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Article *in* Environmental and Resource Economics · November 2015

DOI: 10.1007/s10640-015-9981-2

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Environ Resource Econ
DOI 10.1007/s10640-015-9981-2

Unintended Effects of Targeting an Environmental Rebate

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Accepted: 25 October 2015

Abstract When designing schemes such as conditional cash transfers or payments for ecosystem services, the choice of whom to select and whom to exclude is critical. We incentivize and measure actual contributions to an environmental public good to ascertain whether *being excluded from a rebate* can affect contributions and, if so, whether *the rationale for exclusion* influences such effects. Treatments, i.e., three rules that determine who is selected and excluded, are randomly assigned. Two of the rules base exclusion on subjects' initial contributions. The third is based upon location and the rationales are always explained. The rule that targets the rebate to low initial contributors, who have more potential to raise contributions, is the only rule that raised contributions by those selected. Yet by design, that same rule excludes the subjects who contributed the most initially. They respond by reducing their contributions even though their income and prices are unchanged.

Keywords Behavioral economics · Field experiment · Forest conservation · Public goods · Selective rebates

Electronic supplementary material The online version of this article (doi:10.1007/s10640-015-9981-2) contains supplementary material, which is available to authorized users.

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1 Introduction

Growing concern about environmental public goods such as carbon sequestration, watershed function, and species habitat has led researchers, communities, governments, and international organizations to look for ways to enhance those public goods or, at the least, reduce their rates of deterioration (see, e.g., Klemick 2011; Benthem and Kerr 2013). Among the policy options, payments aimed at raising the voluntary provision of such public goods have increased over the last decade and, alongside this trend in popularity, so have suggestions—theoretical and practical—for how to target such subsidies (Spencer et al. 2009; Pattanayak et al. 2010; Ferraro 2011). In this paper, we present a field experiment on contributions to an environmental public good that investigates whether *being excluded from receiving a targeted rebate* can shift such contributions.

Debates about conservation policy frequently touch on the issue of to whom a given subsidy should be offered. Not surprisingly, there are advocates for each of a number of selection criteria, i.e., different selection rationales. A common top-down approach is to select lands with the highest ecoservice benefits per unit of standing forest (Wu and Babcock 1996; Smith and Shogren 2002), e.g., land with highly valued species or land near rivers upstream of cities. This selection approach is not at all related to landowner behavior. Thus, it can select lands irrespective of the level of risk for their being converted to alternative uses that eliminate desired vegetation. Consequently, it is quite possible that funds are spent on encouraging protection when no encouragement is needed.

A common bottom-up approach is to accept the first volunteers, i.e., "first-come, first served". Yet landowners who do not plan to deforest—due to low agricultural returns or a love of nature—are likely to be more eager to enroll, given their lower opportunity costs. As a result, this selection rule favors the enrollment of landowners with lower clearing risks (Stoneham et al. 2003; Goeschl and Lin 2004; Ferraro 2008) and a subsidy could effectively reward people for conserving nature even if that was in fact the way that they preferred to use their land. Thus, such a selection rule is likely to yield a lower impact upon deforestation (Arriagada et al. 2012; Robalino and Pfaff 2013).

Given those insights, a common recommendation for increasing the impacts on deforestation of conservation subsidies has been to target threat, i.e., to make an effort to enroll lands which face some significant threat of forest loss (Pfaff and Sanchez-Azofeifa 2004; Muñoz-Piña et al. 2008; Wünscher et al. 2008). By design, such a focus on additionality—i.e., lower forest loss relative to what would have occurred in the absence of the subsidy program—aims to exclude those likely to conserve their forests for private reasons. This makes sense if private forest conservation is driven by low agricultural returns, but if a significant driver of private conservation is a pro-social desire to contribute to the public good, then such exclusion may backfire. Excluded individuals may feel they are being punished for having acted pro-socially without any monetary incentive, when they should have been rewarded, and thus react to exclusion from an incentive program by no longer maintaining forest. The following excerpts from a discussion in *RESECON*[1] illustrate this concern:

> "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?"
> "I have to admit that my issue with this design feature [targeting for additionality] 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

[1] RESECON, Land and Resource Economics Network, is an environmental economics listserve (resecon.org).

> someone who has sacrificed his own gain to make his operation more environmentally benign is just taken for granted. If payments are one-off incentives to make permanent changes to practices, I have somewhat less of a problem, but a long-term flow of payments to reward the changes is a slap to those who have made these changes on their own initiative".[2]

These quotes highlight a tradeoff between efficiency and equity considerations in the design of conservation programs: it may be inefficient to pay for public goods that would be provided even without the payment program; yet equity or fairness opinions exist in support of such payments. Consequently, plans to raise efficiency by excluding those privately providing public goods could fail if individuals stop conserving because they perceive their exclusion to be inequitable or unfair.

We examine whether such exclusion affects contributions to public goods—emphasizing that, in our study, any such result arises despite a lack of price and income shifts or strategic behavior (ruled out by our design). We also examine whether the reason for a subject's exclusion—which was communicated to them—affects reactions to being excluded. We do so in a field experiment to ascertain the effect of targeting a rebate for actual contributions to environmental public goods. Specifically, we use a well-known forest-conservation program as the recipient of contributions, with landowners as subjects since they actually do provide public goods from forest conservation.

Subjects are randomly assigned to one of three treatments, rules for selection and exclusion. Two rules base exclusion on a subject's initial contributions (again, all rationales are explained to them) while the third rule bases exclusion only upon the location of the subject's land (we explain that conservation provides more societal benefits in some locations, which are thereby prioritized). We are focused on spillovers, i.e., *whether those who are not treated by a rebate are still affected*. Spillovers matter for policy design, yet our aim is not to pass final judgment on optimal selection as we, e.g., consciously eliminate the possibility of behavioral changes facing a long-term policy.

We emphasize that our focus is on the effects of being excluded from a new targeted rebate. Thus, we diverge from two related literatures (see Sect. 2): (1) studies on how selected individuals respond when treated by a new policy (e.g. a subsidy, a tax or a rebate), and (2) studies in which a new policy is introduced, then removed. Concerning (1), behavioral economics considers, e.g., "motivation crowding" or shifts in motivations and decisions when new financial incentives are introduced. Concerning (2), literature in psychology considers whether introducing and then removing new incentives seems to reduce whatever original motivation drove decisions. Neither of these describes situations we consider, in which, despite unchanged prices and incomes, participants may react to *not receiving* a new rebate.[3] We note that the income of those selected to receive the environmental rebate increases, though the income of the excluded subjects does not.

We implemented our experiment as part of a survey on land use and agricultural practices in Costa Rica. Participants were paid for their time twice during the survey, at the midpoint and end. They were told at the start to expect a payment in appreciation of their time, but did not know its amount or its timing. Just after each of the two payments, participants were asked if they would like to contribute to forest conservation by making a monetary contribution to

[2] RESECON 2013/01/24 New Forest Trends' Ecosystem Marketplace report *State of Watershed Payments 2012*.

[3] Studies suggest effects of changed prices on forest conservation. Incentives to conserve forest can lower local labor demand or crop supply and thus shift wages and prices (see, e.g., Wu et al. 2001; Robertson and Wunder 2005).

Bosque Vivo, a government program that conserves forest ecosystems in Costa Rica using private donations.

For their initial contributions after the first payment, all participants faced the same decision, i.e., how much to donate from the received amount. For the second round, we randomly assigned each participant either to the control treatment or to one of following three selection rules:[4]

- Rule 1 aims to increase the contributions and thus the rebate is offered to those who made lower contributions in Round 1, indicating a potential for increases in contributions;
- Rule 2 at least effectively functions as a reward for those who gave more without incentive and hence the rebate is offered to those who made higher contributions in Round 1;
- Rule 3 offers the rebate to those whose land is located in the socially prioritized location for contributions, i.e., the Nicoya Peninsula, which is one of the driest areas of Costa Rica.

In our control, no rebate is introduced. Subjects are simply asked to contribute again in Round 2, just as they did in Round 1. When it is used, the rebate refunds 50 % of the Round 2 contribution. The full Round 2 contributions still go to Bosque Vivo, since the rebates were paid to participants using funds from the research project.[5] For each of these selection rules, given that both prices and income are unchanged for participants excluded from the rebate, we may hypothesize no change in those subjects' contributions over time relative to the behavior over time observed in the control.

To isolate the effects of selection rules from, for instance, the effects of just time or repetition, we employ a difference-in-difference (DiD) approach. For each rule, we compare the change in behavior across rounds with the change in behavior in the control group (exactly the same script was followed in the control and treatment groups). Importantly, before they made their second decisions to donate, subjects were told that the second payment was to be the last one. This was done to avoid strategic behavior, for example, a participant adjusting her contributions in order to be eligible for future rebates (again, our experiment is not designed to examine dynamic behavior).

We found significant effects upon contributions only under Rule 1, i.e., the selection rule that targets the rebate towards the participants who made the lower initial contributions in Round 1. Although participants selected under this rule raised their contributions over time, in contrast those *excluded* from the rebate under this same selection rule reduced their contributions over time. Given no shifts in prices or income, such a reduction in contributions by those excluded because they made high initial contributions suggests there was a negative shift in their motivations. This group may feel that because they made high pro-social contributions, their exclusion was unfair. However one interprets it, the reduction in contributions by those excluded highlights a tradeoff. Consequently, it suggests an important design challenge for conservation subsidy programs.

The remainder of the paper is organized as follows. Section 2 describes previous literatures concerning selective incentives in environmental programs and concerning shifts in

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

[5] We tried to have the subjects link the rebate directly to their decision, i.e., not see this as seed money or as matching funds (as discussed in, e.g., List and Lucking-Reiley 2002). Thus, the refunded money was handed to the subjects (unlike what is done within a matching-donation setup). This may bring us closer to the operational feel or framing of conservation payment programs (see Eckel and Grossman 2003 on the importance of framing).

motivations. Section 3 describes the design of our field experiment, while Sect. 4 presents all our results. Finally, Sect. 5 summarizes the paper and discusses some implications of our results.

2 Relevant Literature

2.1 Selective Incentives for Environmental Public Goods

Conditional payments—for education or health outcomes or reducing deforestation and forest degradation—have attracted the attention of both practitioners and academics in the past few decades. By conditioning on a socially desired behavior, payments are intended to increase social welfare (Persson and Alpizar 2013; Pattanayak et al. 2010; Ferraro 2011; Rawlings and Rubio 2005). Conditional payments typically define an eligible population (say, forest owners) and then define selection rules that delimit who will ultimately receive the payment. Participation is voluntary.

In the environmental arena alone, there are over 200 payment for ecosystem services (PES) schemes worldwide (Landell-Mills and Porras 2002) and numbers are expected to rise as REDD+ programs are expected to rely heavily on performance based payments.[6] Costa Rica and Mexico have two of the best known national PES programs. In each, the state directly pays landowners conditional on the standing forest remaining as such, a proxy for the provision of environmental public goods. For each, concerns have been voiced about whether (all else being equal, including cost) selection should give priority to forest land facing higher risk of deforestation, i.e. on additionality (Pfaff et al. 2007; Robalino and Pfaff 2013). Economists typically point out that such targeting can increase impact. Of course, any number of approaches could be used to guide enrollments; practitioners frequently mention concerns for equity in the choice of who is targeted for payment.

2.2 Shifting Motivations

In psychology, many researchers have examined the behavioral effects of a payment, to encourage a given action that is first introduced and then removed (see Deci et al. 1999 for a comprehensive review). Related to this, though not requiring the removal of the incentive, "motivation crowding" in behavioral economics has been identified as an issue in a range of settings. Motivation crowding involves a change in how an individual frames a specific decision, which could then shift the decision itself. It can result from introducing a policy, such as a public subsidy or tax, for something that had previously been a private matter. A widely cited example (Gneezy and Rustichini 2000) is a fall in on-time pickup rates from daycare when a fine for being late is established [an environmental analog in Colombia is provided by Cardenas et al. (2000)]. Eckel et al. (2005) observed nearly full crowding out of contributions to charity when subjects were told that their initial allocation had been taxed. Additionally, contributions to a public good are found to change when incentives are introduced (see theory by Frey 1994; Deci et al. 1999; Frey and Jegen 2001; and a survey of the empirical literature by Bowles and Hwang 2008).

[6] REDD+ stands for Reduced Emissions from Deforestation and Forest Degradation and the plus adds conservation to the mix. This is a key strategy to reduce emissions resulting from land use change and deforestation. For more information, see: https://unfccc.int/land_use_and_climate_change/redd/items/7377.php.

None of the above describes our situation, in which people react to *being excluded from a rebate*. A strand of literature on the desire for social approval, however, could provide a basis for an impact on behavior due to being excluded from the rebate (for economic models with social approval see, e.g., Akerlof 1980; Hollander 1990; Bénabou and Tirole 2006; Ellingsen and Johannesson 2008; Andreoni and Bernheim 2009). If private contributions to environmental public goods, e.g., donations or actions in support of forest conservation, are driven partly by a desire to be perceived by others as environmentally friendly, then conservation payments could spoil the clarity of contributions as a signal of one's environmental type (Ariely et al. 2009). While those selected to receive the payment could be compensated for the lost signaling value by their increased income, those not selected might well lower their contributions to the public good.

2.3 Fairness and Motivations

Behavioral and experimental economics also provides potential explanations for individual reactions to being *excluded* from the rebate. One well-established empirical result is that people are nice to those who have treated them fairly but, in stark contrast, wish to punish those who have not, even when punishing implies a cost to themselves (Rabin 1993). This result fits well with the documentation of reciprocity (for theoretical work see, e.g., Falk and Fischbacher 2006). Fairness preferences are also suggested by (relatively) equal divisions of resources in games and costly punishments imposed following unequal divisions (Fehr and Schmidt 2006; Dawes et al. 2007).

In our case, in which subjects have already made private contributions to forest conservation before the selection decision, those excluded from the rebate might feel unfairly treated, in particular if they were excluded due to behaviors that they felt were publicly minded. If so, then how might such offended agents punish unfairness? If an unfair rule concerns forest conservation, the agents affected by it might feel they can punish by reducing their forest-conservation efforts.

Finally, although prices and income are unaltered for those subjects not receiving the rebate, those receiving the rebate enjoy a slightly higher income. This change in the income distribution, small as it might be in our case, might in principle also explain any possible negative reaction of excluded landowners (see e.g. Fehr and Schmidt 1999 for a fairness model capturing this feature).

3 Field Experimental Design

Our experiment starts as a dictator game. One player, the dictator, is given a sum of money to allocate between himself and another player, the receiver (e.g., Kahneman et al. 1986; Forsythe et al. 1994; Hoffman et al. 1996). In our case, the receiver is a charity organization, following Eckel and Grossman (2003) and Carpenter et al. (2008). Dictators are landowners, who were randomly drawn from a carefully constructed database of landowners in Costa Rica.[7] Appointments were made by phone before visiting the respondents' homes individually. Most of those who were contacted wanted to participate, but due to funding and complex logistics, we were able to interview about 50 % of all those contacted after the initial random

[7] Two databases were used for the sampling frame: (1) all 2011 applications received before May 2011 by the National Forestry Financing Fund (FONAFIFO), the authority responsible for payments for ecosystem services program in Costa Rica and (2) a farm census from 2006 and 2007 by the Ministry of Livestock and Agriculture (MAG).

draw, and we could not identify any specific pattern (e.g. by region or proximity to cities) in successful versus unsuccessful interviews.[8] In total, we interviewed 357 landowners in four of Costa Rica's nine regions—Limon, Guapiles, San José, and Nicoya—from November 2011 to January 2012.

Face-to-face surveys are common in Costa Rica. Our subjects consented to participate in what appeared to them to be simply one more standard survey (in our case concerning land use). Thus, our participants did not know that our survey had an experimental or research component.[9]

The survey 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 and the subjects were informed about and received a payment for participation. They were also invited to consider a voluntary, anonymous contribution to forest conservation through a public program called Bosque Vivo, which conserves forest ecosystems in Costa Rica using private contributions. The purpose and nature of the program were described in the survey. Subjects were given private time to decide how much to contribute using the money they had just received. The subjects marked the amount contributed on an envelope with only the survey number as identifier, then put the contribution in the envelope and sealed it.[10] Subjects could only contribute from the payment that they had just received, i.e., 5000 colones (10 USD).

The survey recommenced and ended approximately half an hour later, when a second payment was announced, and then a second invitation to contribute was issued. Subjects were not aware that a second payment was to come when they made their first contribution, but they were made fully aware that the second payment was the last (we did this to avoid any strategic behavior, e.g., adjusting contributions to increase the chances of being selected for future rebates). Each of the two payments equaled 5000 colones, paid in notes and coins to allow for flexibility in multiples of 500 colones (1 USD). Each of the payments was given to the subjects in a sealed envelope. As a reference, the average pay for a day of farm work in Costa Rica is 10,000 colones (20 USD).

Our first round (Round 1) of contributions provided baseline pro-environmental behavior in the absence of any rebate. Our second round (Round 2) introduced the targeted rebate to encourage contributions. The rebate consisted of a refund of 50 % of the contribution made in Round 2. For example, if a treated landowner decided to contribute 1000 colones to Bosque Vivo in Round 2, this amount was put in a sealed envelope addressed to Bosque Vivo and a new payment of 500 colones was made to her (paid for using project funds). Hence, the full amounts of all of the participants' voluntary contributions went to Bosque Vivo. A limited budget was used as justification for having to select some landowners and exclude others from the rebate.[11]

8 Due to lack of socio-economic data on the general population of landowners in Costa Rica, it is not possible to formally prove that our sample is representative of the country. Still, given the carefully constructed databases and protocols used to contact landowners, we are confident our sample is as representative as feasible.

9 List (2008) discusses informed consent in the social sciences. We did not ask for consent to be in an experiment, as we wanted to observe behavior in a setting that was as realistic as possible. The experiment certainly met all ethical norms: anonymity, no additional risk, and while there were no direct research benefits, direct payments were given and ideally societal benefit from increased knowledge occurs from the result of this research.

10 The enumerator observed the contributions, thus an "experimental demand effect" might affect contributions (e.g., Hoffman et al. 1996; Zizzo 2010). However, the difference-in-difference approach addresses this issue. Moreover, our enumerators were carefully trained not to hint what constituted appropriate behavior.

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

To control for the effect of repeated contributions, which alone could influence Round 2 contributions, one set of randomly chosen landowners faced Round 2 without any rebate at all and, hence, no mention of selection rules. We refer to this group as the "control" treatment. Besides the absence of the rebate, scripts and procedures were identical for treated and control subjects.

In Round 2, treated landowners were randomly assigned to one of three selection rules. For two of the rules, whether a landowner qualified for a rebate depended on her behavior in Round 1. Under Rule 1, a landowner received the rebate if she contributed 1000 colones or less in Round 1. In focusing on low givers, we expected that such a rebate would have real potential to encourage selected subjects to give more. Yet excluded subjects are in principle unaffected, which suggests a hypothesis of no significant change in their behavior, compared to the control. If a behavioral reaction results from being excluded from the rebate policy, we expect contributions to decrease, compared to the control. The cut-off of 1000 colones was chosen because average giving in a standard dictator game is around 20 % of the endowment (Camerer 2003) and thus less than 20 %—here 1000 of 5000 colones—is relatively low. Under Rule 2, a landowner received the rebate if he contributed 2500 colones or more in Round 1. Thus, under Rule 2, the rebate functions more as a reward for having contributed more than 50 % of their endowment. The rebate is still intended to increase the contributions of those who are selected and, again, we hypothesize that the subjects *excluded* from the rebate will not be significantly affected, compared to the control.

With regards to both Rules 1 and 2, in which one's prior contributions affect current eligibility for the rebate, it is important to add that subjects were informed that Round 2 was to be the last one. Thus, there was no point in varying one's Round 2 contributions strategically (for example, under Rule 1, reducing one's contributions in Round 2 in order to qualify for the rebate in future rounds).

We want to compare such selection rules, based on past contributions, with selection based on characteristics outside of subjects' control. Thus, our Rule 3 selects for the rebate those whose land is located in the priority location, the Nicoya Peninsula. We explain in the script that Bosque Vivo was prioritizing the area due to its water quality problems during dry seasons, which are well-known throughout the country. This rule mimics the approach of selecting for the rebate land with the highest ecosystem-service benefits. In practice, since this criterion applies irrespective of how small or large the landowner's contribution was in Round 1, we expect that contributions of subjects receiving the rebate would increase slightly. The excluded individuals, however, are unaffected; further, here their exclusion is not based on their behavior, so we hypothesize no change in contributions, compared to the control. In Table 1, we summarize our experimental design.

Finally, each subject was interviewed individually and was informed only about the selection and exclusion rule for receiving the rebate which applied to her/him at the time of the interview. He learned only about that rule, as well as whether he qualified or not, but was given no further information.

4 Results

In Table 2, we present descriptive statistics concerning our subjects' characteristics—in total, as a single group, and divided up by treatment. Recalling that treatments were distributed randomly, as expected we do not find statistically significant differences in the characteristics across treatment groups, with the exception of education. The farmers who received the rebate

Table 1 Summary of experimental design and hypothesis

Control group	Treatment group: a rebate of 50 % of the contributed amount is refunded to the landowner					
Round 1 identical for all groups						
Round 2						
Control	**Rule 1:** encouraging low contributors		**Rule 2:** rewarding high contributors		**Rule 3:** selecting if local contribution in prioritized area	
No rebate: Round 2 identical to Round 1	Selected if…	Excluded if…	Selected if…	Excluded if…	Selected if…	Excluded if…
	Donation ≤₡1000 in Round 1.	Donation >₡1000 in Round 1.	Donation ≥₡2500 in Round 1.	Donation <₡2500 in Round 1.	If property is in Nicoya.	If property is not in Nicoya.

Hypothesis for subjects excluded from the rebate: no change in contribution between Round 2 and Round 1, when compared to the control
Random selection into treatments and control

Table 2 Socio-economic characteristics of subjects, total sample and split samples by control and selection rules

Variables	Description	Total	Control	*Rule 1:* encouraging low contributors	*Rule 2:* rewarding high contributors	*Rule 3:* selecting if local contribution in prioritized area	p value[a]
Male	Dummy variable, = 1 if male, = 0 if female	88 %	84 %	91 %	91 %	87 %	0.38
Age	Continuous variable in years	57.5 years	60 years	56.5 years	57.5 years	56 years	0.16
Education	Dummy variable, = 1 if completed secondary school or higher, = 0 otherwise.	37 %	32 %	31 %	35 %	49 %	0.04
Household size	Continuous variable in amount of household members	3.5 persons	3.3 persons	3.8 persons	3.4 persons	3.7 persons	0.28
Income[b]	Continuous variable of monthly total income in colones	₡877,280	₡742,022	₡1,126,624	₡761,636	₡913,033	0.61
Owned land	Continuous variable of total owned land in hectares	106 ha	112 ha	99 ha	118 ha	93 ha	0.86
Share of forest	Continuous variable of share of total land in forest	30 %	30 %	30 %	26 %	35 %	0.42
PES contract	Dummy variable, = 1 if having or have had a PES contract, = 0 otherwise	37 %	41 %	30 %	35 %	41 %	0.37
Limón	Share of respondents owning land in the province of Limón	23 %	20 %	23 %	28.5 %	19 %	0.40
Guapiles	Share of respondents owning land in the province of Guapiles	18 %	20 %	20 %	20 %	14 %	0.62
Nicoya	Share of respondents owning land in the province of Nicoya	30 %	34 %	29 %	27.5 %	30 %	0.77
San Jose	Share of respondents owning land in the province of San José	29 %	26 %	28 %	24 %	37 %	0.24
Number of obs.[c]		357	87	90	91	89	

[a] The null hypothesis is equal distributions/means between the treatment samples using a Chi-square test/one-way ANOVA-test
[b] Income from farm is calculated as net income
[c] There is some variation in number of observations due to missing values in some variables

in Nicoya are more likely to have completed secondary school or higher. This difference is not expected to affect our results, however, because all of the landowners will be compared with similar subjects from the control group. Landowners in Nicoya who face any given treatment will be compared to a subset of the control participants that is composed of landowners in Nicoya, as described below.

To identify the effect of exclusion upon the changes in contributions across rounds, we use a difference-in-differences (DiD) approach (following Ashenfelter and Card 1985). The change in behavior between Rounds 1 and 2 under each of the three selection rules that define who qualifies for the rebate is subtracted from the change in the control treatment, and the significance of this DiD is tested using a t-test. The DiD should solve three key concerns: i. multiple contributions, i.e. subjects might contribute less in the second round because they had already contributed once when we invited them to make a second contribution; ii. income effects, i.e., subjects might contribute more in the second round because they had already been paid once when they are invited to contribute the second time; and iii. experimenter demand effects. Concerning the latter, although we carefully trained all our enumerators not to show any sign of approval or disapproval when contributions in Round 1 and Round 2 were registered, the contribution in Round 1 might be interpreted by both the landowner and the enumerator as "cues about what constitutes appropriate behavior" (Zizzo 2010:75) and, hence, Round 2 might not be fully independent from Round 1.

We start by studying the behaviors of subjects who were excluded from the rebate then do the same for subjects who qualified for the rebate. As noted earlier, given that the excluded subjects face no change in either prices or income, we may hypothesize that there will be no significant difference between the change in contributions between Rounds 1 and 2 for subjects excluded from the rebate under any of the three rules and the same change for subjects in the control. However, any fairness-based reactions to exclusion would be expected to lower contributions.

Yet to correctly compare changes between rounds in the control versus in any selection rule, one further analytical refinement is needed because subjects who contribute what we call "a little" (i.e., 20 % or less of endowment: $\leq$1000 colones) and subjects who contributed what we call "a lot" (i.e., over 50 % of endowment: $\geq$2500 colones) in Round 1 may be different "giving types." If so, then to isolate effects of our treatments, we would want to compare behaviors from the same "giving type" in comparing those selected for, versus those excluded from an environmental rebate. The bottom half of Tables 3 and 5 show contributions to Bosque Vivo for landowners in the control treatment, with samples split following the cut-off points of our rules for selecting who qualifies for the rebate or who is excluded. They describe the control groups we will actually use. Put another way, given large observed differences in the Round 1 contributions within the control treatment, we will design the DiD indicator with these *ad hoc* control groups to compare the same "giving type" for those excluded from the rebate versus those in the control. The same analytical refinement will also be used when analyzing the behavior of subjects selected for the rebate.

4.1 Effects of Exclusion

The top half of Table 3 shows average contributions by round for those excluded from the rebate under each rule, in the first three columns. A fourth column extends the analysis of Rule 3 by constructing a subsample of excluded high contributors who were exposed to Rule 3 (in the subsample if Contribution in Round 1 >1000 colones). This last column will be used to explore the behavior of excluded high givers, given that their exclusion was not based on their past behavior.

Table 3 Average contributions in colones (₡) by subjects EXCLUDED from receiving the rebate

Treatment	**Rule 1:** encouraging low contributors excluded if donation > ₡1000	**Rule 2:** rewarding high contributors excluded if donation < ₡2500	**Rule 3:** excluded if property is not in Nicoya	**Rule 3 and high contributors:** excluded if property is not in Nicoya with filter for donations > ₡1000
Round 1	₡4070	₡1000	₡3339	₡4234
	(71 obs)	(37 obs)	(62 obs)	(47 obs)
Round 2	₡2824	₡1081	₡2468	₡2862
	(71 obs)	(37 obs)	(62 obs)	(47 obs)
	(79 % excluded)	(41 % excluded)	(70 % excluded)	(76 %excluded)
Difference = R2 − R1	−₡1246***	+₡81	−₡871***	−₡1372***
Ad hoc control	Donation in the control treatment > ₡1000	Donation in the control treatment < ₡2500	Property in the control treatment not in Nicoya	Property in the control treatment not in Nicoya with filter for donations > ₡1000
Round 1	₡4040	₡932	₡3228	₡4366
	(62 obs)	(37 obs)	(57 obs)	(41 obs)
Round 2	₡3250	₡1108	₡2474	₡3220
	(62 obs)	(37 obs)	(57 obs)	(41 obs)
Difference = R2 − R1	−₡790***	+₡176	−₡754***	−₡1146***
DiD indicator	−₡456*	−₡95	−₡117	−₡226

Columns include information for each of our treatment/selection rules in top half. Bottom half shows ad hoc control[a] to account for "giving type"

*** Significant at 1 %, ** Significant at 5 %, * Significant at 10 %, according to a Wilcoxon test for the within-subject comparisons (H_0: Round 2 = Round 1), and a one-tailed t-test for the between-subject comparisons (H_0: Excluded—Similar Controls ≥ 0) to test the hypothesis that there is a negative effect of being excluded from the incentive

[a] Split samples that follow the cut-off points of our rules excluding those not qualified for the rebate.

Contributions by those excluded fell significantly for all selection rules except Rule 2, which rewards high contributors and hence excludes those contributing less than 50 % of the endowment. We should highlight that the average contributions in Round 1 by excluded subjects under Rule 2 are only 1000 colones, including because about 25 % of the participants donated nothing at all in Round 1. This means that these subjects have less room to further decrease their contributions. That must be taken into account when discussing effects of exclusion under the reward treatment: even if the subjects excluded from the rebate do feel upset about being excluded, the fact that they gave little initially limits effects of exclusion. For the design of targeted rebates, the consequences of a negative backlash due to exclusion might be expected to differ between low and high givers.

Differences in giving types are also very notable, to the extent that we adjusted our analyses: by our design, those excluded by Rule 1 and by Rule 2 are particular subsets (over 1000 or under 2500 colones). To address this issue, for comparisons we use *ad hoc* controls that meet the same restrictions. The bottom half of Table 3 shows the behavior of subjects in each of the *ad hoc* control groups. We again see decreases between Rounds 1 and 2 for each of the ad hoc groups, with the exception of the ad hoc group for Rule 2. The same logic discussed above applies to this case. To start with, we compare contributions in Round 1 in treated versus ad hoc control, and find no significant differences (two-tailed t-test; Rule 1: p value $= 0.8925$; Rule 2: p value $= 0.7160$; Rule 3: p value $= 0.7659$; Rule 3b: p value $= 0.8925$). In other words, the Round 1 behavior of subjects in the ad hoc controls is no different from the behavior of subjects in each of the selection rules.

The last row of Table 3 provides a DiD indicator. A negative sign indicates that contributions under a given selection rule decrease more than under the control, thereby providing a simple first estimate of the effects of exclusion.[12] That effect is significant only for the rule that excludes high contributors from the rebate. Those excluded due to high Round 1 contributions lower the average contributions by 456 colones, compared to the control group (one-tailed t-test p value $= 0.07$).

Being excluded under Rule 2, which excludes low givers from Round 1, does not have such an effect (one-tailed t-test p value $= 0.30$). As noted, the already small contributions in Round 1 for this group implies there is little room for further decreases. Moreover, unfortunately the subsample of subjects exposed to this rule is the smallest of all, raising concerns about our statistical power to claim insignificant results. Leaving magnitude aside, though, the share of subjects who reacted negatively to exclusion is significantly smaller for this "reward" rule than under the additionality (Rule 1) and environmental benefit (Rule 3) rules (chi2 t-test p value < 0.01 for reward rule compared with additionality, and p value $= 0.03$ for reward compared with environmental benefit).

Being excluded if the landowner's property is not in Nicoya does not result in significant differences between treatment and control, with the drop in contributions being fairly close to that for the controls (one-tailed t-test $p$value $= 0.38$). Note that Rule 3 excludes both subjects who give little and subjects who give a lot in Round 1. Column 4 of Table 3 tightens this comparison a bit further by considering a subset of those in the third column, i.e., those excluded based on region who also gave over 1000 in Round 1. This group is arguably a bit more similar in terms of "type" to those excluded under the additionality rule. We find that subjects who give a lot in Round 1, but are then excluded on grounds not related to their past behavior, as in Rule 3, do not reduce their contributions significantly (one-tailed t-test p value $= 0.30$). This suggests that the reason for a greater effect under Rule 1 is

[12] Between-subject 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., the Mann-Whitney test) and find similar results to those discussed here.

the rationale provided for having excluded the higher contributors, i.e., under Rule 1 high contributors were excluded precisely because they had contributed a large amount, while under Rule 3 they were excluded based on a factor exogenous to their behavior.

Table 4 provides a regression analysis that explores these issues a bit further. We run two regressions for each of the selection rules, including two for Rule 3 using the subsample of high givers. In each of the first regressions, we control for Round 1 contributions linearly, while in the other regressions we use dummies for contribution levels in Round 1 to allow for non-linearities. For each rule, we pooled the subsamples of the excluded subjects and their *ad hoc* controls.

The regression analysis confirms the previous findings. We find a significant effect of exclusion only under the rule that excludes those who contributed a large amount to start with and, thus, might most feel unfairly treated by the exclusion. Looking at the coefficients in the first row of Table 4, we find that they almost replicate the DiD indicators in the bottom of Table 3.

4.2 Effects of Selection

The top half of Table 5 shows average contributions by round for those selected for the rebate under each rule in the first three columns. Again, the fourth column restricts analysis of Rule 3 to a subsample more comparable to Rule 1, i.e., subjects who gave 1000 colones or less in Round 1.

The bottom half of Table 5 shows the behavior of those in each of the *ad hoc* control groups. We start by comparing the behavior in Round 1 for the treated versus for the control and we find no significant differences between the two (two-tailed t-tests for Rule 1: p value $= 0.283$; Rule 2: p value $= 0.378$; Rule 3: p value $= 0.683$; Rule 3 if initial contribution >1000: p value $= 0.859$).

For Rule 1, i.e., the selection of those who contributed little in Round 1, contributions tend to increase from Round 1 to Round 2 in both the treatment and the control groups. The opposite is true for Rule 2, which selects those individuals for the rebate who already gave a lot in Round 1: in that case, the contributions actually decrease in both the treated group and the control group. This highlights the value of an appropriate difference-in-difference approach to analyzing the data.

For Rule 3, i.e., the one in which the selection for a rebate was based purely upon the region, there was little change overall if selected for the rebate. However, considering the subsample that donates little in Round 1, we find a very similar behavior to that observed for Rule 1.

The last row of Table 5 shows the DiD with a positive sign throughout, i.e., selection for the rebate increases contributions between Rounds 1 and 2, compared to the control. The effect is significant for Rule 1 which targeted those giving little to start. The contributions are 735 colones higher, when compared to the control (one-tailed t-test p value $= 0.04$). This is not true for Rule 2. Those selected for having contributed a lot continue to give a lot in Round 2, with the rebate, yet contributions do not increase significantly compared to the control (one-tailed t-test p value $= 0.28$). A similar result is found for Rule 3, i.e., for selection of all those in Nicoya (one-tailed t-test 0.30).

Table 5's fourth column examines the subsample of subjects under Rule 3 who would have been selected under Rule 1, i.e., gave under 1000 colones in Round 1. We find a significant rise from receiving the rebate, as expected given that these subjects contributed little in Round 1. The rebate manages to increase their contribution by 750 colones, i.e., very close to the

Table 4 Regression results showing effects of exclusion from targeted rebate

Dep. Variable = change in contributions between Rounds 1 and 2	**Rule 1:** encouraging low contributors, i.e. excluded if donation>₡1000		**Rule 2:** rewarding high contributors, i.e. excluded if donation<₡2500		**Rule 3:** excluded if property is not in Nicoya		**Rule 3 and high contributors:** excluded if property is not in Nicoya with filter for donations >₡1000	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Exclusion	−443*	−446*	−91	−65	−67	41	−280	−221
	(0.067)	(0.068)	(0.312)	(0.365)	(0.420)	(0.90)	(0.253)	(0.310)
Contribution in Round 1								
Linear	−0.45***	–	−0.06	–	−0.44***	–	−0.41**	–
	(0.000)		(0.634)		(0.000)		(0.020)	
0	–	Selected	–	Omitted	–	Omitted	–	Filtered out
500 and 1000	–	Selected	–	−239	–	−1060	–	Filtered out
				(0.293)		(0.114)		
1500 and 2000	–	Omitted	–	−94	–	−1417**	–	Omitted
				(0.687)		(0.036)		
2500 and 3000	–	−690	–	Selected	–	−1447*	–	−67
		(0.182)				(0.068)		(0.939)
3500 and 4000	–	−1091	–	Selected	–	−926	–	323
		(0.161)				(0.500)		(0.832)
4500 and 5000	–	−1441***	–	Selected	–	−2526***	–	−1138* (0.055)
		(0.000)				(0.000)		
Constant	1025**	240	228	277	680*	226	655	−323
	(0.048)	(0.518)	(0.185)	(0.128)	(0.061)	(0.497)	(0.427)	(0.590)
# obs	133	133	74	74	119	119	88	88
R^2	0.1190	0.1198	0.0069	0.0197	0.1998	0.1772	0.0647	0.0245

Omitted dummy excluded from regression; *selected* not in regression due to selection rule; Filtered = not in regression due to subsample
*** Significant at 1 %; ** significant at 5 %; * significant at 10 %

Table 5 Average contributions in colones (₡) of subjects SELECTED to receive the rebate

Treatment	**Rule 1:** encouraging low contributors selected if donation≤ ₡1000	**Rule 2:** rewarding high contributors selected if donation≥₡2500	**Rule 3:** selected if property is in Nicoya	**Rule 3 and high contributors:** selected if property is in Nicoya with filter for donations ≤ ₡1000
Round 1	₡605	₡4380	₡2778	₡625
	(19 obs)	(54 obs)	(27 obs)	(8 obs)
Round 2	₡1500	₡3546	₡ 2852	₡ 1375
	(19 obs)	(54 obs)	(27 obs)	(8 obs)
	(21 % selected)	(59 % selected)	(30 % selected)	(30 % selected)
Difference = R2 − R1	+₡ 895*	−₡ 833***	+₡ 74	+₡ 750*
Ad hoc controls	Donation in the control treatment ≤₡ 1000	Donation in the control treatment ≥ ₡ 2500	Property in the control treatment in Nicoya	Property in the control treatment in Nicoya with filter for donations ≤ ₡ 1000
Round 1	₡ 440	₡ 4540	₡ 2583	₡ 667
	(25 obs)	(50 obs)	(30 obs)	(9 obs)
Round 2	₡ 600	₡ 3510	₡ 2517	₡ 667
	(25 obs)	(50 obs)	(30 obs)	(9 obs)
Difference = R2 − R1	+₡ 160	−₡ 1030***	−₡ 66	₡ 0
DiD indicator	+₡ **735****	+₡ **197**	+₡ **140**	+₡ **750****

Columns include information for each of our selection rules in top half. Bottom half shows ad hoc controls[a] to account for "giving type"

*** Significant at 1 %, ** significant at 5 %, * significant at 10 %, according to a Wilcoxon test for the within-subject comparisons (H_0: Round 2 = Round 1) and a one-tailed t-test for the between-subject comparisons (H_0: Selected—Similar Controls ≤ 0) to test the hypothesis that there is a positive effect of being selected for the incentive

[a] Split samples that follow the cut-off points of our rules selecting those who qualified for the rebate

achievement of Rule 1, though the very limited number of observations in this subsample limits our analysis here.

Table 6 provides a regression analysis for the three selection rules used to target the rebate. Again we run two regressions for each of the selection rules, controlling for Round 1 contributions linearly or allowing non-linearity. Again, we see that being selected for the rebate increases contributions only in Rule 1 which encouraged those who give little to increase their contributions. No significant increase in the contributions results from either of the other two selection rules.

4.3 Net Effects of Selection (Exclusion) Given Low Past Contributions

Significant changes—a fall in contributions if excluded and a rise in contributions if selected—arose when selection for the targeted rebate was based on low past contribution, i.e., under Rule 1. Thus, we consider net selection effects only for Rule 1. Figure 1 shows that its two effects can cancel each other out. The upper graph shows that if one person were selected and one excluded from our targeted rebate, the net effect would be a statistically insignificant increase of 279 colones in the contributions to forest conservation (two-tailed t-test p value $= 0.546$). This is just one way to confirm what is hopefully already clear, i.e., that the potential negative spillovers from exclusion are worth tracking. They can be of a magnitude that could influence overall net impacts.

From a policy perspective, the overall performance of any selection rule in a conditional payment program depends not only upon the magnitude of the effect achieved by the payment, but also on the actual disbursements associated with the payment, i.e. with its costs. Thus, the bottom graph in Fig. 1 conveys the same results but with cost subtracted from the average gain per selected individual. The net effect then becomes an also statistically insignificant loss of 89 colones (two-tailed t-test $p$value $= 0.832$). In sum, our results suggest rebates that select those with low private contributions can indeed achieve additionality from the selected group, yet such positive effects can be neutralized if the corresponding implied exclusion discourages those who would have contributed significantly had there been no payment program at all. Obviously, the severity of this issue may depend upon the share of subjects rejected versus selected for payment. As an example, in the case of Costa Rica's Payment for Environmental Services program about 50 % of the applicants are rejected, so the situation may be similar to the one described in Fig. 1.

5 Conclusion

We explored whether being excluded from a targeted environmental rebate can reduce one's contributions to a public good and, if so, whether the reason for exclusion influences those effects. We used a field experiment applied to landowners making actual contributions to Bosque Vivo. This is a well-known program to support Costa Rica's forest cover, though our result is relevant for incentives to support socially desired behaviors (like sending children to school, or even governments encouraging guerrilla members to abandon their weapons, as done in Colombia).

The literature on forest payments is vast but it really focuses upon those selected for payment: do those selected reflect program objectives? face changed prices? reduce deforestation or shift it? The assumption has been that those excluded from a payment will not change behavior, given no change in their incomes or prices faced. Our experiment tests

Table 6 Regression results showing effects of selection for rebate under alternative selection rules

Dep. variable = change in contributions	**Rule 1:** encouraging low contributors, i.e. selected if donation≤ ₡1000		**Rule 2:** rewarding high contributors, i.e. selected if donation≥₡2500		**Rule 3:** selected if property is in Nicoya	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Selection	910**	943***	172	200	170	126
	(0.011)	(0.009)	(0.302)	(0.275)	(0.275)	(0.344)
Initial contribution						
Linear	−1.06**	–	−0.16	–	−0.15 **	–
	(0.008)		(0.385)		(0.043)	
0	–	Omitted	–	Excluded	–	Omitted
500 and 1000	–	−1087***	–	Excluded	–	−929*
		(0.006)				(0.074)
1500 and 2000	–	Excluded	–	Excluded	–	−793
						(0.124)
2500 and 3000	–	Excluded	–	Omitted	–	−1450***
						(0.009)
3500 and 4000	–	Excluded	–	200 (0.705)	–	−1949**
						(0.019)
4500 and 5000	–	Excluded	–	−261 (0.519)	–	−1170**
						(0.020)
Constant	625**	638**	−319	−861**	326	245
	(0.042)	(0.036)	(0.708)	(0.036)	(0.215)	(0.259)
# obs	44	44	104	104	57	57
R^2	0.2203	0.2305	0.0110	0.0125	0.0780	0.1223

Omitted dummy excluded from the regression, *Excluded* not in regression due to selection rule, *Filtered* not in regression due to subsample
*** Significant at 1 %; ** significant at 5 %; * significant at 10 %

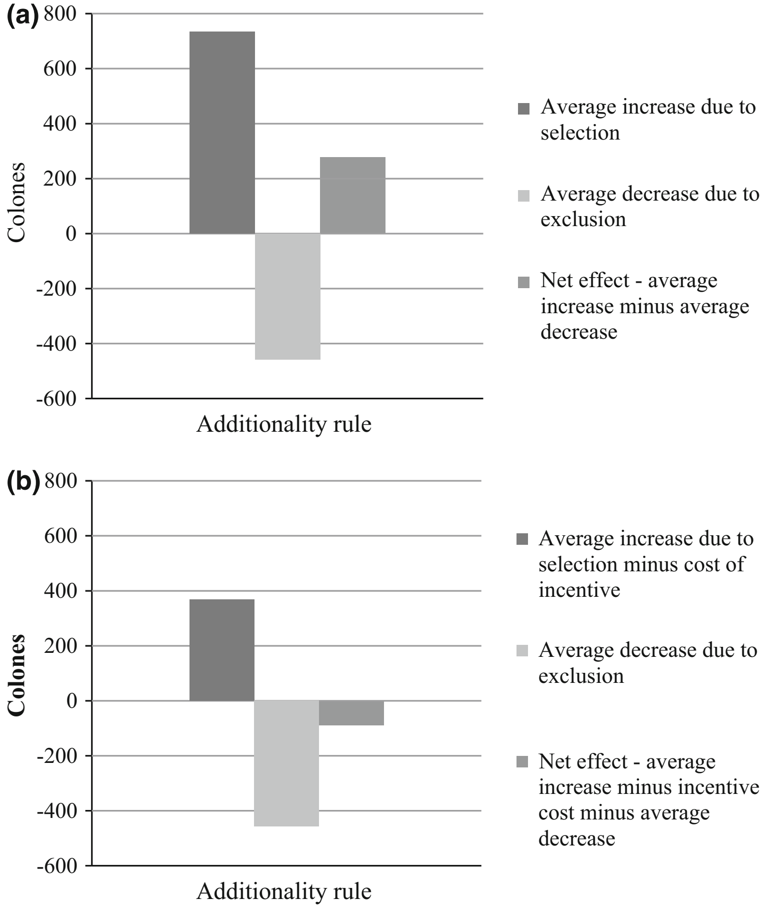

Fig. 1 Average effects of targeting rebate to encourage low contributors; selected if donation≤₡1000, excluded if donation>₡1000 **a** *Changes in Contributions*. *Note*: The net effect above is not significant (two-tailed t-test p value = 0.546). **b** *Accounting for Incentive Cost*. *Note*: The net effect above is not significant (two-tailed t-test p value = 0.832)

this assumption, focusing on those who are excluded from payment programs, be that for budgetary issues or governmental priorities.

Exclusion from a conservation payment can be based on a variety of grounds. For example, the standard recommendation from economists in this area, holding all else equal (such as cost and density of benefits), is to encourage behavior change by focusing payment on those who otherwise would not conserve forest. By design, such a rule excludes landowners who are already conserving (our Rule 1). Alternatively, payments can be designed to reward pro-forest behavior, thereby excluding those conserving very little to start (our Rule 2). Payments can also target conservation in higher priority areas, thereby excluding all other areas (our Rule 3). Our experiment randomly assigned these rules, and a control, for a carefully constructed sample of landowners in Costa Rica.

We use a strict Difference-in-Difference approach that compares the behavior of our treated subjects under any of our three selection rules to a control group. That control is tracked over time, since time and repetition alone can affect contributions. For our comparisons, that control also is chosen to be as close as possible to the treated subjects, e.g., including in initial contributions.

We find significant effects for one rule in which exclusion is based upon past contributions, in particular when those contributing little to forest conservation are selected for the targeted rebate. Such a rule excludes those who contributed more in the past. By encouraging lower contributors, this rule successfully managed to increase contributions of the selected subjects. Yet in addition, by excluding higher contributors, this rule reduced their contributions. Those who had given a lot may have felt they were voluntarily acting pro-socially and, thus, that their exclusion was unfair.

Importantly, we find that the *rationale for exclusion* matters. Excluding those giving very little to start with results in no significant change in contributions by those excluded. Also, since those selected for the targeted rebate were already contributing a large amount, this rule does not change the size of the average contribution of that group either. Finally, when exclusion is based on location, i.e. an exogenous feature not related to past behavior, we find no significant effect on either excluded and selected subjects, although we must note that small sample sizes for the *ad hoc* control groups we employ call for caution in interpreting the insignificant results we describe.

In sum, because private forest conservation is effectively a form of voluntary contribution to a public good, the targeting of a new environmental rebate to those who have shown little pro-social or pro-environmental inclinations could negatively affect the motivation of those who did choose to contribute. Our experiment shows, in a highly controlled environment, that those effects can be meaningful, i.e., could have an effect on the net impacts of a program of conservation incentives.

Various extensions of this analysis could help explore further the nature of such effects. More time periods could allow for dynamics such as strategic behavior in light of selection rules. Although we imagine that such behavior would be realistic for long-term programs, such dynamic effects were consciously ruled out by our current design. One might also consider the effect of the fraction of applicants who are selected for the incentive, as well as whether the participants are aware of the size of the fraction, i.e., whether they are aware of how others are being treated.

Even as it stands, though, our finding—subjects excluded from a targeted rebate because of their high past pro-environmental behavior react to exclusion by decreasing their contributions—seems relevant for policy makers in charge of the design of such programs. It suggests a trade-off between the gain from subjects selected for a targeted rebate and the loss from excluded subjects.

Acknowledgments We are grateful for valuable comments from two anonymous referees, and from Paul Ferraro, Olof Johansson-Stenman, Mattias Sutter, Peter Martinsson, Martin Kocher, Louis Preonas, and participants in seminars at University of Costa Rica, University of Gothenburg, Columbia University, AERE conference, EfD Initiative, EAERE conference, and EEA meeting. Financial support from the Tinker Foundation for this project is gratefully acknowledged, as is support from Sida to the Environmental Economics Unit at the University of Gothenburg and to CATIE through the Environment for Development Initiative and funding from CRED, and the NSF DMUU center at Columbia University.

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