significant at 1%, **=significant at 5% , *=significant at 10%, according to a Wilcoxon test for the within-subject comparisons (H_0: Round 2 contribution=Round 3 contribution) and a one-tailed Mann-Whiney test for the between-subject comparisons (H_0:DiD≤0) to test the hypothesis that there is a positive effect, i.e., additionality, upon selection for the incentive.

Similar to the analysis of excluded subjects, we run a simple OLS to explore whether observed differences in behavior were a reflection of our treatments, even after controlling for the type of contributor revealed in Round 1. Regression 1 in Table 5 is a replication of the results in Table 4. We find a significant increase in contributions due to selection based on the additionality rule and the random rule. The significance of this effect holds in the other two econometric specifications, even after controlling for type of player.

Our results show that being selected under the additionality rule increased contributions by an average of 0.08 tokens (80 colones) when controlling for linear initial contributions and by 0.81 tokens (810 colones) when controlling for increasingly larger initial contributions. Further, higher contributions in the first round imply a significant, although small, increase in the difference in contribution between the second and third rounds.

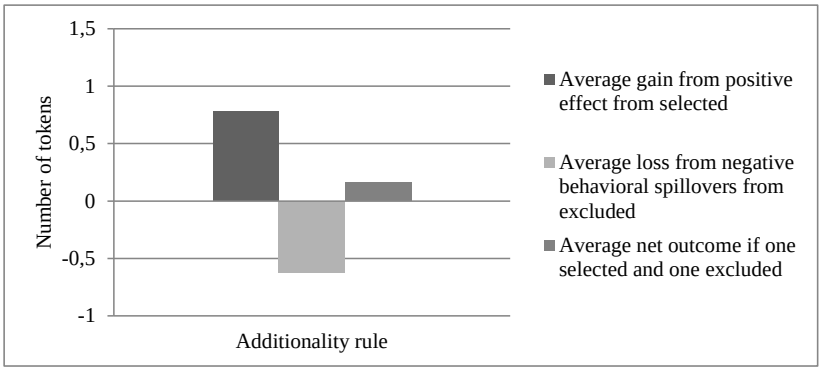
Table 5: Regression results for **selected** compared with control. Dependent variable is difference in contribution between Round 3 and Round 2.			
Coefficients	Regression 1 Without controlling for other variables (p-value)	Regression 2 Controlling for contributions in round one – linear (p-value)	Regression 3 Controlling for contributions in round one – non-linear (p-value)
Intercept (i.e. behavior in the control treatment)	0.12 (0.40)	-0.07 (0.70)	0.06 (0.72)
Additionality rule	0.78*** (<0.01)	0.08*** (<0.01)	0.81*** (<0.01)
Reward rule	0.38 (0.64)	-0.05 (0.91)	0.06 (0.89)
Random rule	1.21*** (<0.01)	1.05*** (<0.01)	1.14*** (<0.01)
Contributions in Round 1		0.11** (0.05)	
Baseline $0 \leq g \leq 2$			
Dummy if $2 < g \leq 5$			0.17 (0.51)
Dummy if $g > 5$			0.60 (0.18)
Number of observations	209 obs.	209 obs.	209 obs.
R-square	0.09	0.11	0.10

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

3.3 Overall effect of selective incentives

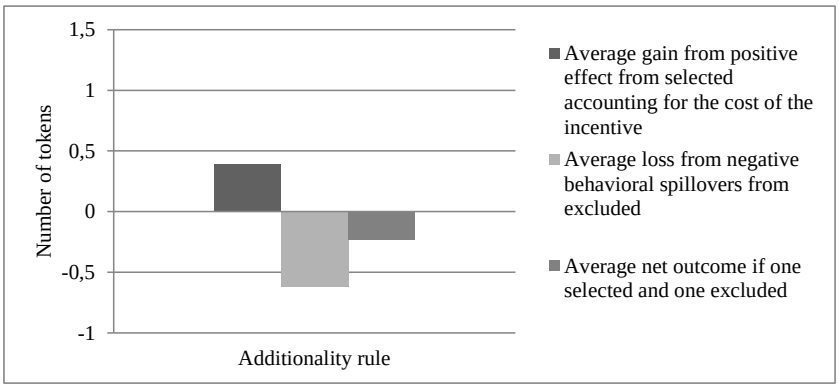
Our results suggest that both behavioral spillovers—negative effects on contribution upon exclusion from an incentive—and additionality—positive effects on contribution upon selection for an incentive—occur for *only one* of the three selection rules: the "additionality" rule. Thus, when investigating the overall impact of targeted incentives, i.e., the magnitude of the average gain per person selected and the average loss per person not selected, we only do so under the additionality rule. The results, illustrated in Figure 1, affirm that the potential gains from incentives targeting those with prior low contributions are lost when accounting for the behavioral reaction of excluded subjects. The average net outcome of 0.16 tokens in contributions is insignificant (p-value=0.55; Mann-Whitney test). In this case, the behavioral spillover decreases the potential gains from incentivizing contributions to the public good (i.e., forest conservation) by almost 80%. This leads to an insignificant increase in overall outcome on average. In evaluating a selection rule, we show that the responses by those who are excluded can matter drastically.

Figure 1. The effect of selection or exclusion under the additionality rule and the net outcome if comparing one selected and one excluded individual. The net outcome under the additionality rule is not significant, according to a two-tailed t-test p-value=0.55.



From a policy point of view, the cost of paying incentives also has to be considered when measuring the net effect of targeted incentives. Figure 2 illustrates the case where the cost of the incentive is subtracted from the average gain per person selected. The net outcome then becomes an insignificant loss of -0.23 tokens (p-value=0.31; Mann-Whitney test). These results show that incentives targeting low contributions might result in an overall zero outcome when the cost of the incentive is incorporated. An overall zero outcome is also found for the reward rule (p-value=0.79; Mann-Whitney test), whereas we find a significant overall positive outcome for the random rule (p-value=0.07; Mann-Whitney test).

Figure 2. The effect of selection or exclusion under the additionality rule and the net outcome if comparing one selected and one excluded individual accounting for the cost of the incentive. The net outcome under the additionality rule is not significant, according to a two-tailed t-test p-value=0.31.



Clearly, the total net effect of using the additionality rule would depend on the shares of included and excluded subjects. At the extreme, if almost all subjects are included, then the few disgruntled individuals are not likely to reduce the gains from additionality. But if the share of excluded subjects is large, behavioral spillovers might lead to a significant decrease in net gains from the additionality rule. In our experiment, the subjects were never informed about the share of excluded or selected subjects. Hence, we cannot comment upon such an effect based on our result. In reality, one would assume that the availability of such information varies depending on context.

4. Conclusions

This paper provides empirical evidence that stakeholders excluded from a monetary incentive that benefits others tend to act less pro-socially than before the incentive was introduced. This unintended effect of exclusion, which occurred even without any change in prices or income, i.e., what we call a behavioral spillover, is found to be highly dependent on the selection rule that defines who qualifies for the incentive. As expected, incentivizing those who acted less pro-socially before the incentive was introduced resulted in increased pro-social behavior, i.e., increased overall contributions to the public good, by those selected for the incentive. Yet those excluded under the very same selection rule significantly decreased overall contributions.

In contrast, neither rewarding past pro-social behavior (i.e., contributions over a certain threshold) nor randomly selecting subjects for payment leads to negative behavioral spillovers upon exclusion. The reward rule, which notably is the most popular first choice of agencies in charge of selective incentive programs, yields no additional contributions from those selected to receive the incentive. In other words, in terms of increasing the provision of the public good, the reward rule achieves nothing, and hence from that perspective alone can be regarded as a waste of funds. We do note that conditional incentive programs often have redistribution of income as a second objective.

These results imply an efficiency-fairness tradeoff in the design of selective conditional incentive programs aimed to promote social objectives through private actions. The standard recommendation from an efficiency standpoint has been to give the incentives to those who in the absence of the incentive program would make decisions that are contrary to the social

objective. We show that this approach indeed manages to change the behavior of those selected for payment. Unfortunately, by excluding those who do show a pro-social attitude even in the absence of the incentive program, such a selection rule runs the risk of alienating them. In our experiment, this results in reduced contributions. Obviously, the net effect depends on the shares of subjects selected and excluded, which in turns depend on both the baseline conditions (compliance with program objectives in the absence of the program) and the available budget. In this paper, we simply raise attention to the need to consider the efficiency-fairness balance of any incentive program.

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APPENDIX

Table A1a. Average contributions for each selection rule and each control for **excluded** subjects.

	Additionality rule	**Reward rule**	**Random rule**
	Excluded Round 2 contribution>2	Excluded Round 2 contribution<6	Excluded Lost lottery
Round 2	5.20 tokens (50 obs)	2.40 tokens (107 obs)	1.69 tokens (26 obs)
Round 3	4.70 tokens (50 obs)	2.62 tokens (107 obs)	2.00 tokens (26 obs)
Difference in contributions	**-0.50 tokens**	**+0.22 tokens**	**+0.31 tokens**
	Control Round 2 contribution>2	Control Round 2 contribution<6	Control Full sample
Round 2	4.25 tokens (24 obs)	1.45 tokens (97 obs)	1.58 tokens (99 obs)
Round 3	4.21 tokens (24 obs)	1.54 tokens (97 obs)	1.70 tokens (99 obs)
Difference in contributions	**-0.04 tokens**	**+0.08 tokens**	**+0.12 tokens**
Behavioral spillover Treatment effect (DiD)	**-0.46 tokens**	**+0.13 tokens**	**+0.19 tokens**

***=significant at 1%, **=significant at 5% , *=significant at 10%, according to a Wilcoxon test for the within-subject comparisons (H_0: Round 2 contribution=Round3 contribution) and a one-tailed Mann-Whiney test for the between-subject comparisons (H_0:DiD≥0) to test the hypothesis that there is a negative effect, i.e., negative behavioral spillover, upon exclusion from the incentive.

Table A1b. Average contributions for each selection rule and each tailor-made control for **selected** subjects.

	Additionality rule	**Reward rule**	**Random rule**
	Selected Round 2 contribution<3	Selected Round 2 contribution>5	Selected Won lottery
Round 2	0.87 tokens (70 obs)	6.81 tokens (16 obs)	3.13 tokens (24 obs)
Round 3	1.77 tokens (70 obs)	7.31 tokens (16 obs)	4.46 tokens (24 obs)
Difference in contributions	**+0.90 tokens*** **	**+0.50 tokens***	**+1.33 tokens*** **
	Control Round 2 contribution<3	Control Round 2 contribution>5	Control Full sample
Round 2	0.72 tokens (75 obs)	7.5 tokens (2 obs)	1.58 tokens (99 obs)
Round 3	0.89 tokens (75 obs)	9.5 tokens (2 obs)	1.70 tokens (99 obs)
Difference in contributions	**+0.17 tokens**	**+2 tokens**	**+0.12 tokens**
Additionality Treatment effect (DiD)	**+0.73 tokens*** **	**-1.50 tokens**	**+1.21 tokens*** **

***=significant at 1%, **=significant at 5% , *=significant at 10%, according to a Wilcoxon test for the within-subject comparisons (H_0: Round 2 contribution=Round3 contribution) and a one-tailed Mann-Whiney test for the between-subject comparisons (H_0:DiD≤0) to test the hypothesis that there is a positive effect, i.e., additionality, upon selection for the incentive.

Script [only read text in small and cursive letters]

RESEARCHER: *Good afternoon/Good morning; welcome to this workshop, we thank you for your participation. My name is _____, my colleagues Ana and Mille will also join us.*

We are carrying out research from CATIE. CATIE is the center for Tropical Agricultural Research and Education located in Turrialba. Today we are going to carry out a series of games, in which you will make decisions on situations that resemble reality. There are no correct or incorrect answers; all the information collected is confidential.

Our objective is to learn from these decisions, so we request that you please pay attention and dedicate time to your answers. The session will last approximately one hour. During this game you will be able to earn money. The amount will depend on your decisions; we will explain this shortly.

[PAUSE] *Do you have any questions?* [WAIT FOR AN ANSWER].

RESEARCHER: *At this time we would like you to confirm that you can dedicate an hour to the game. It would be unfortunate if someone leaves before finishing. As a sign of gratitude, to all of those who remain until the end, we will pay a minimum of 2,500 colones.*

[GIVE TIME TO THINK AND ALLOW PEOPLE TO LEAVE IF THEY WISH TO. BEFORE STARTING, BE SURE TO]:

- If someone cannot participate the whole hour, please request that they leave. Listeners are not allowed.
- Reorder the groups, so they will be balanced. Do not allow them to reorganize themselves.

RESEARCHER: [START POWER POINT PRESENTATION]

☼ **[SLIDE 1]**

RESEARCHER: *Thanks for staying with us. We are going to begin. At this time, we request that you please turn off your cell phones.* [ALLOW TIME TO TURN CELL PHONES OFF]. *Today you will participate in a game divided into three rounds. To guarantee the confidentiality of your decisions, it is very important that you do not speak among each other during the game, and that you follow the instructions exactly.*

If you have doubts, please ask any of us. Please do not open the folder until we ask you to. It is very important that you follow the instructions; if not, we will have to ask you to leave the game.

☼ **[SLIDE 2] CONFIDENTIAL**

RESEARCHER: *The result of this workshop is completely confidential, and the information will be used only for research. To guarantee this, the number that is on the cover sheet of the folder will be your identification.* [SHOW IN THE SLIDE]

No other participant will know your answers. This number will also serve as a receipt for your final payment. [SHOW IN THE SLIDE]

RESEARCHER: *Do you have any questions about what we have talked about so far?* [WAIT FOR AN ANSWER]

☼ **[SLIDE 3] GENERAL DESCRIPTION**

RESEARCHER: *In this game there will be THREE types of players. The first type is the DECISION MAKER. Each DECISION MAKER will receive 10 tokens with a value of 1,000 colones. We will request something simple: each DECISION MAKER should decide how many tokens he will keep and how many will he send to the second type of player, the RECEIVER. In our case, the RECEIVER is the Bosque Vivo initiative from the Environment and Energy Department of Costa Rica. Bosque Vivo receives voluntary donations to cover and*

protect forests and biodiversity in Costa Rica. At the end of the game, the donation will be made anonymously through the internet. The third type of player is the REGULATOR; we will explain their role soon.

☼ **[SLIDE 4] NUMBER OF ROUNDS**

RESEARCHER: *During this session, we will request that you distribute your tokens three times, called rounds. The instructions for each round will be given little by little, as we complete them. As a DECISION MAKER you will receive 10 tokens in each round and should distribute them between you and the RECEIVER. Remember that the receiver is the Bosque Vivo initiative.*

☼ **[SLIDE 5] PAYMENT ESTIMATION**

RESEARCHER: *The payment of the DECISION MAKER and the RECEIVER is estimated as follows:*

- *For the DECISION MAKER: The payment is equal to the amount of tokens kept multiplied by 1,000 colones.*
- *For the RECEIVER: The payment is equal to the amount of tokens received multiplied by 1,000 colones.*

As there will be three rounds, at the end of the experiment only one round will be randomly chosen to define the amount that you will earn in cash and the amount that will be donated to the RECEIVER. Important: this implies that you should pay attention during every round, as the selected round will be announced at the end. The round selected will be the same for all the participants.

RESEARCHER: *Are there any questions*? [WAIT FOR AN ANSWER]

☼ **[SLIDE 6] THE 1st ROUND**

RESEARCHER: *Let's start with the first round, where all of you will be DECISION MAKERs. Each one of you will receive 10 tokens with a value of 1000 colones each. You need to decide how many tokens you will send to the RECEIVER and how many you will keep. You will write down your decision on the decision sheet* [POINT AT THE SCREEN].

- *In the last row you should mark with an X the box that corresponds to how many of your 10 tokens you decide to send to the RECEIVER and how many you decide to keep for yourself.* [POINT TO THE SECOND ROW].
- *For example, if you decide to send 1 token, it means you will keep 9 tokens for yourself. But if you decide to send 9 of your tokens, it means you will keep 1 for yourself.*
- *If at the end of the game this is the selected round, the tokens you kept for yourself multiplied by 1000 colones is the amount you will gain. Note that your decision is independent from the actions of other DECISION MAKERs.*

When you have finished marking the chosen box, please fold the decision sheet, and one of my colleagues will collect it.

[PAUSE] *Are there any questions?* [WAIT FOR AN ANSWER].

☼ **[SLIDE 7] OPEN THE SHEET FOR ROUND 1**

RESEARCHER: *If there are no more questions, please open the folder and then open the sheet that says Round 1. Please make your decision now and mark the box.*

☼ **[SLIDE 8] THANK YOU FOR THINKING**

[ALLOW TIME TO COMPLETE THE TASK]

[Experimenters should collect the sheets for Round 1 and sum the total in Excel]

RESEARCHER: *Thank you, here we go again. During this second round, we have divided you into groups. At no time will you know who is in your group.*

☼ **[SLIDE 9] THE GROUP**

RESEARCHER: *For each group, one of you will be chosen randomly to be the REGULATOR, and the others will maintain their roles as DECISION MAKERs.*

In summary, in each group there will be 1 REGULATOR and the rest will be acting as DECISION MAKERs.

These roles will be maintained for the rest of the game. It is important that nobody knows whether you are a DECISION MAKER or a REGULATOR.

☼ **[SLIDE 10] OPEN THE SHEET FOR YOUR ROLE**

RESEARCHER: *Please open the sheet that says YOUR ROLE; there you will find out whether your role is DECISION MAKER or REGULATOR. Again, please do this in silence and close the sheet so nobody sees which your role is.*

[ALLOW TIME TO OPEN THE SHEET AND SEE THEIR ROLE]

RESEARCHER: *Most of you will be DECISION MAKERs and again should decide how much money to send to the RECEIVER, the Bosque Vivo initiative. The same as in ROUND 1, you will have 10 tokens. The number of tokens you keep and send depends exclusively on your decision, and will not affect or be affected by the decision of other DECISION MAKERs.*

☼ **[SLIDE 11] PAYMENT ESTIMATION OF THE REGULATOR**

RESEARCHER: *In what follows we will explain how the payment for the third player is calculated, that is, the REGULATOR.*

The amount that the REGULATOR will receive is set according to the decisions that all the DECISION MAKERs of the group where he belongs made. It is defined as follows: for each group, we will add the amount that the DECISION MAKERs decided to donate to the RECEIVER and we will divide it among the number of DECISION MAKERs of that group. The result will be the amount that will be paid to the REGULATOR; this money is from our funds. Note that the donation to the RECEIVER will not be affected. This means that the amount that the RECEIVER will obtain is exactly the tokens that the DECISION MAKERs will donate. But in making this decision, the DECISION MAKERs also determine how much to pay the REGULATOR.

So, the REGULATORS do not receive tokens, they are paid according to what the DECISION MAKERS donated to the receiver. At no time will they know in detail how much each member of their group sent; they will only know the average. [CLICK]

For example, in a group of 10 DECISION MAKERs, in the extreme case that all the DECISION MAKERs send the total amount of their tokens to the RECEIVER, the total is 100, divided among 10 DECISION MAKERs gives 10. We will give money from our funds to pay the REGULATOR 10 tokens, which equals 10 thousand colones. Again, recall that the REGULATOR payment comes from our funds, and not from what you send to the RECEIVER.

[PAUSE] *Are there any questions?* [WAIT FOR AN ANSWER].

RESEARCHER: *When opening the sheet that says Round 2, you will find the decision sheet.*

If your role is DECISION MAKER, you should decide how many tokens you will keep and how many you will send to the RECEIVER. During this round, the REGULATORS should respond to some questions.

☼ **[SLIDE 12] OPEN THE SHEET FOR ROUND 2**

Please open the sheet that says Round 2 and make your decision or answer the questions. When finished, close the sheet and hand it to the assistant.

☼ **[SLIDE 13] THANK YOU FOR THINKING**

Please take your time to make your decision.

[ALLOW TIME TO COMPLETE THE TASK]

[Experimenters should collect the sheets and sum the total for REGULATOR in Excel]

Please, it is very important that you do not speak among each other as we estimate the REGULATOR's payment.

[Experimenters should write down the amount to pay the REGULATOR. Make sure that everyone receives a paper so as not to reveal the identity of the REGULATORS. For the case of the DECISION MAKERs, simply put "Thanks for your participation in this round"].

[WAIT FOR SIGNAL OF APPROVAL TO CONTINUE]

Now, everybody receives a sheet. For the REGULATORS it will say the amount to be paid, and for the DECISION MAKERs a message of gratitude. Please open the sheet and when finished, close the sheet, to not reveal the message.

[WAIT FOR THE PARTICIPANTS TO CONTINUE]

RESEARCHER: *Now we will start the third and last round. Everybody will maintain their roles as REGULATORs or DECISION MAKERs and you will continue to belong to the same group. Once again, the DECISION MAKERs will receive 10 tokens each worth 1,000 colones.*

☼ **[SLIDE 14] AN INCENTIVE OR REWARD FOR SOME**

RESEARCHER: During this round an incentive or reward will be used to encourage some DECISION MAKERs to increase their number of tokens sent. Not all DECISION MAKERs will qualify to receive it. To qualify for this incentive or reward, the behavior of the DECISION MAKER during the previous round will be analyzed.

☼ **[SLIDE 15] QUALIFICATION RULES**

The rule to define who receives the incentive will be defined by the REGULATOR. In general, there are three types of rules:

1. *Leave it to luck who receives the incentive;*
2. *Encourage DECISION MAKERs who sent few tokens to the RECEIVER in Round 2. If you sent 2 or less tokens in Round 2, you will qualify to receive the incentive in Round 3;*
3. *Reward the DECISION MAKERs who sent many tokens to the RECEIVER during Round 2. If you sent more than 5 tokens in Round 2, you will qualify to receive the reward in Round 3.*

If the DECISION MAKER qualifies for the incentive, then he will receive an additional half token, for every token sent to the RECEIVER in Round 3. These tokens will be paid from our fund, without implying a decrease in the money for the RECEIVER or REGULATOR.

If the DECISION MAKER does not qualify for the incentive, then his/her situation will be the same as in the previous rounds. This is important: only the DECISION MAKERs who qualify will receive the incentive or reward.

Once the REGULATOR defines what the qualification rule is for the incentive or reward, we will tell you which DECISION MAKERs qualify and which do not.

*Are there any questions? Since this part is a little complicated, it is very important that you understand it well. [*WAIT FOR AN ANSWER].

☼ **[SLIDE 16] EXAMPLE OF A DECISION SHEET**

RESEARCHER: Here is an example of a decision sheet, in case you DO qualify. [SHOW THE EXAMPLE IN THE SLIDE]. *As DECISION MAKER, again you should decide how many tokens you will send and how many you will keep. For every token you send, we will give you an additional half token as an incentive or reward. Obviously, the more tokens you send, the larger the incentive or reward will be. You can see it here.* [POINT IN THE SLIDE: "Remaining tokens from the DECISION MAKER"].

Recall that only some of you will be selected for the incentive or reward, depending on the rule that the REGULATOR chooses. For the DECISION MAKERs who are not selected, the decision sheet will not have changed.

The payment to the REGULATOR will be done in the same way as before; that is, it depends on how many tokens are sent on average.

Are there any questions? [WAIT FOR AN ANSWER].

☼ **[SLIDE 17] OPEN THE SELECTION SHEET**

RESEARCHER: *Please open your selection sheets.*

If you are a DECISION MAKER, please answer the question. If you are a REGULATOR, choose the selection criteria. When finished, please fold the sheet and one of my colleagues will collect it.

☼ **[SLIDE 18] THANK YOU FOR THINKING**

Please take your time to make your decision.

[ALLOW TIME TO COMPLETE THE TASK]

[Experimenters should collect all the sheets and use the Excel file to define which players qualify and which do not qualify for the extra payment. If the decision rule is a "lottery", a coin should be tossed. If it's heads = even numbers. If it's tails = odd numbers. Place the number of each participant on the sheets of Round 3, according to the decision rule selected. Experimenters distribute the sheets of Round 3]

Please, it is very important that you do not speak among each other as we determine who qualifies and who doesn't.

[CONTINUE AS SOON AS THE SHEETS ARE DELIVERED]

RESEARCHER: *At this time we will deliver the decision sheets for round 3. Please do NOT open until we tell you.*

[WAIT FOR SIGNAL FROM RESEARCHERs]

☼ **[SLIDE 19] OPEN SHEET FOR ROUND 3**

RESEARCHER: *You just received a sheet that says Round 3. The sheet for DECISION MAKER states whether you were selected to receive the incentive or not and the reason why. Please read carefully. If you are a DECISION MAKER, you should decide how many tokens you are going to keep and how many you are going to send to the RECEIVER. The payment of the REGULATOR will be the same as before. REGULATORS please answer the questions.*

Please make your decision now and mark the box. When finished, please fold the sheet.

☼ **[SLIDE 20] THANK YOU FOR THINKING**

Please take your time to make your decision.

[ALLOW TIME TO COMPLETE THE TASK]

[Experimenters should collect the sheets and calculate the REGULATOR's payment in Excel]

Please, it is very important that you do not speak among each other as we calculate the REGULATOR's payment.

[Experimenters should write down the amount to pay the REGULATOR. Make sure that everyone receives a paper so as not to reveal the identity of the REGULATORS. For the case of the DECISION MAKERs, simply put "Thanks for your participation in this round"].

[WAIT FOR SIGNAL OF APPROVAL TO CONTINUE]

Now, everybody receives a sheet. For the REGULATORS it will say the amount to be paid, and for the DECISION MAKERs a message of gratitude. Please open the sheet and when finished close the sheet so as not to reveal the message.

☼ **[SLIDE 21] THANK YOU**

RESEARCHER: *Thank you for completing this last round. At last we have finished the 3 rounds included in this game. Now we will choose which one is selected for the cash payment to you. To do this, one of you should come and pull out a ball from this bag* [SHOW THE BAG AND THE THREE NUMBERED BALLS]. *Note that the incentive or reward mentioned during Round 3 will be paid only if the ball for Round 3 is chosen. Please pull out a ball from the bag.* [ALLOW THE PARTICIPANT TO PULL OUT A BALL FROM THE BAG].

RESEARCHER: *As you can see, the round selected to calculate the cash payments for you is round number ____.*

Now we will calculate the payment for the RECEIVER. [WAIT]

During this round, in total you sent _____ colones to the Bosque Vivo initiative. In dollars this is _____.

[EXCHANGE RATE 500 colons = 1 US$]

Now we will make this payment to the RECEIVER, Bosque Vivo initiative. Note that this is a real donation that we are going to send right now.

[ENTER WEB PAGE AND MAKE PAYMENT TO BOSQUE VIVO IN DOLLARS. 1 USD = 500 COLONES. SHOW THE CERTIFICATE AND RECEIPT TO THE PARTICIPANTS]

☼ **[SLIDE 22] SURVEY**

RESEARCHER: *Before finishing, we appreciate if you please complete the enclosed survey. Meanwhile we are going to prepare your payments and call you up individually. At this point it is very important that you have the folder where your identification number for the game is, so we can give you your correct payment. After completing the survey, we invite you to enjoy a snack. Do not forget the folder!*

[COLLECT SURVEYS]

☼ **[GO TO SLIDE 23] THE END! THANK YOU!**

Again, thank you very much for participating in this event.

[AT THE END, COLLECT ALL THE MATERIAL AND PUT IT IN A YELLOW ENVELOPE, CLOSE IT AND WRITE HOUR, DATE AND PLACE]

Paper III

Unintended Consequences of Targeting Forest Conservation Incentives:

Behavioral Insights into Incentive Design

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Abstract

Ongoing concerns about species and water quality, plus growing attention to carbon, have generated significant interest in the use of incentives to promote forest conservation, e.g., payment for ecosystem services (PES) or Reduced Emissions from Deforestation and forest Degradation (REDD) payments. A key challenge in the design of such compensation mechanisms is the choice of whom to pay. Experts and practitioners debate whether the selection of whom to pay should be based on: i) additionality, by paying those who would not conserve without the incentive; ii) rewards, by paying those who are already conserving forest; or iii) location-based environmental benefit, in which case payments would go to those in prioritized areas. In this paper, we use a field experiment to test the hypothesis that these different selection rules imply different effects on contributions to forest conservation by those selected for the incentive and unintended negative effects on contributions by those excluded from the incentive. Our results suggest that it is only a focus on additionality that leads to decreased average contributions to forest conservation by unpaid individuals, thereby limiting the total gains expected from such focus. These results should be considered in the design of conservation incentive programs in general and PES schemes in particular.

Keywords: incentives, payment for ecosystem services (PES) programs, targeting, spillovers, field experiment.
JEL classification: C93, D03, H41.

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1. Introduction

Forests provide key services to humans such as carbon sequestration, biodiversity, and watershed conservation. Ongoing concerns over deforestation and forest degradation trends have led communities, governments, and international organizations to increase their efforts to protect forests (e.g. Barton et al. 2009, Klemick 2011). Among such efforts, compensation mechanisms like payments for ecosystem services (PES) programs, which aim to trigger the provision of ecosystem services by altering the relative prices of alternative activities, have rapidly increased in popularity in the last decade—both in theory and in practice (Pattanayak et al. 2010, Ferraro 2011). A key challenge in the design of compensation mechanisms is the choice of who to pay and why.

At global level, decisions about who should get paid for reducing emissions from land-use take center stage in climate change discussions. Locations with lower deforestation risk have argued for paying for ecosystem services already generated through the protection of their forests. Impact analysts, on the other hand, argue that locations with high deforestation and high opportunity costs of protecting forests should be paid to discourage land-use changes in risk-prone areas (Pfaff and Sanchez-Azofeifa 2004, Muñoz-Piña et al. 2008, Wünscher et al. 2008). These types of arguments are highly present in the negotiations of the currently proposed Reduced Emission from Deforestation and forest Degradation (REDD) schemes.

In Latin America, ongoing national programs to pay for ecosystem services exist in Costa Rica and Mexico. Both have been criticized for their targeting efforts (or lack thereof)[1] and part of the discussion is caused by the fact that not all policymakers are in agreement regarding the objectives behind the targeting efforts. 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 paid land (Wu and Babcock 1996, Smith and Shogren 2002), such as land with highly valued species or land near rivers upstream of cities. An alternative focus is to enroll land with low opportunity costs of providing ecosystem services (Stoneham et al. 2003, Goeschl and Lin 2004, Ferraro 2008). Such land can be effectively targeted with a common, bottom-up approach: enrolling those who apply first. Those whose lands are not profitable in agriculture, i.e., those with low opportunity costs of forest conservation, may be willing to accept even a small amount as a payment to keep

[1] Alix-Garcia et al. (2012), Arriagada et al. (2012) and Robalino and Pfaff (2012) show that most parcels selected for payment in Costa Rica and Mexico were the ones that were least likely to be deforested anyway.

their lands forested and hence be the first to apply. Note that this approach could be perceived as a reward for "forest conservation" since if agriculture is unprofitable, then private land without a policy may retain its standing forest. However, such a situation implies a low impact of payments, since land would be forested with or without payment (Alix-Garcia et al. 2012, Arriagada et al. 2012, Robalino and Pfaff 2012). One more recent recommendation to increase impact is to target deforestation and forest degradation threats, i.e., making an effort to enroll land that faces a threat of being deforested or degraded in absence of payments (Pfaff and Sanchez-Azofeifa 2004, Muñoz-Piña et al. 2008, Wünscher et al. 2008). The focus would then be on those who have shown low conservation efforts. By design, this impact focus ("efficiency," "additionality") excludes from payment those who are most likely to protect their forest without payment. Such a focus makes sense if the force driving standing private forest 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 as excluded individuals might feel like they are being punished instead of rewarded for their high likelihood of acting pro-socially in the absence of the monetary incentive program.

From the local to the global, in payment schemes designed for ecosystems services or REDD, targeting has important consequences for efficiency and fairness. Paying for ongoing activities is seen as inefficient since no new emissions reductions are obtained. At the same time, paying for only new changes in land use can be seen as unfair as it excludes those who already decided not to deforest. In the REDD discussion, for example, a country like Costa Rica, which has successfully achieved a halt in deforestation stands to be left out of the incentives, precisely because of this success. Yet can countries and landowners be expected to continue protecting forests if excluded from payments?

In this paper we investigate responses by paid and unpaid individuals to a monetary incentive aimed at encouraging contributions to forest conservation, and test whether the effects on contributions are different under alternative, commonly used selection rules.

The existing literature suggests the possibility of unintended consequences from PES, affecting land use decisions of individuals who are not part of the payment program, irrespective of whether or not they apply to the program (e.g., Murray et al. 2004). Within this literature, the focus has been on spillovers that operate through prices or income (termed 'price effects', 'slippage', and 'leakage' – see, e.g., Wu et al. 2001, Robertson and Wunder 2005, Alix-Garcia et al. 2012). Price spillovers arise naturally in most economic modeling of

land use. For instance, reduced crop supply due to land being set aside for conservation can raise crop prices, leading to a reallocation of forest degradation or deforestation to areas that would otherwise not have been deforested. This unintended effect operates through changes in market prices. Our study, on the other hand, explores whether there are unintended effects arising from targeting conservation incentives at a given group of potential beneficiaries, implying that some prospective beneficiaries will be excluded. For those excluded by design of the payment program, there is no change in income, yet their exclusion might still trigger a change in behavior. We refer to such an effect as *behavioral spillover*. Note that behavioral spillovers are different from the so-called crowding-out effect. Crowding-out effect is a negative reaction restricted to individuals who do receive the incentive (see theoretical papers by Frey 1994, Deci et al. 1999, Frey and Jegen 2001). Further, we hypothesize that alternative targeting approaches, i.e., selection rules, lead to different levels of *behavioral spillover*. If such spillovers tend to appear, and if they tend to shift based on selection rules, policymakers designing PES programs may want to take these unintended effects into account when evaluating the full impact of their targeting efforts.

The behavioral and experimental economics literature provides several potential explanations for the occurrence of behavioral spillovers. First, a well-established empirical result is that people tend to treat those who treated them fairly more nicely but punish those who did not (Rabin 1993). These very same preferences for fairness are also suggested in experiments by 50:50 divisions of resources and the willingness to give up low offers of resource in order to punish those who suggested them (Fehr and Schmidt 2006, Dawes et al. 2007). In the context of PES, if a particular selection rule is regarded as unfair, agents affected by it might choose to react by reducing their efforts to protect nature. This would be a negative (understood as undesirable) behavioral spillover in the notation of this paper. Second, people's behavior is also guided by how others perceive them (for economic models incorporating social approval see e.g., Akerlof 1980, Hollander 1990, Bénabou and Tirole 2006, Ellingsen and Johannesson 2008, Andreoni and Bernheim 2009). If conservation is driven partly by the desire to be liked and respected by others as an environmentally friendly person, the introduction of a payment in exchange for environmentally friendly behavior could spoil the clarity of the pro-environmental signal (cf. results from Ariely et al. 2009). For agents who are not selected to the program and hence do not receive a payment, the lost signaling value of pro-environmental actions is not compensated by monetary incentives, and could then result in less of such behavior.

Since both price and behavioral spillovers are expected to occur simultaneously, and selection rules are in practice hard to change, we use a field experiment to investigate responses to a targeted incentive by both included and excluded subjects. Our field experiment was conducted with Costa Rican landowners, as part of a face-to-face survey about land use, agricultural practices, and socioeconomic characteristics. Participants were paid for their time and effort. This endowment was then used in the field experiment: Participants were asked whether they would like to contribute to forest conservation by making a monetary contribution to a governmental program called Bosque Vivo. The objective of Bosque Vivo is to conserve key forest ecosystems in Costa Rica — a public good—using funds received from private donors. Hence, the subjects provided conservation efforts indirectly, versus directly through land use decisions as they would in an actual PES program.

In our experiment, behavioral spillovers are explored by conducting a within-individual comparison of decisions in two subsequent rounds: one without incentives and one where incentives are handed out to some and not to others according to previously determined selection rules. In a two-round structure, behavior might change for other reasons not related to the selection rules (our treatments). For example, in the second round subjects might feel they have already contributed once (diminishing altruism) or might feel richer (as they are paid twice). Thus, in order to be able to isolate the effects of our treatment, we use a difference-in-difference (DiD) approach, i.e., we compare changes in decisions for Round 1 and Round 2 under each selection rule with those in a control treatment where both rounds occur without incentives. When we do introduce incentives, we randomly assign one of the following selection rules[2]: [1] *additionality rule*—select those with low (i.e., below a threshold) contributions in the first round; [2] *reward rule*—select those with high (i.e., above a threshold) contributions in the first round; and [3] *environmental benefits rule*—select those whose contributions go only to locations with high environmental benefit.

We find negative behavioral spillovers when incentives are assigned based on rules that select those who initially made low contributions to forest conservation. In this case, participants with high initial contributions significantly *reduce* their average support as a reaction to being excluded. Notably, *only the additionality rule* triggers such behavior. Being

[2] The labels given here for these rules are intended to be descriptive. We did not use them with the participants so as not to generate any signals about expected behavior. The specific script used is available in the Appendix.

excluded because land does not generate high environmental benefits does not prompt this negative behavioral spillover, nor does being excluded for having made relatively low initial contributions to conservation.

The behavior of participants actually selected to receive the incentive is as expected. Under the additionality rule, those with low initial contributions more than doubled their overall support when selected to receive the incentive. In contrast, neither of the other selection rules generated such significant gains. These results set up an efficiency-fairness tradeoff for the design of conservation incentive programs, which is further strengthened by similar results from a laboratory experiment with over 400 students at the University of Costa Rica in Costa Rica (Alpízar et al. 2013).

Our results shed light on an aspect of PES design that is frequently a source of disagreement between experts and practitioners: Targeting incentives to those who would otherwise not contribute to conservation efforts makes sense from an efficiency perspective, experts argue, but excluding those who would protect nature regardless of incentives is hard to justify, practitioners claim. Our results prove both groups right. The following excerpts from a recent discussion in *RESECON*[3] illustrate this discussion. In talking about the need to increase additionality through PES design, one expert argues:

> *"Is it just a PES design issue? And can this be left to the PES designers? Isn't there a property rights issue also involved? 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*

[3] RESECON, Land and Resource Economics Network, is a server by which environmental and resource economists can post and receive information relevant to their work. See further: http://www.resecon.org

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."[4]

Although far from providing an exact estimate of behavioral spillovers in the real world, we show that such spillovers do exists when the additionality rule is used, and can thereby reduce the size of the expected additional benefits. By making efficiency and fairness considerations part of the same equation, those designing selection rules for conservation payment programs would be able to achieve increased additionality.

The rest of the paper is organized as follows. Section 2 describes the experimental design. Section 3 presents the experimental results, and finally Section 4 summarizes the results and concludes the paper.

2. Field experimental design and procedures

The basic structure of our experiments follows the well-known dictator game where a player, i.e., the dictator, is given a sum of money to allocate between himself and another player, i.e., the receiver (e.g. Kahneman et al. 1986, Forsythe et al. 1994, Hoffman et al. 1996). In our game the recipient is a charity organization, hence our experiment follows the experience of Eckel and Grossman (2003) and Carpenter et al. (2008). In our field experiment, subjects consented to be part of a survey.[5] Even though a survey is not part of people's daily life, face-to-face surveys are commonly used in Costa Rica, including a national census in 2011.

The survey itself was an hour-long questionnaire about land use and socioeconomic characteristics of landowners in Costa Rica. About half an hour into the survey, enumerators announced a short break. During the break, the subjects were informed about and received a

[4] 2013/01/24 [RESECON] New Forest Trends' Ecosystem Marketplace Report "State of Watershed Payments 2012" and Launch Event Information. Names are omitted.

[5] See (List 2008) for a discussion on informed consent in the social sciences. Our subjects did not formally consent to be part of the experiment, as such a procedure may have affected the behavior of our subjects, which we wanted to maintain as realistic as possible. Still, strict ethical norms were followed: our research did not put participants at higher risk than in normal life at any time. There were no direct benefits to the participants but this research will hopefully benefit society by increasing knowledge about how people react to incentives. Subjects were treated fairly and kept anonymous at all times.

payment in appreciation of the time and effort[6] dedicated so far to the survey. Then the first round of our experimental design was initiated. Half an hour thereafter, the survey came to an end and a second payment was announced. Then the second and final round of our design was initiated. Note that the 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 was paid in notes and coins to allow for combinations of the 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 cost on average of 10,000 colones (20 USD). We used these relatively high stakes to make the study as realistic as possible. However, since we use a difference-in-difference approach, stake effect 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.

During each round, subjects were invited to make a voluntary and anonymous contribution to a governmental program called Bosque Vivo. The objective of Bosque Vivo is to conserve key forest ecosystems in Costa Rica by using contributions received from private donors, and as such can be regarded as a public good. The contributions were used in region-specific efforts, hence each landowner was given the possibility to contribute money for forest conservation in the region where his/her land was located. The purpose and nature of Bosque Vivo 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.[7] Subjects were told that they could only use the payment they just received, so the maximum amount that could be contributed each time was 5,000 colones, in multiples of 500 colones.

[6] 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.

[7] Since the enumerator had the ability to 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 by strictly focusing on differential treatment effects and not absolute contributions.

In round one, landowners were invited to contribute and no additional information was provided. Round one provides baseline pro-environmental behavior in the absence of any incentive. Round two introduced the treatments, consisting of an incentive to contribute dependent on a randomly assigned selection rule. In order to stress the fact that the incentive was in the form of a payment, we decided to have subjects contribute whatever amount they chose, and then the research team paid the subjects chosen by the selection rule half of the contributed amount.[8] For example, if a subject decided to contribute 1,000 colones to Bosque Vivo, 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. A limited budget was used as justification for using selection rules. For the exact wording, see Appendix. To control for the effect of making repeated contributions, a subsample received round two without any incentive program and hence no mention of selection rules. We refer to this treatment as the control treatment.

In the cases where a selection rule was introduced, three alternative selection rules were tested and randomly distributed in the sample. For the first two rules, selection was determined by the subject's past behavior. In one case, which we here—not in the script—call *additionality rule*, 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 rule is to encourage additional contributions from those who are prone to give little according to the baseline. The additionality rule is the standard recommendation given to PES programs that want to increase their performance by achieving impact. In the standard dictator game, without any incentives, average giving is around 20% of the endowment (see Camerer 2003 for a review). To reach additional contributions on top of this expected outcome without any incentive, the threshold for the additionality rule was set at contributions of 1000 colones or less, i.e., 20% of their endowment.

The second rule, which we here call the *reward rule*, selected subjects who had contributed 2,500 colones or more for the incentive. This rule rewards those who contributed 50% or more of their endowment. We also wanted to test whether selection rules based on

[8] In our experiment, we moved away from "seed money"/matched funds experiments (e.g. List and David Lucking-Reiley 2002) to come closer to the incentive structure of payment for ecosystem services (PES) schemes by framing the incentive as a direct payment to the respondent (landowner). This is important since the framing of incentives for prosocial behavior does matter (Eckel and Grossman 2003).

past behavior lead to more behavioral spillovers than selection rules based on exogenous characteristics, i.e., rules that are outside the landowner's control, e.g., rules based on the provision of ecosystem services. Therefore, in the third rule—called the *environmental benefits rule*—subjects were selected if their contributions affected high environmental benefit locations. Selection based on location was made possible since Bosque Vivo uses contributions for region-specific interventions to conserve and restore forest. Under *the environmental benefit* rule, a subject was selected if his/her contribution led to increased investment by Bosque Vivo in the prioritized area of Nicoya. The prioritization of Nicoya was motivated in the script for their water quality problems during dry seasons.

Subjects only learned about the selection rule applicable for his/her case, and whether he/she qualified for the incentive. For administrative reasons, a receipt was signed before ending the questionnaire.

The sample includes 357 Costa Rican landowners interviewed from November 2011 to January 2012. Two key databases were used as sampling frame: i) all 2011[9] applications received by the National Forestry Financing Fund (FONAFIFO), which is the authority responsible for PES in Costa Rica, and ii) a farm census conducted by the Ministry of Livestock and Agriculture (MAG) in 2006 and 2007. We focus on four out of nine regions of Costa Rica, namely Limon, Guapiles, San José, and Nicoya. To keep a balance between the two datasets, our random sample includes 25% of the observations from each region of the FONAFIFO dataset and approximately 40 random observations per region from the much larger MAG dataset. Appointments with respondents were made by phone before visiting their home. Most of those who were contacted were interested in participating in the survey. However, for logistical reasons it was impossible to meet the preferences of all landowners regarding date and time. Hence, in the end around 50% of all those who were contacted were actually interviewed. The selection rules were randomly distributed between the subjects in each region and between the enumerators.

[9] All applications received until April 2011.

3. Results

The statistical analysis is based on two key insights. First, to investigate the presence of behavioral spillovers, i.e., a change in contributions motivated by exclusion from the incentive, we use a difference-in-differences (DiD)[10] approach. This approach accounts for the two-round structure of our experiment, where contributions could rise or fall across rounds for reasons other than the introduction of an incentive, e.g., in the second round subjects may feel that they have already contributed (diminishing altruism) or may feel richer (as at that point they have been paid twice). Further, in order to identify and use selection rules based on individuals' past behavior, all subjects were asked to mark the amount contributed on an envelope with only the survey number as the identifier. This disclosure of contribution might trigger an experimental demand effect, referred to as "the changes in behavior by experimental subjects due to cues about what constitutes appropriate behavior" (Zizzo 2010:75). In our experiment, contributions might reflect an experimental demand effect arising from the enumerators' ability to observe the subjects' contribution (e.g., Hoffman et al. 1996, Zizzo 2010). The DiD approach should take care of such concerns by strictly focusing on differential treatment effects and not absolute contributions. We look at the within-subject change in contribution from Round 1 to Round 2 for the subsample where a given treatment was applied, and compare it with the change in behavior observed in the subsample that faced the control treatment, i.e., where no incentives were handed out. This is done for excluded and selected subjects separately.

The second insight is more subtle. One could argue that those who contribute a little (i.e., 20% or less of their endowment, ≤1000 colones) and those who contribute a lot (i.e., 50% or more of their endowment, ≥2500 colones) in the first round are actually different types of individuals that might react differently to incentives and exclusion. The cleanest controls are then given by subjects in the control treatment group who would have been excluded should the rule have been applied to them. The same is true for the selected subjects. All our DiD tests use such a matched subsample as control.

[10] Since the work by Ashenfelter and Card (1985), the use of difference-in-difference methods has become very widespread. In the simplest setup, outcomes are observed for two groups for two time periods. The first group is the group exposed to a treatment in the second period but not in the first. The other group, the control group, is not exposed to the treatment during either period. When the same units within a group are observed in each time period, the average gain in the control group is subtracted from the average gain in the treatment group.

Table 1. Average contributions for each selection rule and each control for **excluded** subjects.

	Reward rule	**Environmental benefits**	**Environmental benefits Only 1st contribution>1000**	**Additionality rule**
TREATMENT	Excluded 1st contribution<2500	Excluded Land outside Nicoya	Excluded Land outside Nicoya and 1st contribution>1000	Excluded 1st contribution>1000
1st contribution	1000 colones (37 obs)	3339 colones (62 obs)	4234 colones (47 obs)	4070 colones (71 obs)
2nd contribution	1081colones (37 obs)	2468 colones (62 obs)	2862 colones (47 obs)	2824 colones (71 obs)
Difference in contributions	+81 colones	-871 colones***	-1372 colones***	-1246 colones***
CONTROL	Control 1st contribution<2500	Control Land outside Nicoya	Control Land outside Nicoya and 1st contribution>1000	Control 1st contribution>1000
1st contribution	932 colones (37 obs)	3228 colones (57 obs)	4366 colones (41 obs)	4040 colones (62 obs)
2nd contribution	1108 colones (37 obs)	2474 colones (57 obs)	3220 colones (41 obs)	3250 colones (62 obs)
Repeated contribution effect	+176 colones	-754 colones***	-1146 colones***	-790 colones***
Treatment effect Behavioral spillover (Difference-in-Difference)	**-95 colones**	**-117 colones**	**-226 colones**	**-456 colones***

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

3.1. Reaction of those excluded from the incentive

For the excluded subjects, the average contributions in Round 1 and Round 2 for each selection rule and for its control group are presented in Table 1. Here we also present the results for those excluded under the environmental benefit rules with initial contributions larger than 1,000 colones (i.e., those who would have been excluded under the additionality rule). This is to test whether the reasoning behind the exclusion, i.e., land located outside a high environmental benefit location, might give a different effect on contributions compared with the additionality rule. We start by checking whether the tailor-made control groups for each selection rule have any significant differences to start with as compared with the treated group in their first contribution, i.e., prior to being exposed to the selection rule. In all cases we do not find significant differences (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).[11]

Using DiD, controlling for the fact that the subjects were making two contributions, we find significant negative behavioral spillovers when excluded from incentives under the additionality rule. Thus, when excluding those with relatively high initial contributions, it has the unintended consequence of lowering average contributions by 456 colones (one-tailed t-test p-value=0.07[12]), implying a decrease of 11%. Do note that the only thing different from the control for these individuals is that they were excluded; all else remains the same—hence our claim of behavioral spillovers. For the reward rule, being excluded for having a relatively low initial contribution does not seem to have any negative spillovers (one-tailed t-test p-value=0.30). Nor does being excluded because one's land is not in a region of particularly high environmental benefits appear to trigger this negative reaction (one-tailed t-test p-value=0.38). Notably, even when considering only those with high prior contributions who were excluded due to location, there is no significant reduction in average contribution (one-tailed t-test p-value=0.30), indicating that it is indeed a reaction to the additionality rule that triggers the significant observed reduction in contributions.

[11] 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.

[12] One-tailed t-tests are used to test our hypothesis of negative behavioral spillovers upon exclusion from an incentive.

In Table 1, we note that there is a significant drop in contributions for those with relatively high initial contributions, i.e., those excluded from the incentive with the additionality rule and the environmental benefits rule (p-values<0.01; Wilcoxon test). Hence, the initial contribution rather than the treatment, i.e., exclusion from the incentive, might be driving the negative effect on average contribution. To control for this, we run separate regressions on the change of contributions over rounds for each selection rule. We include initial contributions in both a linear and a non-linear (by using dummies) form. Table 2 presents the results. As expected, we find a significant negative effect of initial contributions for both the additionality rule and the environmental benefit rule, suggesting that high initial contributions significantly decrease contributions in the second round. But even after controlling for such an effect, we still find significant negative behavioral spillovers for the additionality rule. For the reward rule and the environmental benefit rule, the insignificant effect of exclusion does not change when controlling for initial contributions.

Table 2. Regression results for **excluded** subjects. The dependent variable is change in contributions over rounds (p-value).

Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Intercept	228 (0.185)	277 (0.128)	680* (0.061)	226 (0.497)	655 (0.427)	-323 (0.590)	1025** (0.048)	240 (0.518)
Additionality rule							-443* (0.067#)	-446* (0.068#)
Reward rule	-91 (0.312#)	-65 (0.365#)						
Env. benefits rule			-67 (0.420#)	-90 (0.397#)				
Env. benefits rule with contributions>1000					-280 (0.253#)	-221 (0.310#)		
Initial contribution linear	-0.06 (0.634)		-0.44*** (0.000)		-0.41** (0.020)		-0.45*** (0.000)	
Dummies for initial contributions								
500-1000 colones		-239 (0.293)						
1500-2000 colones		-94 (0.687)						
2500-3000 colones				-681 (0.339)		-67 (0.939)		-690 (0.182)
3500-4000 colones				-226 (0.868)		323 (0.832)		-1091 (0.161)
4500-5000 colones				-1756*** (0.000)		-1138* (0.055)		-1441*** (0.000)
Number of observations	74	74	119	119	88	88	133	133
R-square	0.0069	0.0197	0.1998	0.1772	0.0647	0.0245	0.1190	0.1198

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

A one-tailed t-test is used to test the hypothesis that there is a negative effect, i.e. behavioral spillover, upon exclusion from the incentive.

Leaving the magnitude of the effect of exclusion aside and looking at the share of subjects reacting negative upon exclusion from the incentive, we find that the reward rule has a significantly smaller share of negative responses than both the additionality rule and the environmental benefits rule (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).

3.2. Selected to receive the incentive

For the selected subjects the average contributions in Round 1 and Round 2 for each selection rule and for the respective control group is presented in Table 3. As for those excluded, the tailor-made control groups for each selection rule are not statistically different to start with when compared to their treated counterparts (two-tailed t-test *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).

Using DiD, controlling for the fact that the subjects were making two contributions, we find a positive effect on contributions of those who received the incentive because of the additionality rule. Thus, when incentives go to those with relatively low initial contributions, the average contributions increase by 735 colones (one-tailed t-test p-value=0.04), or 122%. We see a similar increase among those who started with low initial contributions but were selected under the environmental benefit rule: their contributions increase by 750 colones (one-tailed t-test p-value=0.05), or 120%. Neither the reward rule—being selected for having relatively high initial contributions—nor being selected for having land with high environmental benefits (full sample) seem to yield a significant increase in contributions (one-tailed t-test p-value=0.28 and 0.30 respectively).

Table 3. Average contributions for each selection rule and each control for **selected** subjects.

	Reward rule	**Environmental benefits (full sample)**	**Environmental benefits Only if 1st contribution≤1000**	**Additionality rule**
TREATMENT	Selected 1st contribution≥2500	Selected Land in Nicoya	Selected Land in Nicoya and 1st contribution≤1000	Selected 1st contribution≤1000
1st contribution	4380 colones (54 obs)	2778 colones (27 obs)	625 colones (8 obs)	605 colones (19 obs)
2nd contribution	3546 colones (54 obs)	2852 colones (27 obs)	1375 colones (8 obs)	1500 colones (19 obs)
Difference in contributions	-833 colones***	+74 colones	+750 colones*	+895 colones*
CONTROL	Control 1st contribution≥2500	Control Land in Nicoya	Control Land in Nicoya and 1st contribution≤1000	Control 1st contribution≤1000
1st contribution	4540 colones (50 obs)	2583colones (30 obs)	667 colones (9 obs)	440 colones (25 obs)
2nd contribution	3510 colones (50 obs)	2517colones (30 obs)	667 colones (9 obs)	600 colones (25 obs)
Repeated contribution effect	-1030 colones***	-66 colones	0 colones	+160 colones
Additionality (Difference-in-Difference)	**+197 colones**	**+140 colones**	**+750 colones****	**+735 colones****

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

As for the excluded subjects, we expect high initial contributions to significantly decrease contributions in the second round. We again run separate regressions on the change in contributions over rounds using a linear and a non-linear formulation of initial contribution. Table 4 presents the results. We find a significant negative effect of initial contributions for the additionality rule, the environmental benefit, and for those with low prior contributions selected due to location, while for the reward rule we do not find any significant effect. The positive effect of incentivizing those with low initial contributions, i.e., the additionality rule, continues to be significant. Similarly, for those with low prior contributions selected under the environmental benefit rule we find a significant positive effect of the incentive. However, as there were rather few observations for this subgroup, this should be explored further in future research. For the reward rule and the environmental benefit rule, the insignificant effect of receiving the incentive does not change when controlling for initial contributions.

Table 4. Regression results for **selected** subjects. The dependent variable is change in contributions over rounds (p-value).

Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Intercept	-319 (0.708)	-861** (0.036)	326 (0.215)	245 (0.259)	543 (0.197)	667* (0.092)	625** (0.042)	638** (0.036)
Additionality rule							910** (0.011#)	943*** (0.009#)
Reward rule	172 (0.302#)	200 (0.275#)						
Env. Benefits rule			170 (0.275#)	106 (0.344#)				
Env. Benefits rule with contributions>1000					716 (0.046#)	833 (0.020#)		
Initial contribution linear	-0.16 (0.385)		-0.15** (0.043)		-0.82* (0.087)		-1.06** (0.008)	
Dummies for initial contributions								
500-1000 colones						-1000** (0.027)		-1087*** (0.006)
1500-2000 colones								
2500-3000 colones				-737* (0.051)				
3500-4000 colones		200 (0.705)		-1245* (0.088)				
4500-5000 colones		-261 (0.519)		-455 (0.133)				
Number of observations	104	104	57	57	17	17	44	44
R-square	0.0110	0.0125	0.0780	0.1223	0.3330	0.4237	0.2203	0.2305

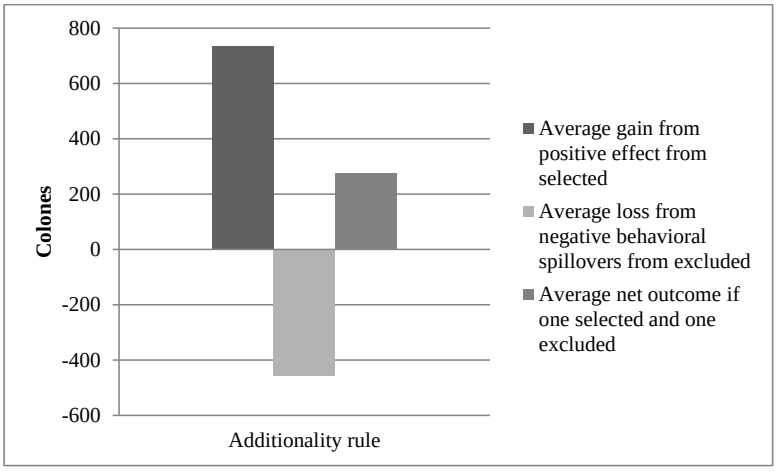
***=significant at 1%, **=significant at 5% , *=significant at 10%.

A one-tailed t-test is used to test the hypothesis that there is a positive effect, i.e., additionality, upon selection for the incentive.

3.3. Overall impact of targeted incentives

Our results suggest that behavioral spillovers—negative effects on contribution upon exclusion from an incentive—and additionality—positive effects on contribution upon selection for an incentive—occur for *only one* of the three selection rules: *the additionality rule*. Thus, when investigating the overall impact of targeted incentives, i.e., the magnitude of the average gain, per person selected, and the average loss per person excluded, we only do so under the additionality rule. The results, illustrated in Figure 1, affirm that the potential gains from incentives targeting threats (i.e., low contributions for forest conservation) are lost when accounting for the behavioral reaction of excluded subjects. The net outcome of an increase of 279 colones in forest conservation contributions is insignificant (two-tailed t-test p-value=0.546). The behavioral spillovers decrease the potential gains from incentivizing contributions for conservation by 62%, leading to a zero average effect in overall outcome. Thus, in evaluating a selection rule, responses by those who are excluded can matter substantially.

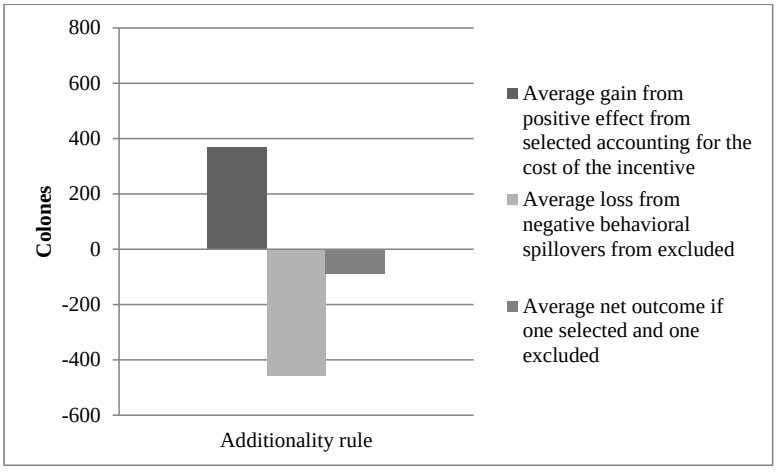
Figure 1. The effect upon selection or exclusion under the additionality rule and the net outcome of one selected and one excluded individual.



Note: The net outcome under the additionality rule is not significant, according to a two-tailed t-test p-value=0.546.

From a policy point of view, the cost of paying incentives also has to be taken into account when measuring the net effect of targeted incentives. Figure 2 illustrates the case where the cost of the incentive is subtracted from the average gain per person selected. The net outcome for the additionality rule then becomes an insignificant loss of -89 colones (two-tailed t-test p-value=0.832). This result shows that incentives targeting threats (i.e., low contributions for forest conservation) could result in an overall zero outcome when the cost of the incentive is incorporated. For the reward and the environmental benefit rule, an overall zero net outcome is still found when accounting for the cost of the incentive (two-tailed t-test p-value=0.983 and p-value=0.912, respectively).

Figure 2. The effect upon selection or exclusion under the additionality rule and the net outcome of one selected and one excluded individual accounting for the cost of the incentive.



Note: The net outcome under the additionality rule is not significant, according to a two-tailed t-test p-value=0.832.

4. Conclusion

The results of this study suggest that behavioral spillovers—which occur if those excluded from incentives in response to the selection rule choose to no longer contribute to forest conservation—are important to consider when designing targeting strategies for conditional incentive programs (such as PES) aiming to increase contributions to a public good. It is striking that the choice of who to pay (those contributing, those not contributing, or depending on location) alone can have a significant impact on contributions. This effect turns out to be especially important when targeting low contributions (e.g., deforestation threats), as such selection implies a negative average impact upon exclusion from the incentive and a positive average impact upon selection for the incentive. This implies an efficiency-fairness tradeoff for PES programs: Targeting those who require incentives to contribute may increase contributions for forest conservation but at the same time decrease contributions through behavioral spillovers, whereas targeting those with past high contributions for forest conservation may leave the contributions of both paid and unpaid agents unaffected.

The three selection rules considered in this study also relate to the debate between experts and practitioners about whether selection rules for forest conservation incentives should be based on additionality (e.g., focus on efficiency by paying those who would not conserve without the incentive), rewards (e.g., focus on fairness by paying those who are already conserving forest), or environmental benefit based on location (e.g., pay those in prioritized areas due to high environmental benefits). Indeed, forest conservation is, to some extent, a matter of voluntary contributions to public goods, and introducing an incentive targeting those who show little pro-social/pro-environmental inclinations might spoil the motivation of those who do like to contribute.

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APPENDIX

The script translated to English[13]

Good morning / afternoon / evening.

My name is ... I come from CATIE, which is the Tropical Agricultural Research and Higher Education Center located in Turrialba. It is an education center that also conducts research. This survey is part of a study regarding the productive activities of farms in Costa Rica. The questions relate to various issues of daily life and the characteristics of the farms. I would like to clarify that we do not represent the government and that this interview is confidential and voluntary.

The interview will last approximately one hour and our donor has authorized us to make a payment in compensation for the time that you have agreed to share with us. Remember that all information that we collect will be treated confidentially and is only for research purposes.

[If the respondent asks how much the payment is, respond that it depends on the duration of the interview. If the respondent wants more clarification, say: "if I may I will come back to the exact amount later." If the respondent asks how we will pay, respond payments are made in cash].

May I begin the interview? [Mark with an X]

□ Yes □ No

PART I Household characteristics [End Part I]

ROUND 1

Thank you for answering the first part of the interview. We'll take a short break and in appreciation of your time and effort in this first part of the interview, our donors have permitted us to pay you. In this envelope there are 5,000 colones for you. [Give payment in the white envelope. If asked who our donor is, answer The Tinker Foundation from the U.S.A., which supports this type of research].
Please open the envelope and make sure that the amount is complete [Give the interviewee time to check the amount].

Without commitment from you, we would like to invite you to make a voluntary and anonymous contribution to a public program called *Bosque Vivo*. This program provides funding to protect and restore the forest and to plant trees in Costa Rica. In your case, the program will use the contributions in the region where your property is located, that is in _____ [Read only the region where the property of the interviewee is located: Limón, Guanacaste (Nicoya), Puntarenas, San José, Cartago].

You are, of course, free to decide whether or not to contribute from the money you just received. We will take the contributions made in your region and give it to *Bosque Vivo*. I'll leave you alone for you to make your decision. If you wish to make a contribution, please put the preferred amount in this envelope and write the amount contributed in a corner of the envelope. Then, please seal the envelope completely.

[Give the envelope marked *contribution* with ID 1 (for round 1), stand back so respondents can make the decision in private].
_____[CONTRIBUTION in colones. Write down the contribution at the end of interview]
[Mark an X over the treatment that does not apply in P.55.]

Thank you very much, now let's continue with the interview.

[13] Original version of the script in Spanish is available upon request.

[End Part II]

ROUND 2

[SUBJECT FACED ONLY ONE OF THE FOLLOWING TREATMENTS]

[**Control treatment**]

Thank you for answering the second part of the interview. We have now finished and in appreciation of your time and effort you will again receive 5,000 colones. Here is your money. [Give payment in the white envelope].

Please open the envelope and make sure that the amount is complete [Give the interviewee time to check the amount].

Without commitment from you, again we would like to invite you to make a voluntary and anonymous contribution to *Bosque Vivo* with this money. Remember that in your case, the program will use the contributions in the region where your property is located, that is in _____ [Read only the region where the property of the interviewee is located: Limón, Guanacaste (Nicoya), Puntarenas, San José, Cartago].

You are, of course, free to decide whether or not to contribute from the money you just received. I'll leave you in private for you to make your decision. If you wish to make a contribution, please put the preferred amount in this envelope and write the amount contributed in a corner of the envelope. Then, please seal the envelope completely.

[Give the envelope marked *contribution* with ID 2 (for round 2), stand back so respondents can make the decision in private].
_____[CONTRIBUTION in colones. Write down the contribution at the end of interview]

[**Additionality rule treatment**]

Thank you for answering the second part of the interview. We have now finished and in appreciation of your time and effort you will again receive 5,000 colones. Here is your money. [Give payment in the white envelope].

Please open the envelope and make sure that the amount is complete [Give the interviewee time to check the amount].

Without commitment from you, again we would like to invite you to make a voluntary and anonymous contribution to *Bosque Vivo* with this money. Remember that in your case, the program will use the contributions in the region where your property is located, that is in _____ [Read only the region where the property of the interviewee is located: Limón, Guanacaste, Puntarenas, San José, Cartago].

However, this time we want to encourage contributions to *Bosque Vivo* by returning to you half of what was contributed. For example, if a person contributes 500 colones, *Boque Vivo* gets those 500 colones and the person will receive 250 colones back from us.

Since our budget is limited, we can only offer incentives to some respondents. In this case, we will offer this

incentive only to those who ... [Read only one of the treatments below, depending on whether or not they qualify. Mark an X over the treatment that does not apply to avoid errors.]

[The person qualifies if contribution≤1,000]	[The person does not qualify if contribution>1,000]
…contributed 1,000 colones or less earlier. Since you contributed 1,000 colones or less in the first part of the interview, you qualify for this incentive. So, for example, contributing 500 colones will now cost only 250 colones, because we will refund 250 colones separately.	…contributed 1,000 colones or less earlier. Since you contributed more than 1,000 colones in the first part of the interview, you do not qualify for this incentive. For a person who qualifies, contributing 500 colones will now cost only 250 colones, because we will refund 250 colones separately. Since you do not qualify, the conditions to make a contribution are the same as before.

You are of course free to decide whether or not to contribute from the money you just received. Do you have any questions? I'll leave you in private for you to make your decision. If you wish to make a contribution, please put the preferred amount in this envelope and write the amount contributed in a corner of the envelope. Then, please seal the envelope completely.

[Give the envelope marked *contribution* with ID 2 (for round 2), stand back so respondents can make the decision in private].
_____[CONTRIBUTION in colones. Write down the contribution at the end of interview]

[If someone wonders why they do not qualify, answer that we do not have money to give the extra payment to all, hence we can only select a few, and now we had selected to give the incentive to those who [repeat selection rule] _____].

[**Reward rule treatment**]

Thank you for answering the second part of the interview. We have now finished and in appreciation of your time and effort you will again receive 5,000 colones. Here is your money. [Give payment in the white envelope].

Please open the envelope and make sure that the amount is complete [Give the interviewee time to check the amount].

Without commitment from you, again we would like to invite you to make a voluntary and anonymous contribution to *Bosque Vivo* with this money. Remember that in your case, the program will use the contributions in the region where your property is located, that is in _____ [Read only the region where the property of interviewee is located: Limón, Guanacaste (Nicoya), Puntarenas, San José, Cartago].

However, this time we want to encourage contributions to *Bosque Vivo* by returning to you half of what was contributed. For example, if a person contributes 500 colones, *Boque Vivo* gets those 500 colones and the person will receive 250 colones back from us.

Since our budget is limited, we can only offer incentives to some respondents. In this case, we will offer this incentive only to those who ... [Read only one of the treatments below, depending on whether or not they qualify. Mark an X over the treatment that does not apply to avoid errors.]

[The person qualifies if contribution≥2,500]	[The person does not qualify if contribution<2,500]
…contributed 2,500 colones or more earlier. Since you contributed 2,500 colones or more in the first part of the interview, you qualify for this incentive. So, for example, contributing 500 colones will now cost only 250 colones, because we will refund 250 colones separately.	…contributed 2,500 colones or more earlier. Since you contributed less than 2,500 colones in the first part of the interview, you do not qualify for this incentive. For a person who qualifies, contributing 500 colones will now cost only 250 colones, because we will refund 250 colones separately. Since you do not qualify, the conditions to make a contribution are the same as before.

You are, of course, free to decide whether or not to contribute from the money you just received. Do you have any questions? I'll leave you in private for you to make your decision. If you wish to make a contribution, please put the preferred amount in this envelope and write the amount contributed in a corner of the envelope. Then, please seal the envelope completely.

[Give the envelope marked *contribution* with ID 2 (for round 2), stand back so respondents can make the decision in private].
_____[CONTRIBUTION in colones. Write down the contribution at the end of interview]

[If someone wonders why they do not qualify, answer that we do not have money to give the extra payment to all, hence we can only select a few, and now we had selected to give the incentive to those who [repeat selection rule] _____].

[**Environmental benefit rule treatment**]

Thank you for answering the second part of the interview. We have now finished and in appreciation of your time and effort you will again receive 5,000 colones. Here is your money. [Give payment in the white envelope].

Please open the envelope and make sure that the amount is complete [Give the interviewee time to check the amount].

Without commitment from you, again we would like to invite you to make a voluntary and anonymous contribution to *Bosque Vivo* with this money. Remember that in your case, the program will use the contributions in the region where your property is located, that is in _____ [Read only the region where the property of the interviewee is located: Limón, Guanacaste (Nicoya), Puntarenas, San José, Cartago].

However, this time we want to encourage contributions to *Bosque Vivo* by returning to you half of what was contributed. For example, if a person contributes 500 colones, *Boque Vivo* gets those 500 colones and the person

will receive 250 colones back from us.

Since our budget is limited, we can only offer incentives to some respondents. In this case, we will offer this incentive only to those who ... [Read only one of the treatments below, depending on whether or not they qualify. Mark an X over the treatment that does not apply to avoid errors.]

[The person qualifies if his/her property is in Nicoya.]	[The person does not qualify if his/her property is not in Nicoya.]
... have property in a region located in a dry area as they face greater problems with water quality. To promote greater contributions in those dry regions, we decided to focus on properties in Nicoya. Given that your farm is in Nicoya, you qualify for the incentive. So for example, contributing 500 colones will now cost only 250 colones, because we will refund 250 colones separately.	... have property in a region located in a dry area as they face greater problems with water quality. To promote greater contributions in those dry regions, we decided to focus on properties in Nicoya. Given that your farm is outside Nicoya, you do not qualify for the incentive. For a person who qualifies, contributing 500 colones will now cost only 250 colones, because we will refund 250 colones separately. Since you do not qualify, the conditions to make a contribution are the same as before.

You are of course free to decide whether or not to contribute from the money you just received. Do you have any questions? I'll leave you in private for you to make your decision. If you wish to make a contribution, please put the preferred amount in this envelope and write the amount contributed in a corner of the envelope. Then, please seal the envelope completely.

[Give the envelope marked *contribution* with ID 2 (for round 2), stand back so respondents can make the decision in private].
_____[CONTRIBUTION in colones. Write down the contribution at the end of interview]

[If someone wonders why they do not qualify, answer that we do not have money to give the extra payment to all, hence we can only select a few, and now we had selected to give the incentive to those who [repeat selection rule] _____].

We have now finished the survey, and I would only like ask for your signature on this receipt, merely for administrative reasons. [Provide receipt and request signature]

I would like to thank you for your time and participation. Thank you very much!

Paper IV

Payments in Cash or in Kind for Ecosystem Services:

Stated Preferences of Costa Rican Landowners

Anna Nordén[1]

Abstract

This paper investigates landowners' preferences for type of payment, cash or in kind, for the provision of ecosystem services. A choice experiment analysis focusing on the effect of different levels of cash and in-kind payments on participation in a payment for ecosystem services (PES) contract is provided. We use an educational in-kind payment in the form of days of practical training offered free of charge to the recipients. The results indicate a positive correlation between participation in a PES contract and the magnitude of the cash payment—higher cash payments increase the probability of participation—while participation seems uncorrelated with the magnitude of the in-kind payment. We also find that both in-kind and cash payments increase the likelihood of participation in shorter PES contracts (i.e., 5 years), while in-kind payments have no significant effect on participation in longer contracts (i.e., 15 years). Higher levels of cash payment seem to be what is needed to increase the likelihood of participation in longer contracts. In addition, we investigate heterogeneity in preferences for type of payment, which can help policymakers better target payment types to specific groups of landowners.

Keywords: payment for ecosystem services, cash payments, in-kind payments, stated preferences, Costa Rica.

JEL classification: Q28, Q57

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1. Introduction

Both cash and in-kind payments (e.g., capacity building, seeds provision, tax credits, and grants to community-based organizations) are increasingly popular to use as incentives for the provision of ecosystem services. In the beginning of the 2000s there were nearly 200 payment for ecosystem services (PES) programs in place (Landell-Mills and Porras 2002) and since then the number has increased even more (Pattanayak et al. 2010). PES offers conditional payments to motivate private landowners to invest in land-use practices that lead to conservation or production of ecosystem services (Ferraro and Kiss 2002, Wunder 2005). Although cash payments are the most common means of compensation, some programs offer payments in kind. The Chinese national sloping lands conservation program (SLCP) offers tree seedlings to subsidize reforestation or agro-forestry and grain to replace forgone production. The PES program in Los Negros, Bolivia, offers payments in beehives supplemented with apicultural training (Asquith et al. 2008). In Pimampiro, Ecuador, conservation of cloud forest is compensated by supporting the development of orchid nurseries, while land tenure is the main incentive mechanism for watershed protection and carbon sequestration in a PES program in Sumberjaya, Indonesia (Hangrove and Chandler 2004).[2]

The voluntary nature[3] of most PES programs makes the understanding of the relationship between the type of payment and participation a key issue as the effectiveness of a program hinges on enough landowners enrolling and fulfilling their management requirements (Pagiola 2008). However, knowledge about preferences regarding the type of payment for the provision of ecosystem services is scarce (for a few exceptions see Porras and Hope 2005, Robertson and Wunder 2005, Porras et al. 2007, Zabel and Engel 2010). Findings suggest that offering a combination of cash and in-kind payments may increase the likelihood of adopting conservation practices compared with only offering cash payments (Porras and Hope 2005, Porras et al. 2007). Further, it has been shown that in-kind payments tend to be preferred by those with a long distance to markets (Zabel and Engel 2010).

[2] See further examples in case study profiles available at http://www.watershadmarkets.org

[3] An exception is the Chinese SLCP, where some involuntary enrollment has been reported (Bennett 2008). Sommerville et al. (2009: 2) argue that, although participation in a PES scheme is voluntary, "service providers do not necessarily have the choice whether or not to provide the service, such as in cases where land-use change is illegal." However, such restrictions are seldom (if ever) perfectly enforced, and landowners may choose to deforest, even if such an action is illegal. It is estimated that roughly 85 percent of all tropical deforestation occurs illegally.

This paper investigates landowners' preferences for the type of payment, cash or in kind, for participation in a PES contract. Unlike previous studies (Porras and Hope 2005, Robertson and Wunder 2005, Porras et al. 2007, Zabel and Engel 2010), the present study provides a choice experiment analysis focusing on the effect of different levels of the cash and in-kind payment on participation in a PES contract. This enables us to provide policymakers with information regarding the relationship between the type of payment used and participation. This information can be particularly relevant and helpful in the process of designing payment schemes. Further, we provide information regarding heterogeneity in payment type preferences, which can help policymakers better target payment types to specific groups of landowners. For instance, on-farm income dependency has been shown to be an important explanatory variable for participation in PES contracts when payments are made in cash (e.g., Zbinden and Lee 2005, Arriagada 2009). Therefore, this variable is of great interest when investigating heterogeneity in preferences for type of payment.

Our survey-based choice experiment is based on the Costa Rican Pagos por Servicios Ambientales (PSA) program and was conducted with landowners from Costa Rica. The PSA program was the first of its kind and since 1997 it has made cash payments mostly related to forest conservation for nearly half a million hectares of land, which accounts for almost 10% of the total area of Costa Rica (Pattanayak et al. 2010). This program is frequently used as a blueprint for PES programs in other countries; therefore the results of this paper may provide relevant input for policymakers designing new PES contracts. The main reason for using a choice experiment to investigate landowners' preferences is that with this method, insights are gained about landowners' preferences of payment types and/or payment levels presently not available or not being used.

In our study, landowners were asked to make trade-offs between cash—colones[4] per year—and an educational in-kind payment—days per year of practical training, free of charge to the recipients. The two main reasons for using an educational in-kind payment were 1) that there is a demand for such courses and 2) that such educational in-kind payments are not resalable, implying a clear trade-off to the cash payment.[5] However, the welfare effect of in-kind payments that cannot be directly sold in a market is determined by their returns over time. In the case of the Bolivian PES program in Los Negros, for instance, the net present values of beehive transfers ranged from approximately −US$15/ha/year (negative value) to

[4] 500 colones = US$1 (November 2011).

[5] In contrast to tree seedlings and grain, which are goods that can be sold on a market and turned into cash, capacity building cannot be sold. In the former case the in-kind payment might as well be treated as cash (Currie and Gahvari 2008).

+US$13/ha/year (Robertson and Wunder 2005). This diversity in return from apiculture was shown to be highly dependent on the beekeeping skills of the landowner (Asquith et al. 2008). Therefore, the welfare outcomes of providing in-kind payments are dependent on both the characteristics of the recipient and the specific in-kind payment provided. This has implications for the design of our choice experiment. Even though the levels of the educational in-kind payment were made comparable to the levels of the cash payment in terms of the cost to the policymaker, these levels may not be comparable in terms of benefit. For instance, there is an extra cost for recipients to benefit from the educational in-kind payment since practical training demands time. Thus, the utility derived from a recipient's perspective may not be comparable between the cash and the in-kind payment levels. Nevertheless, the objective of this study is to compare levels within the same payment type to explore the relationship between participation in a PES contract and the type of payment used.

The use of in-kind payments for ecosystem services provision is often motivated by the potentially more lasting benefits as cash payments are more vulnerable to rapid and less welfare-enhancing spending[6] (Robertson and Wunder 2005). In the PES context, people express concern that cash payments are invested in timber extraction in non-contracted land. This kind of leakage (or slippage) obviously has negative effects on the efficiency of PES as then the deforestation is simply spatially shifted. Recent empirical evidence for the Mexican PSAH (Pago de Servicios Ambientales Hidrológicos) program shows an approximately 4 percent average reduction in program efficiency due to leakage, as a result of increased deforestation on property belonging to program recipients (Alix-Garcia et al. 2012).[7] Cash payments can also, in some contexts, be confounded with a payment for the land itself. For example, non-participation in soil conservation programs in Kenya was motivated by the perception that direct cash payment could weaken the ownership of the land (Porras et al. 2007). Similar perceptions are also found in Bolivia (Robertson and Wunder 2005, Asquith et al. 2008). Further, in situations where individuals' ability to manage cash income is expected

[6] Individuals have been shown to have preferences for immediate utility over delayed (for an overview see Frederick et al. 2002, and see Cardenas and Carpenter 2008, for an overview of results from developing countries). Further, self-control issues, i.e., the individual's ability to control his/her behavior in order to obtain some reward later (for a theoretical model see Thaler and Shefrin 1981), may result in cash payments being spent on less desirable commodities (e.g., alcohol and tobacco) with no long-term investment properties. To avoid this, it has been argued that people constrain themselves in some way to make sure that they don't have immediate access to their cash (e.g., put it in the bank). There are also studies showing that some institutions in developing countries—such as "rotating saving"—exist because they help people manage self-control problems through commitment devices (e.g., Gugerty 2007).

[7] However, there is considerable heterogeneity in their results, where the leakage is considerable more severe in the poorest quartile than in the wealthiest.

to vary, and hence the same cash income could provide different outcome for different individuals, in-kind payments have theoretically been shown to be more welfare improving (Thurow 1974). Still, cash payment allows for greater flexibility in the use of resources and more readily compensates for the lost income that the introduced land-use restrictions of the PES contract conditions imply. Cash payment is also less prone to be seen as paternalism.[8]

Even though the choice of payment type is far from obvious from a welfare perspective, there is some evidence that people might behave differently depending on in which form the incentive is provided. Heyman and Ariely (2004), for instance, argue that recipients seem to be more likely to view in-kind payments as compatible with reciprocal exchange and are therefore less concerned about the magnitude of in-kind payments. On the contrary, Gneezy and Rustichini (2000) found evidence that magnitude seems to matter for cash payments. In their real-behavioral experiments, students who were paid small amounts of cash performed worse, both mentally (answering a set of questions taken form an IQ test) and physically (collecting donations for a charitable cause), than those who were not paid. Further, in labor economics, it has been found that monetary and non-monetary incentives are treated differently by employees. Monetary incentives seem to be treated as compensation (for doing hard work), while non-monetary incentives are rather considered a means of recognition (e.g., Pfister 2007, Kube et al. 2012). In line with this, Lacetera and Macis (2010) show that there is a difference in hypothetical willingness to donate blood depending on whether the payment is made in cash (10 euro) or non-cash (a 10-euro voucher to purchase books or food). In their study, cash payments were found to have a larger probability of crowding out blood donations than non-cash payments. Additionally, by using a field experiment where subjects were given the opportunity to become blood donors with or without any compensation, Mellström and Johannesson (2008) show that crowding out of blood donations can be alleviated by allowing individuals to donate the payment to charity. However, gender differences seem to be driving their results as blood donations by men were unaffected.

This possibility of obtaining different outcomes depending on the type of payment used is important in understanding the landowner's decision to participate in a PES contract. Nevertheless, the decision to participate in a PES contract is a result of a complex decision-making process that is only to a limited extent affected by the payment itself. For instance,

[8] Since most PES arrangements are voluntary, providing payments as in-kind would be an example of libertarian paternalism (Sunstein and Thaler 2003), where institutions (both private and public) steer participants (who have volunteered) in directions to promote their and others' welfare by ensuring investments to secure the provision of ecosystem services (see also Camerer et al. 2003).

motivation to participate might be due to peer pressure or learning, e.g., the fact that the neighbor takes part and other signaling effects, where in search of social approval the individual tries to signal behavior defined as "good" based on social norms and values (for economic models incorporating social approval, see e.g., Akerlof 1980, Hollander 1990, Bénabou and Tirole 2006, Ellingsen and Johannesson 2008, Andreoni and Bernheim 2009). Further, participation might be motivated by an individual's tendency to cooperate depending on the cooperation of others, i.e., conditional cooperators (experimental evidence is found in Fischbacher et al. 2001, for an overview see Gächter 2007). Although in the present paper we do not specifically investigate individuals' motivation for participating in a PES contract, motivation is certainly an important factor explaining why individuals might behave differently depending on the type of payment.

The remainder of the paper is structured as follows: Section 2 describes the attributes and levels in the choice experiment, as well as the design and the econometric model. The results are presented and discussed in Section 3 and Section 4 concludes the paper.

2. Design of the Payment for Ecosystem Services Contract Choice Experiment

To investigate the relationship between participation in a PES contract and the type of payment, we used a choice experiment where the respondents were asked to choose their preferred contract for a hypothetical piece of land of 10 hectares. Since payments in the current PSA program in Costa Rica are made per hectare and land size is highly diversified, the number of hectares in the scenario was fixed so that each respondent faced a trade-off between the same stakes. The size of 10 hectares was chosen as it represents the minimum land size of most respondents. Even though 10 hectares of land was not necessarily what the respondent owned, the discussions with the respondents in the pilot studies show that most respondents could relate to this size of land.

The scenario, reported in Figure 1,[9] was read to each respondent by an enumerator.[10] The scenario informed the respondents of the requirements stated in the contract, e.g., that the area has to be fenced, cattle cannot enter, there must be firebreaks, no trees can be cut, and hunting on the land under contract is not allowed. These requirements are the same as those in the current PSA contracts. In the PSA program, there are four types of contracts: forest conservation, reforestation (timber plantation), agroforestry, and natural regeneration.

[9] The original scenario in Spanish is available upon request.
[10] Literacy (defined as those in the population 15 years or older who could read and write) in Costa Rica is almost 95% (CIA 2012 Aug.). However, to make sure that all respondents received the same information, the scenario was always read to the respondents by well-trained enumerators.

However, agroforestry and natural regeneration account for a rather small share of both applications and the total area and were therefore not taken into consideration in the choice experiment scenario. Even though the choice experiment used hypothetical land, it was found during the pilot studies that the most common reason among those who turned down an offered conservation contract was that they did not currently have forest. Thus, to avoid getting too small samples, which would have been the case if forest conservation and reforestation contracts were investigated separately, forest conservation and reforestation were placed together in the choice experiment scenario. Independently of category (conservation or reforestation), all PSA contracts in Costa Rica carry the same requirements, which are presented in the scenario.

Figure 1. Choice experiment scenario read by the enumerators.

Ok, we have now reached the part of the survey where we would like to know what kind of compensation people would like to receive in exchange for reforesting or conserving the forest. That is, let's say you are offered different types of payments in the form of a contract for planting trees or protecting forest on your property. The contract would last 5, 10, or 15 years during which you would commit to reforesting or protecting the forest. Now, having an area under a reforestation or forest protection contract means that the area has to be fenced, cattle cannot enter, there have to be clear boundary lines around the area, and no one can cut down trees or hunt in the area under contract. As a reward for this commitment, you would receive a compensation in the form of cash, paid days of practical training, or both.

Now, paid in days of practical training means that you or someone you designate would receive a number of days of practical training, free of charge, in an area of relevance for your land, such as cattle or crops improvement, tree planting, land administration or tourism management, or any other topic that you might find useful. So, now I will present 5 different situations to you, each one with three different options. For each situation, I want you to tell me which one you would choose. Before you start choosing, let's assume that we are talking about a contract for 10 hectares of land. Of course, 10 hectares is not necessarily what you have in reality, but please make your decision as if this were the case.

[Show the first situation]. As you can see here, in Contract number 1, the contract would last [read the length of the contract] and have a payment in training of [read days per year of practical training] and a cash payment of [read cash payment per year]. Net payment means the amount that would remain after covering all costs required, and in the case of reforestation, also after paying the costs of reforesting this area. This money maintains its value over time. Meanwhile, in Contract number 2, the contract would last [read the length of the contract] and have a payment in training of [read days per year of practical training] and a cash payment of [read cash payment per year]. Finally, you can always choose no contract at all. [Repeat this text for each situation].

2.1 The attributes and their levels

Since payment for ecosystem services contracts already exists, the key attributes of the current contracts were included, i.e., duration and type of payment. The levels of the different attributes were identified and developed through focus groups and interviews with officials of the PSA program and organizations[11] with a long history of providing training to landowners in Costa Rica. The survey was tested in various pilot studies with a focus on the levels and the understanding of them.[12] This preparatory work was crucial in order to ensure that the attributes and attribute levels in the choice experiment were well understood by the respondents. A description of the attributes and their levels is given in Table 1.

Table 1. The attributes and the attribute levels of the choice experiment.

Attributes	**Description**	**Levels**
Duration of contract	The length of the contract in years.	0 (no contract), 5, 10, 15 years
Payment in training	Annual educational in-kind payment for a PES contract described as the number of days of practical training, free of charge, offered to the respondent or someone appointed by the respondent.	0, 10, 15, 20 days per year.
Payment in cash	Annual cash payment for a PES contract described as the net payment, i.e., money left after covering the investment costs, in present value.	0; 100,000; 150,000; 200,000 colones net per year.

* US$1=500 colones (November 2011 exchange rate)

Duration of the contract

The information that emerged as a result of discussions with policymakers and various pilot studies shows that most respondents seem to have strong preferences for shorter

[11] The organizations included the program for livestock and ecosystem management (grupo Ganadería y Manejo del Medio Ambiente – GAMMA) and forest and forest management in Central America (FINFOR) at the Tropical Agricultural Research and Higher Education Center (Centro Agronomico Trópical de Investigación y Enseñanza (CATIE)) and also the National Institute of Learning (Instituto Naciónal de Aprendisaje (INA)).

[12] We conducted two focus groups with landowners and officials of the PSA program as well as interviews with educational institutions where the relevant attributes and attribute levels were defined. In the pilot studies, the respondents were asked follow-up questions regarding their understanding and the importance of the attributes in the choice experiment. We ran a total of five pilot studies; the attribute levels and descriptions of the attributes were improved according to the outcomes.

contracts; thus, duration could in many cases be treated as a negative attribute. The final contract duration levels were 5, 10, and 15 years. These levels did not deviate much from the levels that were used for PSA contracts until 2011 (5 years for forest conservation and 15 years for reforestation).[13] However, it had enough variation to identify the landowners' preferences regarding contract duration.

In-kind payment

In the current PSA program there are no in-kind payments. In the choice experiment, however, we use an educational in-kind payment in the form of days of practical training offered free of charge to the recipients. According to the pilot studies and organizations offering various kinds of training programs, preferences for the type of training are highly heterogeneous. Therefore, to not evaluate the preferences for a specific capacity-building course, the respondents were informed that they were able to choose the practical training in an area of relevance for their land. The practical training can hence be seen as a voucher for the tuition fee, where the recipients choose which courses they would benefit from. The most commonly requested practical training courses in the pilot studies were used to create some examples of potential courses to choose from, e.g., cattle or crops improvement, tree planting, land administration, and tourism management.

An educational in-kind was used for three main reasons. First, according to some of the institutions offering capacity-building courses, landowners highly appreciate and demand such services. Second, despite the fact that there is a market offering different capacity-building courses to landowners in Costa Rica, such educational in-kind payments are not resalable, which gives a clear trade-off to the cash payment. Thirdly, education has particular long-term benefits by equipping landowners with knowledge of alternative income opportunities. For example, capacity building with respect to production technologies to reduce the need to purchase animal feed may lead to increased income from livestock farming and more trees (see examples of silvopastural practices in Pagiola et al. 2005).

On the negative side, as Blank (2002) points out, gains from education (or other in-kind payments) might look more promising to society than to the individual herself as benefits from such consumption might be received by others than the person receiving the training. Further, it seems reasonable to assume diminishing marginal utility of training as, after a

[13] In 2012, forest conservation contracts were extended to last for 10 years. The yearly payment remained at the 2011 level of 64US$ per hectare.

certain amount of days of practical training, the benefits of improved knowledge—ultimately leading to potentially increased income in the form of improved or new production techniques on the farm—might be outweighed by the cost of time for each day of extra training. Thus, in the choice experiment it was important to not exceed a reasonable and credible number of days of training per year that in reality would be received as a benefit and not as a burden. After consulting both suppliers and users of capacity-building courses, 20 days of practical training was set as the maximum amount of days per year. It is important to note that in the scenario, the offered training could be beneficial to the landowner by educating himself/herself, family members, or farm staff.

One central claim made by critics of stated preference methods is that the respondent does not sensitively react to the extent of increased compensation, implying insensitivity to scope. This problem occurs if respondents are unable to distinguish between the different attribute levels, which is often connected to situations where the respondents are asked to value a good or service unfamiliar to them.[14] Even though in-kind payments are not used in the current PSA program, Costa Rican farmers are very familiar with capacity-building programs and many of the respondents had previously paid to participate in such courses.

Cash payment

All payments to contracted landowners in Costa Rica are currently made in cash. A forest conservation contract of 10 hectares pays up to 320,000 colones per year (equivalent to 640 USD per year), according to the payment levels in 2011. As a reference, a day of farm labor would cost on average 10,000 colones. Subtracting only the mandatory cost of a forest engineer, or around 25% of the total payment, the landowner would potentially be left with 240,000 colones per year for 10 hectares (equivalent to 480 USD). This money has to cover the costs to fulfill the requirements of the contract and the transaction costs. For reforestation the payment is higher and paid out during the first five years as the costs (e.g., for tree seedlings and planting) are much higher than for forest conservation. Clearly, net payments are highly diversified between landowners. Therefore, only net payments at present value were used in the choice experiment. Note that the lowest level of 0 colon means that no money is left after covering all the investment costs.

[14] One such example is protection of endangered species, where the respondents' monetary value has been shown to be insensitive to the amount of saved species (for further discussions regarding insensitivity of scope see for example Foster and Mourato 2003, and Goldberg and Roosen 2007).

The levels of the cash payment were made comparable to the levels of the educational in-kind payment in terms of the cost for the policymaker. A capacity-building course in agriculture, livestock, or ecotourism generally has a tuition fee per day and person starting at 10.000 colones (20 USD).[15] The highest level of the in-kind payment, i.e., 20 days of practical training, at the current market price would thus cost around 200,000 colones, which is set to be the highest net cash payment level. This is comparable to the current cash payment level for forest conservation in the PSA program if investment costs are low.

2.2 Design of choice sets

When designing a choice experiment, all researchers face an intrinsic problem of how much information or complexity to incorporate. Even though some studies have focused on the effect of the choice set complexity and found that it matters for the ability to make consistent choices or the rate of non-responses (e.g., DeShazo and Fermo 2002, Hensher 2006, Carlsson and Martinsson 2008), in the end the researcher has to make a judgment regarding the capacity of the targeted audience. In this study, most respondents had relatively low levels of education and were not experienced with this type of multiple choice situations. Therefore, pilot studies with 3-6 choice sets were conducted. We found that the majority of respondents maintained a high level of concentration when answering three choice sets. However, when we added up to three more choice sets, we observed a decreased concentration in the last choice sets. Thus, to achieve a balance between collecting as much information as possible and the cognitive burden of answering many choice sets, five choice sets were used in the main study.

We used a cyclical design to construct the choice sets.[16] A cyclical design is based on an orthogonal approach and the attribute level for each new alternative is set to be the next higher attribute level. When the highest level is reached, the new alternative is assigned the lowest level (Bunch et al. 1996). In total our final design consisted of ten choice sets blocked into two blocks with five choice sets in each version [1,2,3,4,5] and [6,7,8,9,10]. To test for order effects, two more blocks were created by reordering the first five choice sets into two

[15] The cost was calculated in collaboration with the National Institute for Learning (INA) in Costa Rica, one of the main suppliers of training courses for agriculture and forestry in Costa Rica. The cost of a course depends on group size, the need for material, and travel costs for leaders and teachers.

[16] From the full factorial design, twelve alternatives were created in a fractional factorial design procedure using the SAS 9.3 software. When creating the fractional factorial outcome, the possibility of having the same attribute level for cash and in-kind payment was eliminated. Further, according to results of the pilot studies, two of the choice sets had or were expected to have an alternative that was never chosen. Therefore, these choice sets were excluded in the final questionnaire.

more versions, [2,3,4,5,1] and [3,4,5,2,1]. These four versions were randomly distributed between respondents and enumerators.

As can be seen in the choice set in Figure 2, the respondents were asked to make their choice between two contract alternatives and an "opt-out" alternative, i.e., preferring not to have any contract. There was always an "opt-out" alternative since participation is voluntary in most PES programs. Those who always chose the "opt-out" alternative were asked a follow-up question about their reason for this preference. Respondents were also asked questions about prior participation in capacity-building courses as well as their need for such training. These questions were asked as the current demand for practical training may be an important explanatory variable for preferences regarding this type payment.

Figure 2. Choice set as shown to the subject.

	Contract 1	Contract 2	No Contract
Duration of contract	15 years	5 years	I don't want a contract
Payment in training 	20 days per year	0 days per year	
Payment in cash 	150,000 colones net per year (in present value)	200,000 colones net per year (in present value)	
Your choice			

2.3 Econometric model

To analyze the choice experiment, we used a standard random utility approach (McFadden 1974, Manski 1977). In this model the utility of an individual's choice is decomposed into a non-random component, which can be observed, and a stochastic non-observable term. It is assumed that the respondents consider the alternatives and the attributes offered in a specific choice situation and then choose the alternative that would give them the highest utility.

Our estimations of the parameters initiate in a basic model, referred to as **Model 1**, where the utility of individual *q* from alternative *i* (Contract1; Contract2; No Contract) is expressed as

$$U_{iq} = \alpha_i + \boldsymbol{x_i'\beta} + \varepsilon_{iq}$$

where α_i is an alternative specific constant capturing the intrinsic preferences for having a contract. $\boldsymbol{x_i}$ is a vector of the attributes describing alternative *i*, namely the duration of the contract and different levels of payments in cash and in kind (including no cash payment and no practical training). The vector of parameters for the attributes, $\boldsymbol{\beta}$, is the focus of our estimation. ε_{iq} is the stochastic term representing the unobservable factors or measurement errors. Individual *q* would choose alternative *i* over alternative *j* if $U_{iq} > U_{jq}$.

In this basic model the relationship between participation and the type of payment is estimated without taking into account heterogeneity in preferences due to individual characteristics. Additional information regarding to what extent observable socioeconomic characteristics could explain the preference for participation and preferences for different types of payments could be used by policymakers to target certain payment types at different landowners. Thus, a full model including interaction terms between socioeconomic variables and the alternative specific constant, i.e., having a contract or not, and between socio-economic variables and the levels of cash and in-kind payments is estimated. We refer to this model as **Model 2**, which is specified as

$$U_{iq} = \alpha_i + \boldsymbol{x_i'\beta} + \left(\boldsymbol{z_q}\alpha_i\right)'\boldsymbol{\gamma} + \left(\boldsymbol{x_i'z_q}\right)'\boldsymbol{\delta} + \varepsilon_{iq},$$

where in addition to Model 1, $\left(\boldsymbol{z_q}\alpha_i\right)$ is a vector of interaction terms between socio-economic variables $(\boldsymbol{z_q})$ and the alternative specific constant (α_i), and $\boldsymbol{\gamma}$ captures the heterogeneity in preferences for participating in a contract as a function of individual characteristics (e.g., age, gender, and on-farm income dependency). $\left(\boldsymbol{x_i'z_q}\right)$ is a vector of interaction terms between the attributes and socioeconomic variables, and $\boldsymbol{\delta}$ captures the heterogeneity in preferences for the attributes that is due to individual characteristics.

The parameters $(\alpha, \beta, \gamma, \delta)$ are estimated using a standard conditional logit model, which applies maximum likelihood estimation. Even if observed heterogeneity is captured in Model 2 by including socioeconomic variables, it should be noted that the standard

conditional logit model is limited in the taste variation modeling as the unobservable heterogeneity is not captured.[17]

2.4 Hypotheses to be tested

Previous studies have found differences between payment types in terms of how people react to different levels of the payment. In general, people seem sensitive to the level of cash payments while levels do not tend to matter for the in-kind payment (e.g., Gneezy and Rustichini 2000, Heyman and Ariely 2004, Pfister 2007, Kube et al. 2012). In this paper, the following hypotheses regarding the relationship between payment levels (i.e., cash and in kind) and participation in payment for ecosystem services contracts will be tested:

Hypothesis 1: Participation in a PES contract is positively correlated with the magnitude of the cash payment, $\beta_{100\text{cash}} < \beta_{150\text{cash}}$; $\beta_{100\text{cash}} < \beta_{200\text{cash}}$; $\beta_{150\text{cash}} < \beta_{200\text{cash}}$.

Hypothesis 2: Participation in a PES contract is uncorrelated with the magnitude of the in-kind payment, i.e., $\beta_{10\text{days}} = \beta_{15\text{days}}$; $\beta_{10\text{days}} = \beta_{20\text{days}}$; $\beta_{15\text{days}} = \beta_{20\text{days}}$.

Hypotheses 1 and 2 are first tested by using the parameters (β) estimated by a standard conditional logit of Model 1. Heterogeneity is not considered in this basic model. To capture heterogeneity in preferences for the payment attributes, the hypotheses presented above are also tested using parameters estimated by a standard conditional logit of Model 2. In these tests, Hypothesis 1 and 2 are tested by adding the estimated parameters capturing heterogeneity in preferences for the payment attributes that is due to individual characteristics (δ) to the estimated parameters of the levels of payments (β). The specific hypotheses are presented in Table A4 in the Appendix.

[17] For comparison, the estimates of the random parameter logit (RPL) model (McFadden and Train 2000) are reported in Table A2 in the Appendix. We used a PRL model where the panel properties, i.e., that the same respondent is making repeated choices, are taken into account. The results regarding the relationship between participation and the type of payment are similar to the result of the conditional standard logit model. Thus, we will concentrate our analysis on the conditional logit model.

2.5 Data collection

We used two key databases to obtain the sampling frame: i) all applications received by the agency responsible for the PSA program (National Fund for Forest Financing – FONAFIFO) in 2011[18] and ii) all registered farms, almost 46,550, in Costa Rica from the census conducted by the Ministry of Livestock and Agriculture (MAG) in 2006 and 2007. The data collection was concentrated to three out of nine geographical regions of FONAFIFO: Guápiles, San José Oriental, and Nicoya.[19] These regions were selected on recommendation by FONAFIFO as data is available and they represent both urban and rural regions. These three regions comprise about 40% of all applications received by FONAFIFO in 2011. The sample was created by randomly selecting almost 25% of the observations in the FONAFIFO dataset for the selected regions and, in addition, 40 observations per region from the MAG dataset. The intensity of sampling of the FONAFIFO dataset implies that there is an overrepresentation of applicants to the current PES program compared with the population.

In-person interviews were conducted only with the land use decision-maker. We focused on private households, which represent about 90% of all applicants to the PSA program. Corporations and indigenous reserves were excluded in the analysis as our main interest is the preferences of private agents. Appointments with respondents were made through telephone calls before visiting their home.[20] Most of those who were contacted were interested in participating in the study. However, due to logistical reasons it was impossible to meet the preferences of all landowners regarding date and time, hence in the end around 50% of all those who were contacted were actually interviewed. There was no discernible pattern characterizing those who were not interviewed.[21] The choice sets were randomly distributed between the individuals in each region and between the enumerators.

[18] FONAFIFO receives most applications in February and March each year. For forest conservation the application process ends at the end of March, but the other categories are open until the end of December the same year. In this study the dataset included all applications up to April 2011.

[19] The region of Limón was used for the pilot studies. This region is similar to the other regions used for the main study.

[20] Telecommunication is extensive in Costa Rica with around 60% of the rural households connected to a fixed phone line (World Telecommunication and ICT Development Report 2010). In 2011, Costa Rica had over 4 million mobile cellular telephone subscribers (the total population in Costa Rica was calculated to around 4.6 million people the same year) (CIA 2012 Aug.).

[21] Unfortunately, detailed socioeconomic information regarding non-respondents is not available.

3. Results

The choice experiment was integrated in a survey regarding the socio-economic impact of the payment for ecosystem services (PSA) program in Costa Rica[22] conducted from November 2011 to January 2012. A total of 246 successful interviews[23] with private landowners were undertaken. To check for differences between the regions, where San José represents an urban region while Guapiles and Nicoya represent more rural regions, the descriptive statistics are divided by region and regional dummy variables are included in the econometric analysis. The descriptive statistics of the pooled sample, the subsample for each region, and the subsamples of applicants and non-applicants to the current PSA program are presented in Table 2.

3.1 Descriptive statistics

The majority of the respondents, over 80%, are men and the average age is 58 years. The educational level of the respondents is generally quite low, with the average highest level attained being incomplete secondary school. More than half of the respondents are on-farm income dependent and the average land size is around 108 hectares while the median is 34 hectares. Looking at the regions, the average size of the land is significantly different between the regions, with Guapiles having the smallest average land size and Nicoya the largest (significant at a 1% level according to a Kruskal-Wallis test).

Moreover, around 50% of the respondents had previously participated in some kind of capacity building and over 80% of the respondents stated a need for a training program connected to their land. The most frequently demanded courses are connected to agriculture and livestock, but tourism and forestry are also popular topics. Landowners from Guapiles have significantly less experience of capacity building than those from the other regions, while San José had significantly more (significant at a 1% level according to a Kruskal-Wallis test). This might be explained by the region of San José having proximity to markets offering such services.

[22] The comprehensive study consisted of questions regarding socio-economic variables, land characteristics, and knowledge of PES, as well as specific questions regarding the PES contract if the respondent was participating. A natural field experiment was also connected to the study. Alpízar et al. (2013) and a report by Alpízar et al. (2012) are connected to the data collected.

[23] Two respondents did not answer all five choice sets and were therefore excluded.

More than half of the respondents had applied to the PSA program and 40% had at some point held a contract. At the time the choice experiment was conducted, 31% of the respondents held an ongoing contract. This high proportion of landowners holding a contract or applying to the program is a result of deliberate use of applicants to the program within the sample frame and hence does not reflect the proportion of applicants in the population. There are some differences between the regions, with Nicoya having significantly higher shares of applicants to the PSA program.

Applicants to the PSA program have a significantly higher education level (completed secondary school) than non-applicants (completed primary school) (significant at a 5% level according to a Mann-Whitney test). Similar results are found for on-farm income, where applicants are less likely than non-applicants to be dependent on on-farm income (36% vs. 66%, significant at a 5% level according to a Mann-Whitney test). Further, land size is significantly larger for applicants. The results of a probit model (see Table A1a in Appendix) show that on-farm income independency, farm size, and higher education level significantly increase the probability to apply to the current PSA program. These results support earlier findings that those participating in the PSA program have larger farms, lower dependency on on-farm income, and higher education levels (Miranda et al. 2003, Ortiz et al. 2003, Hope et al. 2005, Zbinden and Lee 2005, Sierra and Russman 2006, Arriagada et al. 2009, Porras 2010).

Table 2. Descriptive statistics presented as mean values and standard deviation in parentheses.

Variable	Description	Pooled sample	Guapiles	San José	Nicoya	p-value[1]	Applied	Never applied	p-value[2]
Age	The respondent's age in years	58 years (12.27)	61 years (12.40)	57 years (12.50)	59 years (12.80)	0.12	56 years (11.91)	61 years (12.12)	<0.01
Male	Dummy for male (1=male, 0=female)	86%	85%	88%	85%	0.79	84%	88%	0.32
Education	The respondent's highest level of education	Incomplete secondary school (2.02)	Complete primary school (1.90)	Incomplete secondary school (1.94)	Incomplete secondary school (2.11)	0.08	Complete secondary school (2.12)	Complete primary school (1.55)	<0.01
Household members	Number of household members	3.57 (1.63)	3.33 (1.27)	3.89 (1.86)	3.36 (1.48)	0.16	3.42 (1.44)	3.72 (1.80)	0.45
Farm income	Dummy for whether the respondent's main income comes from the farm (1=yes, 0=no)	51%	48%	58%	45%	0.17	36%	66%	<0.01
Average land size	Average land size measured in hectare.	108 ha (189)	51 ha (88)	108 ha (215)	135 ha (191)	<0.01	175 ha (241)	39 ha (59)	<0.01
Median land size	Median land size measured in hectares.	34 ha	15.5 ha	30 ha	56.5 ha		73 ha	16.5 ha	
Experienced capacitation	Dummy for whether the respondent participated in capacity building before (1=yes, 0=no)	51%	28%	59%	53%	<0.01	49%	53%	0.52
Stated need for capacitation	Dummy for whether the respondent stated a need for capacity building before (1=yes, 0=no)	83%	67%	87%	86%	<0.01	84%	82%	0.61
Applied to the program	Dummy for whether the respondent applied to the PES program (1=yes, 0=no)	51%	30%	52%	59%	<0.01	100%	N/A	
Contract at any time	Dummy for if the respondent has held a contract at any time (1=yes, 0=no)	40%	26%	36%	51%	<0.01	79%	N/A	
Contract at present	Dummy for if the respondent holds a contract at time of the survey (1=yes, 0=no)	31%	20%	31%	36%	0.13	62%	N/A	
# of respondents		246	46	100	100		125	121	

[1] p-values according to Kruskal-Wallis test for three independent groups. [2] p-values according to Mann-Whitney test for two independent groups.

In the choice experiment, 33% of the respondents (81 out of 246) chose not to participate in any PES contract. The share of non-participants in the choice experiment was significantly higher in the group that had never applied to the existing contracts (significant at 5% according a Mann-Whitney test). Within that group, almost half of those (57 out of 121 respondents) chose not to participate in any contract in the choice experiment compared with only 19% of the respondents who had applied (24 out of 125).

We use a probit model to analyze whether any observable socioeconomic variables can explain the probability of participation in the PES contract described in the choice experiment, and find that being an applicant to the current PSA program significantly increases the probability of choosing to participate in a contract (results are presented in Table A1a in the Appendix). Similar results have been found in the Chinese Sloping Lands Conservation Program (SLCP), where those who had already participated in the program were more likely to continue participation even if the new contract had different characteristics (Mullan and Kontoleon 2009).

Earlier we showed that on-farm income dependency increases the probability of applying to the current PSA program. We therefore expect on-farm income to have both a direct effect on participation in the PES contracts in the choice experiment and an indirect effect through being an applicant. We decompose the effect of on-farm income dependency by using the *khb* method in Stata (Kohler et al. 2011). The results, presented in Table A1b in the Appendix, show that 57% of the total effect of on-farm income on participation goes through application.

In addition, our results from the probit model show that a stated need for capacity building increases the probability of participating in a PES contract. This variable is later included in the econometric analysis of heterogeneity in preferences for type of payment.

To understand the reasons for choosing not to participate in any contract in the choice experiment, some follow-up questions were asked. The answers reveal that the main reason for non-participation is the fear of losing control over land-use decisions or even losing the property rights of the land. Comparable results have been found for farmers in Kenya, where non-participation in soil conservation programs were motivated by the perception that direct cash payments could weaken the ownership of the process (Porras et al. 2007). Another common reason stated in our study is that payments are too low. This result supports the findings of Arriagada et al. (2009), who found low payments to be one of the main reasons among landowners in the Sarapiquí Region for not enrolling land in the Costa Rican PSA

program. Similarly, in a developed-country setting, Wilson and Hart (2000) found that the two main reasons for not enrolling land in agro-ecosystem schemes in ten European countries were that the offered payments were too low or that such contracts did not fit with farm management plans. In our study, the average stated sufficient payment was almost 776,000 colones (US$1,552)[24] for 10 hectares of land per year. Comparing this number with the 2011 payment levels of US$640 per year for the same area makes such payments seem rather unrealistic. According to these results, the choice of having no contract seems to imply that the landowners are making a choice where staying in the current land-use is the preferred option. Hence, in those cases there does not appear to be a status quo bias occurring when the respondent prefers to stay in the current situation even though another alternative would make him/her better off. This has in fact been found to be quite common in decision making in general (Samuelson and Zeckhauser 1988) and in choice experiments in particular (Adamowicz et al. 1998).

Our results show that several factors influence non-participation in the PES contracts presented in the choice experiment. However, respondents who chose to always opt out made no trade-offs between the attributes. Since no additional information is derived from these respondents, they were excluded from the econometric analysis of the data. Note that this does not mean that the remaining respondents did not opt out, but only that those who never chose to participate are excluded from the tests of the hypotheses concerning the relationship between participation in a PES contract and type of payment.

3.2 Estimations of parameters for choice experiment

In the econometric analysis of the choice experiment, the levels of payment attributes are coded using a dummy variable approach, where the zero-payment levels (i.e., zero net cash payment and zero days of training) are taken as baseline. This allows us to describe utility changes for each of the other levels of the in-kind and cash payments without assuming a particular functional form. This allows us to test the hypotheses regarding the relationship between the type of payment and participation in a PES contract. Duration is treated as a continuous variable.

[24] Fifteen respondents answered this question.

In the first stage in the econometric analysis we test for order effects by using **Model 1** and the three versions [1,2,3,4,5] , [2,3,4,5,1], and [3,4,5,2,1]. If such an effect cannot be rejected, this has to be taken into account in the estimation of the parameters. The log-likelihood values of the separate conditional logit models are -207.56, -111.86, and -170.47, respectively, and the log-likelihood value of the concatenated model is -498.15. The likelihood ratio test leads to $\lambda = -2\ [-498.15 - (-207.56 - 111.86 - 170.47)] = 16.52$. This value is smaller than 26.30, the critical value of the $\chi 2$ distribution at the 5% significance level with (8+8+8)–8=16 degrees of freedom. Hence, the hypothesis of equal parameters could not be rejected. In other words, there is no order effect.

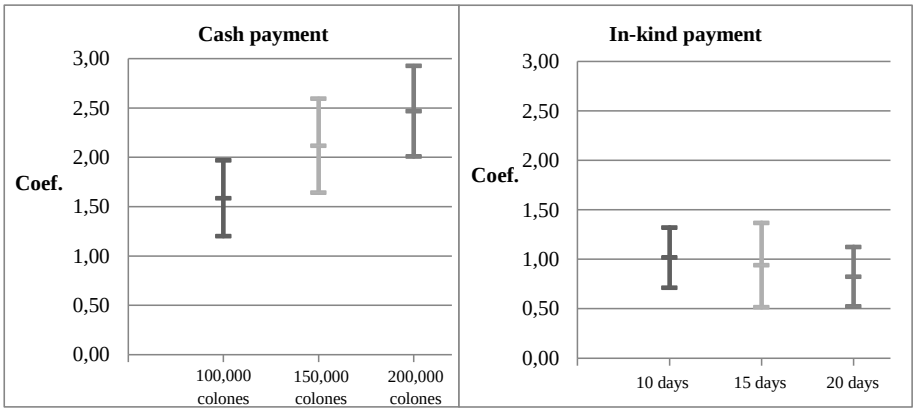
The parameter estimation initiates from **Model 1**, a basic model specified as the probability of selecting a particular PES contract as a function of attributes of the contract and of having a contract or not. The results of the conditional logit estimates of **Model 1** are reported in the first column in Table 3.[25] The insignificant coefficient of the alternative specific constant, i.e., having a contract or not, implies that all else equal the respondents are neither likely nor unlikely to choose a contract instead of no contract. The duration of the contract is significantly negative, showing a tendency of respondents preferring short-term contracts.

Focusing on the parameters of greatest interest for this study, i.e., the types of payments, we find a significantly positive effect on the probability to choose to participate in a contract for both cash and in-kind payments, compared with the reference alternative of zero payment.[26] The coefficients and the 95% confidence intervals for each level of the cash and the in-kind payment from the conditional logit Model 1 are plotted in Figure 3. As can be seen, participation seems uncorrelated with the level of the in-kind payment while increasing cash payments seems to lead to a higher probability of choosing to participate in a contract.

[25] To account for unobservable taste heterogeneity, the random parameter estimates of Model 1 are also estimated. The results are provided in Table A2 in the Appendix. The heterogeneity in preferences in duration seems to be the sole driver of the improvement in estimation in the random parameter model, according to a log-likelihood test. The results regarding the relationship between participation and the type of payment are basically the same compared with the conditional logit model, hence the advantage of using a random parameter model is rather limited.

[26] To get a sense of the magnitude of the effect on participation of a one level (marginal) increase in the cash payment or the payment in days of training the mean marginal effects are estimated. For instance, an increase from 0 to 10 days of training increases the utility of participating by 0.25, while increasing the cash payment from 0 to 100,000 colones increases the same utility by 0.39.

Figure 3. The relationship between level of payment and participation in a PES contract.



Note: This figure illustrates the relationship between level of payment and participation in a PES contract. The coefficients from the conditional logit model (Model 1) are used and the confidence intervals of 95% are shown.

Table 3. Conditional logit estimation results, standard errors in parentheses.

	Model 1	**Model 2**
	Coefficient (Standard error)	Coefficient (Standard error)
Contract, α	-0.24 (0.24)	-0.78 (0.84)
Duration of contract, $\beta_{duration}$	-0.07*** (0.02)	-0.07*** (0.02)
Payment in training, 10 days, β_{10days}	1.02*** (0.16)	0.69* (0.37)
Payment in training, 15 days, β_{15days}	0.94*** (0.22)	-0.09 (0.49)
Payment in training, 20 days, β_{20days}	0.82*** (0.15)	0.21 (0.34)
Payment in cash, 100,000 colones, $\beta_{100cash}$	1.59*** (0.20)	1.57*** (0.27)
Payment in cash, 150,000 colones, $\beta_{150cash}$	2.12*** (0.24)	1.86*** (0.31)
Payment in cash, 200,000 colones, $\beta_{200cash}$	2.47*** (0.23)	2.15*** (0.31)
Guapiles*contract	-	-0.01 (0.34)
San José*contract	-	0.33 (0.28)
Age*contract	-	0.01 (0.01)
Male*contract	-	-0.70** (0.36)
Education*contract	-	0.05 (0.07)
Depending on on-farm income*contract	-	0.31 (0.50)

Total land size[27]*contract	-	0.001 (0.001)
Experienced capacity*contract	-	0.55** (0.27)
Contract at any time*contract	-	-0.42 (0.41)
Stated need of capacity*10 days training	-	0.53 (0.39)
Stated need of capacity*15 days training	-	0.94* (0.48)
Stated need of capacity*20 days training	-	0.69** (0.34)
Depending on on-farm income*10 days training	-	-0.22 (0.32)
Depending on on-farm income*15 days training	-	0.66 (0.45)
Depending on on-farm income*20 days training	-	0.15 (0.28)
Depending on on-farm income*100,000 colones	-	0.24 (0.38)
Depending on on-farm income*150,000 colones	-	0.88* (0.49)
Depending on on-farm income*200,000 colones	-	0.99** (0.49)
Log-likelihood	-662.27	-639.82
Pseudo R-square	0.17	0.19
Number of respondents	165	165
Number of observations	825	825

***, **, and * means significance at the 1%, 5% and 10% levels, respectively.

The following hypotheses were tested using a Wald chi-square test and the β parameters estimated in **Model 1**:
H_0 tested[28];
Cash payments
$\beta_{100cash} < \beta_{150cash}$ not rejected (p-value=0.999)
$\beta_{100\text{cash}} < \beta_{200\text{cash}}$ not rejected (p-value=0.999)
$\beta_{150\text{cash}} < \beta_{200\text{cash}}$ not rejected (p-value=0.994)

In-kind payments
$\beta_{10days} = \beta_{15days}$ not rejected (p-value=0.724)
$\beta_{10\text{days}} = \beta_{20\text{days}}$ not rejected (p-value=0.327)
$\beta_{15\text{days}} = \beta_{20\text{days}}$ not rejected (p-value=0.543)

[27] Since some landowners have more than one piece of land, the total land size is the sum of all land owned by the respondent.

[28] To test for inequality, a chi-square test with 1 degree of freedom can be transformed in a normal standard distribution by obtaining the square root of the Wald statistics and calculating the p-value under normal standard distribution. The p-value from the Wald chi-square test of equal parameters for the cash payments are: $\beta_{150\text{cash}} = \beta_{200\text{cash}}$ rejected (p-value=0.012); $\beta_{100\text{cash}} = \beta_{200\text{cash}}$ rejected (p-value=0.001); $\beta_{100cash} = \beta_{150cash}$ rejected (p-value=0.002).

The hypothesis that the relationship between the magnitude of the payment and participation in a PES contract differs depending on the type of payment is tested using a Wald chi-square test for the estimated parameters in Model 1. The results are presented in Table 3.

Hypothesis 1: Participation in a PES contract is positively correlated with the magnitude of the cash payment, $\beta_{100cash} < \beta_{150cash}$; $\beta_{100cash} < \beta_{200cash}$; $\beta_{150cash} < \beta_{200cash}$.

The null hypothesis of larger coefficients for higher level of cash payment cannot be rejected at a significance level of 5%: the magnitude of the cash payment seems to have a positive effect on participation in a contract.

Hypothesis 2: Participation in a PES contract is uncorrelated with the magnitude of the in-kind payment, i.e., $\beta_{10days} = \beta_{15days}$; $\beta_{10days} = \beta_{20days}$; $\beta_{15days} = \beta_{20days}$.

The null hypothesis of equal coefficients for in-kind payment cannot be rejected at a significance level of 5%: the magnitude of the in-kind payment seems to have no effect on participation in a contract.

According to the results of this study, higher cash payments increase the probability of participation. In-kind payments, on the other hand, seem to imply a rather constant probability of participating in a PES contract. Note that the attribute levels of the type of payments are comparable in terms of the cost for the policymaker. Thus, for a given cost there would be higher participation with cash payments than with educational in-kind payment. However, even if the cost of 10 days of training is comparable to the cost of 100,000 colones in cash payment, the benefit gained of 10 days of training may not be comparable to the benefit of 100,000 colones in cash, e.g., different alternative cost of time. In addition, the benefit from participating is probably not zero. Thus, comparing the costs of providing cash and in-kind payments would be a conservative approach. The objective of this study is to compare levels within the same payment attribute to explore differences in the relationship between participation in PES contracts and type of payment.

Participation in a PES contract is also connected to the commitment of following the requirements during the contract period. Hence, respondents' utility of choosing a contract depends on: i) the payment, which is shown to give positive utility for both cash and in-kind payments; ii) the duration of the contract, which is shown to give disutility, and iii) the intrinsic motivation for having a contract or not, which is shown to be negative, even though

insignificantly so. Using a Wald chi-square test for the estimated parameters in Model 1, we test the null hypothesis of no impact on utility from the different levels of cash and in-kind payments, accounting for the disutility of having a contract (α) and the duration ($\beta_{duration}$). The hypotheses and the p-values are presented in Table A3 in the Appendix.

The null hypothesis of no impact on utility can be rejected at the 5% significance level for all levels of the cash payment in combination with each level of the duration of the contract, i.e., 5, 10, and 15 years, with the exception of a cash net payment of 100,000 colones per year in combination with a contract of 15 years. For the educational in-kind payment, the null hypothesis of no impact on utility is rejected at 10% for short contracts, i.e., 5 years, and practical training of 10 or 15 days. In summary, for shorter contracts both cash and in-kind payments seem to give positive utility and hence are expected to increase participation. However, since there is no significant utility derived from 20 days of practical training, fewer days of practical training seem to be a better choice to increase participation in shorter contracts. For longer contracts, in this case 10 or 15 years, in-kind payment does not seem to have the potential to increase participation. Instead higher cash payments seem to be what is needed to ensure utility from participation in longer contracts.

3.3 Estimations of parameters accounting for observable heterogeneity

To capture heterogeneity in preferences for participating in a contract and for type of payment as a function of individual characteristics, **Model 2** is estimated.[29] In this model, an interaction term between the levels of the in-kind payment and stated need of capacity building is included. Stated need of the in-kind payment is expected to increase the preference for more days of practical training and is hence included to test for this. On-farm income dependency is shown to be an important explanatory variable for participation in the current PSA program, where payments are made in cash. Thus, this variable is of great interest when investigating heterogeneity in preferences for type of payment.[30] All levels of both types of

[29] We also estimated two separate probit models to explore the individual characteristics that may explain the likelihood of being on-farm income dependent or in need of capacity building. The results, shown in Table A5 in the Appendix, show that younger individuals, women, those with higher education, and applicants to the current PSA program are less likely to be dependent on on-farm income, while those who have experienced capacity building seem more likely to be on-farm income dependent. The probability of stating a need for capacity building is significantly increased with experience of capacity building.

[30] We tested interaction terms between the levels of payments and the dummy variable for applicant to the current PSA program and they were insignificant, which suggests that landowners' preferences for the level of cash and in-kind payment did not differ between applicants and non-applicants.

payments are interacted with on-farm income dependency. The results of the conditional logit estimates are reported in the second column in Table 3.

For the heterogeneity in preferences for participating in a contract or not, we find that, in general, men seem less likely to participate in new PES contracts than women. Further, previous experience of capacity-building programs significantly increases the likelihood of participating in a contract. When it comes to the heterogeneity in preferences for the level of the in-kind payment, landowners with a stated need for capacity building prefer both 15 days and 20 days of free practical training, compared with the reference alternative of no practical training; this can be seen from the positive interaction effect between the stated need of capacity building dummy and the dummy for 10 days or 15 days of practical training respectively. Landowners who are on-farm income dependent prefer higher levels of cash payments compared with the reference group of those who are on-farm income independent; this is shown by the significant positive interaction effect between the on-farm income dependency dummy and cash payment levels of 150,000 colones and 200,000 colones.

Using the parameters estimated in **Model 2,** we test our hypotheses by running a test where the parameters of the interaction terms between the payment levels and on-farm income dependency and stated need for capacity building respectively, are taken into account. The results, presented in Table A4 in the Appendix, show that for those who are on-farm income dependent, each increase in cash payment implies a higher probability of participating in a PES contract. For those who are not dependent on on-farm income, there is only a significant difference between a net cash payment of 100,000 colones and 200,000 colones.

For the in-kind payment, the likelihood of participating in a PES contract still seems to be uncorrelated with the number of paid days of practical training. However, those with on-farm income dependency and a stated need for capacity building have a tendency to prefer higher in-kind payments, i.e., more days of practical training, even though it is not statistically significant. Although the relationship between participation in a PES contract and type of payment is in line with what is found in Model 1, Model 2 gives a better understanding of some of the heterogeneity in preferences for the type of payment that seems to be important.

4. Conclusions

This paper uses a choice experiment, conducted with landowners in Costa Rica, to explore the relationship between participation in a payment for ecosystem services (PES) contract and the type of payment used, i.e., cash or in kind. We use an educational in-kind payment in the form of days of practical training offered free of charge to the recipients. The main findings are that 1) there is a positive correlation between participation in a PES contract and the magnitude of the cash payment—higher cash payment seems to give a higher probability of participating in a contract, 2) participation seems uncorrelated with the magnitude of the in-kind payment, 3) for shorter PES contracts (i.e., 5 years), both in-kind and cash payments increase the likelihood of participation, and 4) for longer contracts, higher levels of cash payment are needed to increase the likelihood of participation.

The results of this paper contribute to the literature by providing information regarding the relationship between participation in PES contracts and type of payment used. This knowledge is important in order to efficiently provide ecosystem services. Further, the results of this paper extend the findings in previous studies (e.g., Gneezy and Rustichini 2000, Heyman and Ariely 2004, Pfister 2007, Kube et al. 2012, who found that magnitude seems to matter more for cash payments while less so for in-kind payments) by exploring the heterogeneity in preferences regarding type of payment and participation. We show that our results regarding the relationship between participation and type of payment hold even if we control for heterogeneity due to individuals characteristics. In addition, we find that 1) in-kind payments tend to be more likely to increase participation among those with a stated need for the in-kind payment offered and 2) landowners who depend on on-farm income have stronger preferences for higher levels of cash payments. In line with previous studies, our results indicate that non-participants in the current PSA program in Costa Rica are more dependent on on-farm income (cf. for instance Arriagada 2009). To motivate this group to participate, higher cash payments are needed. However, landowners who are dependent on on-farm income are also more experienced with capacity-building programs, which in turn are shown to make them more likely to state a need for capacity building. Thus, increased participation of on-farm income-dependent landowners may be achieved if free days of practical training are also offered.

Further, since the levels of the cash and in-kind payments are designed to be comparable in terms of cost, for a given cost there would be higher participation with cash payments than with in-kind payments. However, the cost of providing practical training is

calculated at current market prices. In-kind payments in a national PES program could realistically be offered much cheaper than in the market due to the economies of scale that larger provision of goods or services implies. A natural extension of this study would be to run further choice experiments with a larger set of levels of different types of payments and test for different combinations of payments to increase participation at the lowest cost.

Among all the factors influencing participation, two crucial factors are certainly the type of payment used and the magnitude of it. This paper offers some important insights in this regard by showing that increased cash payments seem to increase participation while participation seems unaffected by increased payments in the form of practical training. However, payments in practical training are preferable in shorter contracts and in shorter courses. In contrast, cash payments are preferable when used in larger amounts and in longer contracts. The question of which payment type should be used further depends on the degree of freedom that the individuals ought to be given. Addressing this is beyond the scope of this paper. Nevertheless, our results call for a greater consideration of the preferences of providers of ecosystem services regarding type of payment when designing PES contracts. For policymakers, this type of input can be used to help design the most effective payment systems for ecosystem services when the goal is to maximize participation.

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Appendix

Table A1a. The probit estimation results of the probability of yes or no answer to being applicant to the current PSA program (yes = 1, no = 0) and of the probability of yes or no answer to participate in a PES contract in the choice experiment (yes = 1, no = 0); standard errors in parentheses.

	Dependent variable application to current PSA program	Dependent variable participation in PES in choice experiment
	Coefficient (Standard error)	Coefficient (Standard error)
Age	-0.02** (0.01)	-0.01 (0.01)
Male	0.02 (0.28)	-0.41 (0.30)
Education	0.12** (0.05)	-0.05 (0.05)
Household members	-0.11 (0.06)	0.03 (0.06)
Farm income	-0.75*** (0.20)	-0.15 (0.20)
Average land size	0.01*** (0.001)	-0.001 (0.001)
Stated need for capacity building	0.14 (0.25)	0.44* (0.23)
Applied to the program	-	0.87*** (0.21)
Constant	1.10 (0.74)	0.83 (0.74)
Log-likelihood	-122.27	-136.60
Pseudo R-square	0.28	0.11
Number of observations	244[#]	244[#]

Table A1b. Result from the *khb* method to decompose the direct effect of on-farm income dependency (*Farm income*) on participation in PES contract in the choice experiment and the indirect effect through being an applicant (*Applied to the program*)[31].

Farm	Coefficient (Standard error)	Summary of confounding
Total effect (reduced model)	-0.35* (0.20)	*Conf_ratio*
Direct effect (full model)	-0.15 (0.20)	The total effect is 2.3 times larger than the direct effect
Indirect effect (difference)	-0.20*** (0.07)	*Conf_Percentage*
Pseudo R-square	0.11	57% of the total effect is due to being an applicant.
Number of observations	244[#]	

***, **, and * significant at the 1%, 5%, and 10% levels, respectively.
[#]Two of the respondents did not answer the question concerning need for capacity building.

[31] The following Stata command is used: *khb probit trade farm || applied, concomitant (age gender edu housemem size needcap) summary*.

Table A2. Random parameter logit estimation results; standard errors in parentheses.

Random parameters	Mean coefficient (Standard error)	Coeff std. (Standard error)
Duration of contract, $\beta_{duration}$	-0.09*** (0.02)	0.20*** (0.02)
Payment in training, 10 days, β_{10days}	1.26*** (0.19)	0.32 (0.65)
Payment in training, 15 days, β_{15days}	1.35*** (0.27)	<0.01 (0.70)
Payment in training, 20 days, β_{20days}	1.10*** (0.19)	0.07 (1.01)
Non-random parameters [a]		
Contract, α	0.44 (0.30)	
Payment in cash, 100,000 colones, $\beta_{100cash}$	1.74*** (0.22)	
Payment in cash, 150,000 colones, $\beta_{150cash}$	2.40*** (0.30)	
Payment in cash, 200,000 colones, $\beta_{200cash}$	2.81*** (0.30)	
Log-likelihood	-609.56	
Pseudo R-square	0.33	
Number of respondents	165	
Number of observations	825	

***, **, and * significant at the 1%, 5%, and 10% levels, respectively. [a] The contract parameter and the cash payment parameters were assumed to be fixed in the random logit model. However, we tried letting the cash payment parameters be random and normal/log-normal distributed but no significant heterogeneity was found, nor any differences in signs.

Table A3. The null hypotheses of no impact on utility of different combinations of attribute levels using estimated parameters from Model 1 and p-values from a Wald chi-square test.

Hypotheses	P-value
$U = \alpha + \beta_{duration} * 5 + \beta_{100cash} = 0$	0.01
$U = \alpha + \beta_{duration} * 10 + \beta_{100cash} = 0$	0.01
$U = \alpha + \beta_{duration} * 15 + \beta_{100cash} = 0$	0.07
$U = \alpha + \beta_{duration} * 5 + \beta_{150cash} = 0$	0.01
$U = \alpha + \beta_{duration} * 10 + \beta_{150cash} = 0$	0.01
$U = \alpha + \beta_{duration} * 15 + \beta_{150cash} = 0$	0.01
$U = \alpha + \beta_{duration} * 5 + \beta_{200cash} = 0$	0.01
$U = \alpha + \beta_{duration} * 10 + \beta_{200cash} = 0$	0.01
$U = \alpha + \beta_{duration} * 15 + \beta_{200cash} = 0$	0.01
$U = \alpha + \beta_{duration} * 5 + \beta_{10days} = 0$	0.07
$U = \alpha + \beta_{duration} * 10 + \beta_{10days} = 0$	0.70
$U = \alpha + \beta_{duration} * 15 + \beta_{10days} = 0$	0.39
$U = \alpha + \beta_{duration} * 5 + \beta_{15days} = 0$	0.09
$U = \alpha + \beta_{duration} * 10 + \beta_{15days} = 0$	0.91
$U = \alpha + \beta_{duration} * 15 + \beta_{15days} = 0$	0.17
$U = \alpha + \beta_{duration} * 5 + \beta_{20days} = 0$	0.24
$U = \alpha + \beta_{duration} * 10 + \beta_{20days} = 0$	0.63
$U = \alpha + \beta_{duration} * 15 + \beta_{20days} = 0$	0.04[#]

[#]This is found to be significantly negative.

Table A4. This matrix shows test results of Hypotheses 1 and 2 depending on on-farm income dependency and stated need for capacity building. Parameters from Model 2 used.

	Dependent on on-farm income	**Not dependent on on-farm income**
Stated need for capacity building	***Cash payments*** $\beta_{100cash} + \delta_{\text{Depending on on-farm income*100,000 colones}} <$ $\beta_{150cash} + \delta_{\text{Depending on on-farm income*150,000 colones}}$ **not rejected** (p-value=0.999) [32] $\beta_{100cash} + \delta_{\text{Depending on on-farm income*100,000 colones}} <$ $\beta_{200cash} + \delta_{\text{Depending on on-farm income*200,000 colones}}$ **not rejected** (p-value=0.999) $\beta_{150cash} + \delta_{\text{Depending on on-farm income*150,000 colones}} <$ $\beta_{200cash} + \delta_{\text{Depending on on-farm income*200,000 colones}}$ **not rejected** (p-value=0.973) ***In-kind payments*** $\beta_{10days} + \delta_{\text{Depending on on-farm income*10 days training}} + \delta_{\text{Stated need of capacitation*10 days training}} =$ $\beta_{15days} + \delta_{\text{Depending on on-farm income*15 days training}} + \delta_{\text{Stated need of capacitation*15 days training}}$ **not rejected** (p-value=0.131) $\beta_{10days} + \delta_{\text{Depending on on-farm income*10 days training}} + \delta_{\text{Stated need of capacitation*10 days training}} =$ $\beta_{20days} + \delta_{\text{Depending on on-farm income*20 days training}} + \delta_{\text{Stated need of capacitation*20 days training}}$ **not rejected** (p-value=0.870) $\beta_{15days} + \delta_{\text{Depending on on-farm income*15 days training}} + \delta_{\text{Stated need of capacitation*15 days training}} =$ $\beta_{20days} + \delta_{\text{Depending on on-farm income*20 days training}} + \delta_{\text{Stated need of capacitation*20 days training}}$ **not rejected** (p-value=0.112) *Share of respondents in this group: 43%*	***Cash payments*** There is only a significant difference between the parameters of cash payment level of 100,000 colones and 200,000 colones hence only these parameters are tested for inequality[33]. $\beta_{100cash} < \beta_{200cash}$ **not rejected** (p-value=0.992). ***In-kind payments*** $\beta_{10days} + \delta_{\text{Stated need of capacitation*10 days training}} =$ $\beta_{15days} + \delta_{\text{Stated need of capacitation*15 days training}}$ **not rejected** (p-value=0.247) $\beta_{10days} + \delta_{\text{Stated need of capacitation*10 days training}} =$ $\beta_{20days} + \delta_{\text{Stated need of capacitation*20 days training}}$ **not rejected** (p-value=0.260) $\beta_{15days} + \delta_{\text{Stated need of capacitation*15 days training}} =$ $\beta_{20days} + \delta_{\text{Stated need of capacitation*20 days training}}$ **not rejected** (p-value=0.880) *Share of respondents in this group: 40%*

[32] To test for inequality, a chi-square with 1 degree of freedom can be transformed in a normal standard distribution by obtaining the square root of the Wald statistics and calculating the p-value under normal standard distribution. The p-values from the Wald chi-square test of equal parameters for the cash payments are: $\beta_{100cash} + \delta_{\text{Depending on on-farm income*100,000 colones}} = \beta_{150cash} + \delta_{\text{Depending on on-farm income*150,000 colones}}$ rejected (p-value=0.001);
$\beta_{100cash} + \delta_{\text{Depending on on-farm income*100,000 colones}} = \beta_{200cash} + \delta_{\text{Depending on on-farm income*200,000 colones}}$ rejected (p-value=0.001);
$\beta_{150cash} + \delta_{\text{Depending on on-farm income*150,000 colones}} = \beta_{200cash} + \delta_{\text{Depending on on-farm income*200,000 colones}}$
rejected (p-value=0.054).

[33] The p-values from the Wald chi-square test of equal parameters for the cash payments are: $\beta_{100cash} = \beta_{150cash}$ not rejected (p-value=0.201); $\beta_{100cash} = \beta_{200cash}$ rejected (p-value=0.016); and $\beta_{150cash} = \beta_{200cash}$ not rejected (p-value=0.130).

	Dependent on on-farm income	Not dependent on on-farm income
No stated need for capacity building	**Cash payments** $\beta_{100cash} + \delta_{\text{Depending on on-farm income}*100{,}000 \text{ colones}} <$ $\beta_{150cash} + \delta_{\text{Depending on on-farm income}*150{,}000 \text{ colones}}$ **not rejected** (p-value=0.999) $\beta_{100cash} + \delta_{\text{Depending on on-farm income}*100{,}000 \text{ colones}} <$ $\beta_{200cash} + \delta_{\text{Depending on on-farm income}*200{,}000 \text{ colones}}$ **not rejected** (p-value=0.999) $\beta_{150cash} + \delta_{\text{Depending on on-farm income}*150{,}000 \text{ colones}} <$ $\beta_{200cash} + \delta_{\text{Depending on on-farm income}*200{,}000 \text{ colones}}$ **not rejected** (p-value=0.973) **In-kind payments** $\beta_{10days} + \delta_{\text{Depending on on-farm income}*10 \text{ days training}} =$ $\beta_{15days} + \delta_{\text{Depending on on-farm income}*15 \text{ days training}}$ **not rejected** (p-value=0.861) $\beta_{10days} + \delta_{\text{Depending on on-farm income}*10 \text{ days training}} =$ $\beta_{20days} + \delta_{\text{Depending on on-farm income}*20 \text{ days training}}$ **not rejected** (p-value=0.827) $\beta_{15days} + \delta_{\text{Depending on on-farm income}*15 \text{ days training}} =$ $\beta_{20days} + \delta_{\text{Depending on on-farm income}*20 \text{ days training}}$ **not rejected** (p-value=0.676) *Share of respondents in this group: 7%*	**Cash payments** There is only a significant difference between the parameters of cash payment level of 100,000 colones and 200,000 colones, hence only these parameters are tested for inequality.[34] $\beta_{100cash} < \beta_{200cash}$ **not rejected** (p-value=0.992). **In-kind payments** $\beta_{10days} = \beta_{15days}$ **not rejected** (p-value=0.131) $\beta_{10days} = \beta_{20days}$ **not rejected** (p-value=0.295) $\beta_{15days} = \beta_{20days}$ **not rejected** (p-value=0.539) *Share of respondents in this group: 10%*

[34] The p-values from the Wald chi-square test of equal parameters for the cash payments are: $\beta_{100cash} = \beta_{150cash}$ not rejected (p-value=0.201); $\beta_{100cash} = \beta_{200cash}$ rejected (p-value=0.016); and $\beta_{150cash} = \beta_{200cash}$ not rejected (p-value=0.130).

Table A5. The probit estimation results of the probability of the yes/no answer to dependency on on-farm income (yes = 1, no = 0) and of the probability of the yes/no answer to need for capacity building (yes = 1, no = 0); standard errors in parentheses.

	Dependent variable Dependent on on-farm income	*Dependent variable* Stated need for capacity building
	Coefficient (Standard error)	Coefficient (Standard error)
Age	-0.02*** (0.01)	-0.01 (0.01)
Male	0.67*** (0.27)	-0.60* (0.36)
Education	-0.19*** (0.05)	0.09 (0.06)
Farm income		0.35 (0.23)
Average land size	0.001 (0.001)	-0.001 (0.001)
Experience of capacity building	0.36** (0.18)	0.41** (0.21)
Applied to the program	-0.73*** (0.20)	0.08 (0.23)
Constant	1.40 (0.56)	1.56 (0.71)
Log-likelihood	-140.43	-103.84
Pseudo R-square	0.18	0.07
Number of observations	246	244[#]

***, **, and * significant at the 1%, 5%, and 10% level, respectively.

[#]Two of the respondents did not answer the question about their need for capacity building.

Paper V

Do Entrance Fees Crowd Out Donations for Public Goods?

Evidence from a Protected Area in Costa Rica*

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Abstract

In this paper, we investigate how different levels of entrance fees affect donations for a public good, a natural park. To explore this issue, we conducted a stated preference study focusing on visitors' preferences for donating money to raise funds for a protected area in Costa Rica given different entrance fee levels. The results reveal that there is incomplete crowding-out of donations when establishing an entrance fee.

Keywords: crowding out; entrance fee; donation; voluntary contribution; stated preferences; protected areas; Costa Rica.

JEL Classification: Q26, Q28.

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1. Introduction

All too often, important public goods such as national parks and cultural institutions (e.g., museums and opera houses) find themselves under financial pressure, making the understanding of the efficacy of alternative funding strategies a key component of the management and policy decisions regarding these public goods (e.g., Huck and Rasul 2011, Alpízar and Martinsson 2012). The broad issue of how to increase donations to public goods has caught the attention of economists over the last decades. The issue has mainly been investigated from the perspective of how to combine fundraising with external mechanisms such as providing a small gift in return (e.g., Alpizar et al. 2008; Falk 2007), information about others' donations (e.g., Frey and Meier 2004), and information that donations will be topped up (seed money) (e.g., List and Lucking-Reiley 2002), where most of the results indicate that the aforementioned mechanisms increase donations. An alternative or complementary strategy would be to combine the possibility of donating with a mandatory minimum contribution to the public good by introducing a fee or a tax. The objective of this paper is to investigate the effect of different mandatory contribution levels on donations for a public good. To this end, we conducted a stated preference study to explore visitors' preferences for donating money to raise funds for a protected area given different entrance fee levels.

The early seminal work of Titmuss (1970), who claim that blood donations will be reduced if donors are compensated, indicates that there might be a correlation between voluntary actions and money. Thus, the key issue is whether money per se crowds out voluntary actions. There is by now a well-established literature on the theoretical foundations of pro-social behavior. For example, Bénabou and Tirole (2006) developed a model where behavior is explained by three main motives: extrinsic, intrinsic, and reputational. The essence of this model is that not only amount of money (extrinsic motive) but also intrinsic motivation and how people

are perceived by others affect people's behavior, which may explain why blood donations are expected to decline if donors are monetarily compensated (see, e.g., Mellström and Johannesson 2008), as well as why more people decide to pick-up their children late from daycare when a fee for late pick-ups is introduced (Gneezy and Rustichini 2000) and why people put more physical effort into an exercise that increases their donations when donations are visible to others (Ariely et al. 2009).

The essential problem of public goods is the propensity of subjects to free-ride. However, both introspection and massive empirical literature support that many people voluntarily contribute to public goods and do not free-ride (for findings from public goods experiments, see, e.g., Zelmer 2003). However, the question is how donations are affected if a minimum contribution level, which for example could be motivated as a tax or a fee, is introduced. Is there a complete crowding out of voluntary donations? The public finance literature has investigated this issue from a tax perspective and generally predicts that government spending on public goods financed by lump-sum taxes completely crowds out voluntary contributions, i.e., that a tax results in a dollar-for-dollar reduction in voluntary contributions (e.g., Bergstrom et al. 1986; Andreoni 1988). This prediction has been tested in public goods experiments conducted in a laboratory, with the general finding being that an imposed lump-sum tax results in incomplete crowding-out (e.g., Andreoni 1993; Chan et al. 2002; Gronberg et al. 2012). A fee is, however, conceptually different from a tax (e.g., see findings reported in Kallbekken et al. 2011), and in this paper we are interested in to what degree an entrance fee crowds out donations to a protected area. In case of complete crowding out, there is a dollar-for-dollar reduction in donations when an entrance fee is increased, ruling out any motive besides extrinsic motivation; in case of incomplete crowding-out, on the other hand, intrinsic motivation still matters.

The rest of the paper is organized as follows. Section 2 describes the model and the study design, Section 3 presents the results, and Section 4 summarizes and concludes the paper.

2. Model and design of study

2.1. Model

We apply a Lancastrian approach to model utility of visiting a public good, in this case a national park, and this assumes a multi-attribute utility function (Lancaster 1966). Thus, the utility of a specific state of the public good is a function of the levels of the attributes, which include a monetary attribute, usually a fee, and a vector of non-monetary characteristics of the public good. However, to be able to test whether a mandatory entrance fee crowds out donation, we separate the cost attribute into fee and donation. These two monetary attributes differ in their impact on utility since the fee is only related to the extrinsic motivation, while amount donated is motivated by both extrinsic and intrinsic motives. Thus, if an individual's behavior is guided by intrinsic motives, we expect the utility of donating money to be positive (given that the mandatory entrance fee is not set too high). However, we expect an inverted u-shape relationship between donation and utility relating to the combined effect of extrinsic motivation caused by out of pocket payment for a donation and the intrinsic motivations relating to the good feeling one enjoys when a decision to contribute to a good cause is made, e.g., warm glow (e.g., Andreoni 1990). For entrance fees, we are allowing for a non-linear relationship since we expect that there may be an intrinsic motivation for paying a fee as well. Further, we expect the intrinsic motivation of donating to be negatively correlated with the level of the entrance fee. To capture this, an interaction term between donation and fee is included. Thus, below we have an indirect

utility function that depends on income (Y), donations (D), and entrance fees (F) besides the enjoyment of the attributes of the public good as captured by a vector of attributes ($\widehat{G}$):

$$V = \alpha + \beta_1 D + \beta_2 D^2 + \beta_3 Y + \beta_4 F + \beta_5 F^2 + \beta_6 D * F + \hat{\beta}_7 \widehat{G} \, . \qquad (1)$$

In this formulation of the utility function for a visitor to the protected area, we expect $\beta_1 > 0$ and $\beta_2 < 0$, which capture our hypothesis that utility increases with the initial decision to donate a positive amount but decreases for higher donations. For entrance fees, we expect a marginal disutility of paying an entrance fee given by $\beta_4 < 0$. However, we do allow for the possibility of having a marginal utility of paying an entrance fee that varies with the level of the fee, which is given by β_5. A priori we do not have an expectation regarding the sign of this coefficient.

Finally, the decision to donate will also depend on the current entrance fee levels. The term $\beta_6 < 0$ should reflect our hypothesis that the total utility from making a donation is tempered by the entrance fee levels, so that the total marginal utility from donating is lower if the entrance fees are high, i.e.,

$$\partial V / \partial D = \beta_1 + 2\beta_2 D + \beta_6 F \, . \qquad (2)$$

The final amount donated to the national park is assumed to be a trade-off between the disutility of taking money out of your pocket to pay the entrance fees and donations, and the utility of feeling good donating to a good cause.

2.2. Design of study

Our study is conducted in Cahuita National Park[1] in Costa Rica, which is a park currently without entrance fees and where people enter multiple times[2]. We conduct an on-site study where we randomly sample respondents from all visitors to the park when there is no entrance fee, avoiding potential sample selection bias. Thus, the park constitutes a good case study since there is currently no entrance fee and it relies solely on donations. Cahuita National Park is located along the shoreline of the Caribbean cost of Costa Rica and attracts two main types of visitors: beach-goers and nature lovers, and to some extent people who combined these two interests. Since Cahuita National Park attracts heterogeneous groups of visitors in terms of both country of origin and reasons for visiting the park, we are able to also investigate how heterogeneity affects the donations given different entrance fee levels.

Our research approach is based on surveying park visitors. The survey consisted of several parts. First there was a battery of socioeconomic questions and questions related to the park visit. This was followed by a choice experiment exploring visitors' donation preferences given different entrance fee levels. Here we also included non-monetary attributes related to proposed improvements in the park, as planned by the park authority.

In the choice experiment, each respondent made four repeated choices between two different alternatives describing what the park could be like next time they visited. Given that we included the status quo levels as part of the description of the alternatives, and that visitors had

[1] Cahuita National Park is one of the most frequently visited parks in Costa Rica with around 50,000 visitors per year from all over the world. The main entrance to the park is located on the doorstep of the town of Cahuita and receives around 95% of all park visitors (a second entrance receives only 5% of the visitors and charged an entrance fee of 6USD when the study was conducted; visitors entering through this remote entrance are not included in our survey). Visitors to the park are kindly invited to make a voluntary donation upon arrival in addition to compulsory registration.

[2] An average visitor enters the park three times.

already chosen to visit the park under the status quo levels, an opt out alternative was not included. Before the respondents were asked to make their choices in the experiment, the enumerator read a scenario to them (see the scenario in the Appendix). The survey and attributes were developed in cooperation with the community and the park authority and were then refined through several pilot studies. After careful selection of the attributes and levels, we used a D-optimal design allowing for interaction effects to create forty choice sets. The forty choice sets were blocked into ten groups, which were then randomly allocated to the respondents. Thus, each respondent faced four choice sets, which were shown in random order to reduce potential order effects. The survey was given to international visitors who had just entered the park, either walking along the park's only trail or on the beach, from Tuesday to Saturday.[3] We interviewed people shortly after entering the park to avoid them gaining more experience from the park. The enumerators[4] were carefully instructed to select participants without following a discernible pattern, and we regularly controlled the representativeness of the sample by comparing the sample (and enumerators' subsamples) with the population as registered in the park's guest book, in which all visitors entering the park had to register (results shown in Table 2). Moreover, the field supervisors were present in the park at all times and the quality of the field work was controlled through daily debriefings and frequent monitoring.

The alternatives were described by four attributes: use of the revenues from recreation in Cahuita National Park, information signs available, entrance fee, and donation. During the pre-studies, we found that residents and international visitors have very different preferences for

[3] We excluded Sundays since a large fraction of visitors on this day are local residents of the town of Cahuita. Mondays had a low visiting rate so they were used for preparation of materials and data coding.

[4] In order to ensure the quality of the field work, we implemented a highly ambitious training and supervision programme (see excellent advice on this topic in Whittington 2002). Enumerators went through a thorough two-week training programme in which they were instructed and guided on how to conduct interviews. The training also included explaining the choice experiment method and the importance of their role as enumerators in the research process.

attributes in protected areas, which has also been found in previous studies (e.g., Chase et al. 1998, Hearne and Salinas 2002). Thus, we focus on international visitors.[5]

At present, funds are used for basic park maintenance and also for small community projects ranging from environmental education to solid waste management. However, the park authority would also like to use some funds to improve the infrastructure, in particular by constructing elevated trails, picnic huts, and tables. Thus, the first non-monetary attribute in the choice experiment is use of funds in addition to park maintenance and community projects, where we included the following attribute levels: environmental education for the population of Cahuita, improved picnic huts available for visitors, and the construction of elevated trails to access the forest including wildlife observation towers. As a second non-monetary attribute we included the provision of information at the park, which was consistently mentioned in the pilot studies as being an important aspect of ensuring park enjoyment. Currently, information is only available at the entrance, yet one could also envision information being made available along the trails and even in a leaflet. For this attribute we use the following levels: (i) information at the entrance on a large poster (map) describing the park's facilities; (ii) information at the entrance on a large poster (map) describing the park's facilities and information about wildlife along the trail; and (iii) information at the entrance on a large poster (map) describing the park's facilities, and a free leaflet about wildlife.

For the monetary attributes, focus groups and discussions with the park authority made it clear that any attempt to charge an entrance fee of more than 6 USD would not be credible unless all the other parks in the area also increased their fees. Thus, we used entrance fee levels ranging

[5] Local residents of Cahuita were initially excluded since they by law would be exempt from paying an entrance fee. Further, the policy discussions at the time were rather focused on introducing an entrance fee for international visitors only.

from 0 to 6 USD per day. We also introduced donation as a monetary attribute ranging from 0 to 6 USD. Table 1 summarizes the attributes and their levels, and Figure 1 shows an example of a choice set.

Table 1. Attributes and attribute levels of the choice experiment.

Attributes	Levels
Financing maintenance and community projects…	… and environmental education for the population of Cahuita (level 1, current use of funds)
	…. and improved picnic huts and tables for visitors (level 2)
	….and the construction of elevated trails to access the forest, including wildlife observation towers (level 3)
Information signs available…	…by the entrance on a large poster (map) describing the park's facilities (level 1, current state of information available)
	…by the entrance on a large poster (map) describing the park's facilities, and information signs about wildlife along the trail (level 2)
	…by the entrance on a large poster (map) describing the park's facilities, and a free leaflet about wildlife (level 3)
Entrance fee	0,1,2,3,4, and 6 USD
Donation	0,1,2,3,4, and 6 USD

To analyze the data from the choice experiment, we use a standard random utility approach where individuals' choices depend on the observed variables in the choice experiment and the unobserved variables that are captured in an additive error term. It is assumed that the respondents consider the two alternatives offered in every choice situation and then choose the alternative that would give them the highest utility next time visiting to the park. An individual will then prefer the generic choice alternative $\{1\}$ over choice alternative $\{2\}$ if $V^{\{1\}} - V^{\{2\}} > 0$. Hence, our econometric model is based on the following equation:

$$V^{\{1\}} - V^{\{2\}} = \left[\beta_1 \Delta D + \beta_2 \Delta D^2 + \beta_4 \Delta F + \beta_5 \Delta F^2 + \beta_6 \Delta (D * F) + \hat{\beta}_7 \Delta \hat{G}\right] > 0 \qquad (3)$$

Figure 1. Example of a choice set.

Characteristics	*Alternative 1*	*Alternative 2*
Funds will be used for the overall maintenance of the park, community projects…	… and improved picnic huts and tables for visitors	...and the construction of elevated trails to allow access into the forest and towers to observe wildlife
Information signs will be available…	...by the entrance; there will be a large sign board with a map describing the park's facilities	...by the entrance; there will be a large sign board with a map describing the park's facilities, as well as information signs about wildlife along the trail
Entrance fee of…	3 USD	1 USD
Your donation is…	1 USD	2 USD

Which alternative would you prefer?

☐ *Alternative 1* ☐ *Alternative 2*

We apply a random parameter logit model in Limdep in our estimations of the taste parameters to allow for heterogeneity in preferences among respondents, and we account for the fact that the same individual makes four repeated choices (McFadden and Train 2000). We use a normal distribution for both the monetary and non-monetary attributes.[6] To estimate the parameters, we rely on simulation method since the unconditional probability that an individual chooses any given alternative in any given choice set is given by the integrals of the standard logit probabilities over all possible values of β (for details see Train 2003). The model is estimated using 500 Halton draws. The application of the random parameter logit model also allows us to retrieve the individual parameters of each respondent by using Bayes Theorem (e.g., Train 2003). In this paper the focus is to investigate the effect of different mandatory entrance fees on donations for a public good, in this case a national park. Thus, by using the individuals' parameters, we can calculate the maximum donation for each individual, *q*, in the sample, given different entrance fees, as[7]

$$D_q^{max} = -\left(\frac{\beta_{1,q}+\beta_{6,q}*F}{2\beta_{2,q}}\right) . \qquad (4)$$

[6] This does allow for both positive and negative signs, which is reasonable for the attributes applied. For the money attributes some people might not mind donating money, and some people might not mind paying an entrance fee as they are presumably paying for a good cause. Substantial lab and field experimental evidence supports the idea that economic agents do like to sacrifice their own financial gains for the provision of public goods. This has been found in the context of voluntary donations to protected areas (see for example Alpizar et al. 2008).

[7] We can calculate this ratio since the scale parameter is canceled.

3. Results

We interviewed 769 adults from a total population of 5,182 international visitors to Cahuita National Park who visited the park during the study period December 2007 to March 2008 with a break during the holiday season. The descriptive statistics of our selected sample and of the international visitors who were not part of the choice experiment (information was obtained from the compulsory registration book at the park entrance) are shown in Table 2. By and large, our sample is representative of the population as registered in the park's guest book, which all visitors entering the park had to register in. Although the majority of the interviewed visitors are Europeans and, generally, highly educated, there are visitors from all over the world. 77% of the international park visitors made a donation, similar to what was stated by the interviewed visitors. The revealed average donation from the registration for the sample who took part in our survey is 2.01 USD per person, which is slightly higher than the average donation of 1.61 USD per person of those visitors who did not take part in our survey.

Table 2. Descriptive statistics for our sample and from the registration book for the sample of international visitors who did not take part in the choice experiment.

	International visitors		
	Information from registration book	*Information from our survey*	*p-value*[i]
Observations/Respondents	4, 413 obs.	769 resp.	
Country			
USA/Canada	35%	33.5%	0.58
Latin America/Caribbean	7%	6.5%	0.60
Europe	56%	58%	0.48
Others	2%	2%	0.71
Male	47.5%	46.5%	0.55
Gave donation when entering the park	77%	77%	0.99
Average donation per person	1.61 USD	2.01 USD[ii]	<0.01
Average conditional donation per person	2.16 USD	2.54 USD[ii]	<0.01
Average age	-	38 years	
Education			
University (with or without degree)	-	78.5%	
Main reason for visiting Cahuita National Park			
Only beach	-	28%	
Only nature	-	40%	
Mix of both	-	32%	

[i] *The null hypothesis of equal means in the samples.*

[ii] *The donation as registered at the entrance for those individuals who took part in the choice experiment and that we managed to match with the registration book according to time of entry and personal characteristics, i.e., 469 respondents were matched.*

In our econometric analyses of the choice experiment, we code the levels of the non-monetary attributes using a dummy variable approach. The monetary attributes, i.e., donation and entrance fee, are continuous variables where the non-linearity is captured by including the square of those attributes as shown in equation (3). Our results from the random parameter logit model, where the data is treated as panel data taking into account that people are doing a series of choices, are presented in Table 3. For the non-monetary attributes, visitors generally prefer provision of information along the trails in the park compared to the current information provided only at the park entrance. Yet the current financing of environmental education projects in the community is preferred compared to financing facilities in the park such as picnic huts and elevated trails.

The total effect of donation on utility depends on three variables in the utility function: donation, squared donation, and donation interacted with entrance fee. The overall relationship can be described as an inverted u-shaped relationship between donation and utility since donation is positive and squared donation is negative. This also holds for entrance fee, but the intrinsic motivation, e.g., "warm glow" effect, of paying a fee is weaker.[8]

[8] These results are robust even when we allow for correlation between the random parameters.

Table 3. Random parameter logit estimations (p-values in parentheses)

	Mean Coefficient	Coeff. Std.
Non-Monetary attributes		
Baseline: Environmental Education		
Dummy: Financing picnic huts	- 1.898 (<0.01)	1.939 (<0.01)
Dummy: Financing elevated trails	- 0.543 (<0.01)	1.339 (<0.01)
Baseline: Information at Entrance		
Dummy: Information at entrance and along trail	0.321 (<0.01)	0.510 (<0.01)
Dummy: Information at entrance and free leaflet	0.070 (0.17)	0.360 (<0.01)
Monetary attributes		
Donation	0.210 (<0.01)	0.184 (<0.01)
Donation^2	- 0.027 (<0.01)	0.018 (<0.01)
Entrance fee	0.148 (<0.01)	0.200 (<0.01)
Entrance fee^2	- 0.030 (<0.01)	0.006 (0.04)
Donation*Entrance fee	- 0.038 (<0.01)	0.069 (<0.01)
Number of observations	3076	
Number of respondents	769	
Log-likelihood function	-1680	

Table 3 also reports the coefficients of the standard deviations from the mean coefficients, which indicate the degree of unobserved heterogeneity. The coefficients for the standard deviation of the parameters for the non-monetary attributes (new infrastructure and additional information) are both highly significant. Thus, although on average visitors significantly object to investments in new infrastructure in the park, there is considerable heterogeneity in their preferences, even up to the point that there is a considerable probability of

sign reversal. The same is true for the information attribute. The standard deviations of the coefficients of variables relating to entrance fees and donations are highly significant, by and large indicating that visitors have very heterogeneous preferences for entrance fees and donations. These results support our choice of the random parameter logit model.

To investigate whether entrance fees crowd out donations, we calculate individual maximum donations at different entrance fee levels using the individual parameters retrieved from the random parameter logit model. Table 4 shows the predicted average maximum donation for entrance fees in the range of 0 to 6 USD, calculated as follows:

$$D_q^{max} = \max\left[\left(\frac{-\beta_{1,q}+\beta_{3,q}\beta_{5,q}F}{2\beta_{2,q}}\right), 0\right] \quad , \qquad (5)$$

which rules out negative donations, which per definition are not possible. Further, since entrance fees lower than 50 cents are rarely observed in reality, these were treated as zero donations. In the presentation, we separate proportion predicted to donate zero, average total donation, and average conditional amount donated (i.e., amount donated given a positive donation). We find an increase in the predicted share of zero donation when the entrance fee increases. The proportion of positive donation is significantly lower for all entrance fees compared with no entrance fee, except for an entrance fee of 1 USD. In line with other stated preference studies,[9] we find a discrepancy between stated and revealed average donation. This can be seen when comparing the revealed average donation in Table 2 of around 2 USD with the predicted average donation in Table 4 of almost 4 USD when the entrance fee is set to zero. Since we expect this discrepancy to be constant between the donation levels, our estimations of the trade-off between donation and

[9] In the literature, the findings regarding hypothetical bias in choice experiments are mixed. Johansson-Stenman and Svedsäter (2012), for instance, found a significantly higher hypothetical marginal willingness to pay (MWTP) for an environmental good (donations to WWF) compared with a real-money MWTP, while Carlsson and Martinsson (2001) found no significant difference between real and hypothetical MWTP.

entrance fee are still credible. The predicted average conditional donation decreases as the entrance fee increases, and the conditional amount donated is significantly lower for all entrance fees compared with no entrance fee. Thus, the predicted average donation also decreases significantly from 3.98 USD when there is no entrance fee to 0.75 USD when the entrance fee is 6 USD. This shows an incomplete[10] crowding-out of donations in the sense that an increase in the entrance fee of 1 USD on average decreases donations by approximately 0.54 USD, as shown in Table 4. This means that the marginal reduction in donations is less than the marginal increase in the entrance fee. Basically what happens is that the proportion of individuals giving zero increases substantially, while the average reduction in predicted average conditional donations decreases by only 0.45 USD when the entrance fee is increased from 3 USD to 6 USD.

Figure 2 gives a detailed description of the distribution of donations with no entrance fee and with an entrance fee of 6 USD. As can be seen, the distribution of predicted maximum donations shifts to much lower values and the proportion predicted to not give any donations climbs from zero with no entrance fee to almost 60% with an entrance fee of 6 USD. An overall test shows a significant difference, at the 1% significant level, in the distribution of donations between the case of no entrance fee and one with an entrance fee of 6 USD.

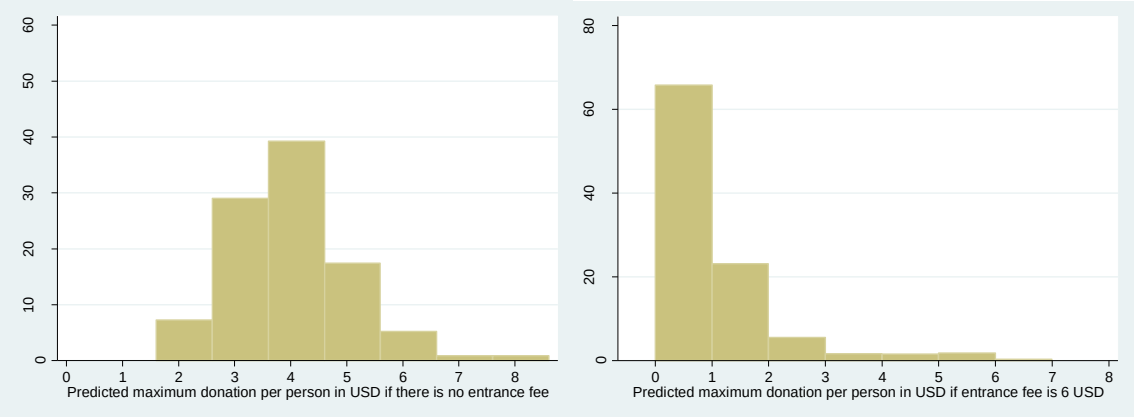
[10] A complete crowding out would decrease donations by 1 USD on average for every 1 USD increase in the entrance fee.

Table 4. Predicted share of zero donations, average donation, and conditional average donation per person in USD given the level of entrance fee using individual parameters. P-values are presented testing the null hypothesis of equal outcomes with and without an entrance fee, at each studied entrance fee level.

	Predicted share of zero donation	p-values[i]	Average predicted donations	p-values[ii]	Average predicted conditional donations	p-values[iii]
No entrance fee	0%		3.98 USD		3.98 USD	
Entrance fee of 1 USD	0%	0.99	3.29 USD	<0.01	3.29 USD	<0.01
Entrance fee of 2 USD	3.5%	<0.01	2.60 USD	<0.01	2.69 USD	<0.01
Entrance fee of 3 USD	16%	<0.01	1.94 USD	<0.01	2.31 USD	<0.01
Entrance fee of 4 USD	34%	<0.01	1.39 USD	<0.01	2.11 USD	<0.01
Entrance fee of 5 USD	54%	<0.01	1.00 USD	<0.01	2.16 USD	<0.01
Entrance fee of 6 USD	59.5%	<0.01	0.75 USD	<0.01	1.86 USD	<0.01

[i] P-value from a chi-squared test, testing H_0: share of zero donations are equal between no entrance fee and the given level of entrance fee.
[ii] P-value from a t-test, testing H_0: average donations are equal between no entrance fee and the given level of entrance fee.
[iii] P-value from a t-test, testing H_0: average conditional donations are equal between no entrance fee and the given level of entrance fee.

Figure 2. The left graph shows the distribution of predicted maximum donation per person in USD with no entrance fee. The right graph shows the distribution of predicted maximum donation per person in USD with an entrance fee of 6 USD. The distributions are significantly different between the two entrance fee levels (chi-square p-value<0.01).



To check the robustness of our results, more detailed analyses are made by splitting the sample into sub-samples based on reason for visiting the park, country of origin, and gender, using the individual parameters retrieved from the random parameter logit model estimated above. Incomplete crowding-out of donations from introducing an entrance fee is found in all sub-samples at the 1% significance level (data available upon request). However, beach-goers are predicted to give significantly lower average donations at all entrance fee levels compared with those visiting the park to enjoy nature. Further, we tested for cultural differences in preferences regarding how to finance public goods as visitors from countries with a culture of financing public goods through voluntary donations, i.e., visitors from the USA and Canada, are expected to experience less crowding-out of donations by an introduction of an entrance fee compared with visitors from countries outside these countries, of which a majority have a tradition of financing public goods with taxes rather than visitation fees. However, we find no

significant differences between these two groups of countries. Nor do we find any significant differences between men and women.

4. Conclusions

In this paper we have investigated crowding out of donations to a public good, i.e., a protected area, when a mandatory fee is introduced. We find that there is incomplete crowding out, indicating that other motives beyond extrinsic motives matter. These findings are robust in more detailed within sub-population analyses. The main contribution of our results is that there seems to be incomplete crowding out when a compulsory fee is introduced. Nevertheless, when considering overall funding, it is important for policymakers to have a good understanding of the price elasticity of the mandatory entrance fee, and based on that be able to determine an optimal fee level.

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APPENDIX

Scenario

Thank you. Let's continue with the next part. In order to make sure that we can provide you with the best experience in Cahuita, I will ask you to do four evaluations. Here is an example of one such evaluation.

(GIVE THE RESPONDENT THE EXAMPLE CARD. PLEASE POINT AT THE EXAMPLE WHEN YOU DESCRIBE THE FOLLOWING.)

- *Each card will have two different alternatives.*
- *Each alternative describes how Cahuita National Park could look the next time you visit.*
- *For each card, your task is to choose the alternative that you prefer: either alternative one or alternative two.*
- *The alternatives are described by four different characteristics. (*POINT AT THE EXAMPLE CARD)
- *The characteristics and the different levels are explained on this card.* (TURN THE EXAMPLE CARD OVER)
- *On each card you will always find these four characteristics, but you will only find one of these levels in an alternative* (POINT AT THE LEVELS). *So, only the levels will change.*
- *The alternatives will not differ in any other aspect than those shown on the card*
- *Please read them carefully.*(GIVE THEM TIME TO READ)

Do you have any questions?

Let´s go back to the example card. As you can see here (POINT AT THE EXAMPLE):

- ***Alternative one** will fund improved picnic huts and tables for visitors while **Alternative two** will fund the construction of elevated trails to allow access into the forest and towers to observe wildlife.*

- *In **Alternative one** there will be a large sign board with a map by the entrance describing the park's facilities, while in **Alternative two** there will also be information signs about wildlife along the trail.*

- *In **Alternative one** the entrance fee is 3USD and your donation is assumed to be 1 USD while **Alternative two** has an entrance fee of 1 USD and your donation is assumed to be 2 USD.*

Imagine that each alternative describes how Cahuita national park could look the next time you visit. Please look at each alternative and tell me which one you prefer. Take your time!

(MARK THE ALTERNATIVE THAT THE RESPONDENT PREFERRED ON THE EXAMPLE CARD)

ALTERNATIVE 1	ALTERNATIVE 2

Please turn back to the explanation of characteristics and let's continue.

(SHOW ONE CHOICE-SET CARD AT A TIME)

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