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Behavioral Spillovers from Targeted Incentives: Losses from Excluded Individuals Can Counter Gains from Those Selected

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Abstract

An increasing number of policies condition transfers upon the taking of socially desired actions − such as donating blood, departing conflict or mitigating climate change. Many such incentives are targeted, i.e., they exclude individuals based on potential recipients' characteristics or actions. We hypothesize that: pro-sociality can be reduced by exclusion despite no price or income changes; and, further, the rationale for excluding people can itself influence whether any such undesirable side effects of pro-social incentives arise. To test for such 'behavioral spillovers', we use a laboratory experiment to study a subsidy to pro-social donations in which subjects are fully informed about why they are selected, or not, for the subsidy. We introduce three selection rules and track changes in donations. Selecting for the subsidy those who initially acted less pro-social (i.e., gave little to start) increased donations, while rewarding greater pro-sociality did not. Yet the selection rule which targeted those with lower prior pro-sociality also intentionally excluded the people who donated more initially and that selection rule reduced the donations by the excluded. This shows a tradeoff between losses from excluded and gains from selected individuals.

Keywords

monetary incentives; conditional payments;
economic experiments; behavioral economics

JEL classification

C91, D03

1. Introduction

The use of monetary incentives to promote behaviors that a society values is increasing. Called 'conditional cash transfers' − based on health outcomes, school attendance, vaccination or blood donation (Fizsbein and Schady 2009) − 'pay for performance' (Miller and Babiarz 2013) or 'performance-based payments' (Pattanayak et al. 2010), critically these incentives are conditioned upon socially approved behaviors.[1] The authorities typically decide upon a target population, i.e., who qualifies for such an incentive based upon individuals' characteristics and/or past behaviors.

Examples in developing countries include conditioning payments upon outcomes to improve health care (Miller and Babiarz 2013) and, in Colombia since 2006, conditioning the provision of cash and social services − e.g., education, health, psychosocial care − on departure from groups that have been involved in armed conflict (Denissen 2010). Around the world, to increase forest, payments conditioned on private pro-environmental land uses increasingly complement the more common approach of public land restrictions such as protected areas (see, e.g., Pfaff et al. 2009).

When compared to restrictions upon private actions, resource transfers to private individuals might well be expected to increase the acceptance of some form of state intervention. Yet the use of public funds also might generate a demand to generate societal impacts from private actions. Targeting is motivated by both goals, transfers and impact. Transfers have been targeted at those judged to merit greater societal attention, such as poor regions or single mothers, and utilized as rewards for those who have undertaken 'pro-social' behaviors on their own. Yet for impact, one may argue against rewarding past pro-sociality − instead targeting those who are less pro-social unless they receive incentives to change their behaviors (for forest, see Robalino and Pfaff 2013).

The latter argument motivates our paper. Targeting incentives at those who require incentive to act pro-socially is a way of trying to change the behavior of those selected for the incentive, in particular targeting change relative to baseline behaviors. While that is reasonable, maintaining a focus on impact one might wish to juxtapose any behavioral changes by selected individuals with *possible changes by those excluded*. If a program excludes those who act pro-socially, they may deem that program unfair, and reduce pro-sociality, even as others receive payments to do more. Such responses by the excluded individuals could counter some of the gains from those selected.

[1] Note that these examples cover a range from the mostly pro-social motivations associated with blood donations to settings with strong private motivations, such as getting kids to school (see, e.g., Mexico's *Oportunidades* program).

Such a potential reaction by the excluded is not considered, to date, within incentives design. Its potential importance may have been assumed away because for excluded individuals neither prices nor incomes change (and indirect effects might be small). Yet fixed prices and incomes do not rule out negative reactions to a lack of policy fairness. We hypothesize that those who were voluntarily acting pro-socially may feel it was unfair that they were excluded from such policies and, as a result, shift behavior counter to policy goals. Further, we hypothesize that the reasoning on which the exclusion was based could affect such reactions. Random exclusions, for instance, which are increasingly common as randomized controlled trials spread (see Duflo et al. 2007), may be perceived as less unfair than intentionally rewarding the least pro-social prior behaviors.

To test these hypotheses we employ an experiment with university students who make actual donations to a public program. We study the effects of exclusion on behaviors with real impacts, while making use of the control offered by experiments to eliminate any confounding spillovers.[2] Specifically, we compare contributions by students selected for, and excluded from, an incentive. We do so for each of three quite different selection rules that determine who faces the incentive.

Literature suggests the importance of 'non-neoclassical' motivations consistent with our sort of hypothesis, providing evidence about fairness, envy, spite and inequity aversion (Dur and Glazer 2007, Goel and Thakor 2005, Straub and Murnighan 1995, Pillutla and Murnighan 1996, Fehr and Schmidt 1999, Bolton and Ockenfels 2000). In order to shed light on the optimal design for conditional incentives, we do not attempt to distinguish among the possible interpretations of negative reactions to incentive programs but, instead, focus on whether exclusion triggers them.[3]

Our experiment uses the structure of a well-known game in which a player, i.e., the dictator, is given money to allocate between herself and another player, the receiver (see, e.g., Kahneman et al. 1986, Forsythe et al. 1994, and Hoffman et al. 1996). Our recipient is not in the same room but, instead, is a governmental conservation program perceived to pursue social goals: Bosque Vivo helps to conserve key forest ecosystems in Costa Rica, the location for our experiments (thus, we are following the leads of both Eckel and Grossman 2003 and Carpenter et al. 2008).

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[2] Spillovers also occur due to changed prices or incomes but our focus is 'behavioral spillovers' driven by fairness. In terms of prices, conservation payments might reduce the amount of land in agriculture, leading to an increase in the price of arable land that can shift land use by others. In terms of incomes, a concentration of such payments could shift local purchasing power upwards, leading non-participating landowners to produce more and thus deforest more (for theoretical work see, e.g., Wu et al., 2001 and for empirical work see Alix-Garcia et al. 2012 on Mexican PES).

[3] Section 2 considers literatures about the motivations for pro-social behavior and behavioral reaction to incentives.

Subjects are allocated money in each of three rounds, while the structure is shifted gradually. Round 1 is a standard dictator game. Round 2 introduces a regulator who, in Round 3, chooses a selection rule to allocate incentives. In Round 2, she does nothing but get a payoff that rises with donations (comparing Round 2 to Round 1 finds no effect of adding the regulators in this way). Round 3 introduces our subsidy for donations, one not previously mentioned to the participants. Here the regulator chooses, and announces, which selection rule is utilized out of the following: *additionality* – select those with low (below threshold) Round 2 contributions to the public good; *reward* – select those with high (above threshold) Round 2 contributions to the public good; and *random* – select by lottery.[4] There were also control sessions where no incentive was introduced. To isolate the effects of selection rules, Round 3 is compared to Round 2 for each rule and then those differences are compared to the same difference for control sessions without any incentive.

Our results from studying almost 400 students from the University of Costa Rica provide evidence that *pro-social behaviors may be reduced by those excluded from a targeted incentive* (one that was intended solely to increase pro-social behaviors among the selected individuals). We first confirm that gains among those selected do arise with targeting: a subsidy which targets those who had not acted pro-socially without an incentive does increase contributions. However, that targeting rule intentionally and explicitly excludes the more pro-social and, further, it does so based on pro-social behavior. We find that it yields significant negative behavioral spillovers. In contrast, neither rewarding past pro-social behaviors nor randomly selecting people for the subsidy generates significant reductions in pro-social behavior among the excluded individuals. Behavioral spillover did not occur for every exclusion but it did arise depending on the rationale.

These results set up a tradeoff in the targeting of incentives to promote pro-social behaviors. Selecting those who require an incentive to contribute may raise their contributions beyond what would happen without incentives. Yet it may also lower contributions by those people excluded, since the people who were already pro-social without incentives may stop acting in that fashion.

The remainder of this paper is as follows. Section 2 provides some additional description of relevant literatures about motivations and fairness. Section 3 describes our experimental design, i.e., the modified dictator game, as well as the sample we study. Section 4 presents our findings. In Section 5, we conclude and consider implications for the design of selective incentive policies.

[4] These labels for rules were not used with participants in order not to generate any signals about expected behavior.

2. Related Literature

In both psychology and economics, studies have shown that the introduction of monetary incentives for an action considered to be relatively pro-social can lead to less pro-social behavior ('motivation crowding') compared to when the act was voluntary (Frey 1994, Deci et al. 1999, Gneezy and Rustichini 2000, and others).[5] Sometimes the situation involves an incentive that is at first introduced and then later also removed. In all of these situations, individuals in question are faced with new incentives, i.e., they have been selected for inclusion in whatever intervention is intended to increase the desired pro-social behaviors. In contrast, we study the possibility that the individualswho are excluded from such a new incentive reduce their pro-social behaviors.[6]

Literature on social approval offers one reason those not directly affected by a new incentive might change behavior. If private charity, e.g., were driven by a desire to be perceived by others as altruistic, then a subsidy to donations could spoil the clarity of 'signaling your type to others' by donating (Ariely et al. 2009). Such a loss of signaling value applies irrespective of whether one was planning to donate or not (for economic models incorporating such social approval issues see, for instance, Akerlof 1980, Hollander 1990, Bénabou and Tirole 2006, Andreoni and Bernheim 2009, and Ellingsen and Johannesson 2008). We suspect that the non-public nature of donations in our experiment makes this less likely to be an explanation of our empirical results, though it is important to note that in principle, an experimenter does provide a type of audience.

Negative reactions upon exclusion from a new policy, such as we have hypothesized, also could be suggested by prior evidence in behavioral-and-experimental economics concerning how people respond to their treatment by others. Rabin (1993), for instance, finds that we treat nicely those who treated us fairly, and treat poorly those who did not treat us well. That is in line with considerable documentation of reciprocity (for related theory see Falk and Fischbacher 2006). That preferences for fairness may yield negative reactions also is suggested by the (relatively) equal division of resources in various games, as well as the costly punishment of those proposing unequal divisions (Dawes et al. 2007, Fehr and Schmidt 2006). For our case, agents excluded based upon behaviors that they felt were pro-social may well feel they are being treated unfairly. In our experiment, that is something the excluded agents can 'punish', by reducing their donation.

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[5] Frey and Oberholzer-Gee 1997, Cardenas et al. 2000, Frey and Jegen 2001, and Mellström and Johannesson 2008.
[6] Here we are using the term 'pro-social' to describe behaviors that would not be predicted by the narrow definition of a purely selfish *homo economicus* since they provide some benefit to others at some form of cost to oneself.

3. Experimental Design

3.1 Structure & Payoffs

While our institution was altered across rounds, the underlying experimental structure was a dictator game. The dictator received 10 tokens, each worth 1000 colones,[7] to allocate between herself and a public forest-conservation program called Bosque Vivo (our use of such a public recipient follows Eckel and Grossman 2003 and Carpenter et al. 2008). Bosque Vivo's objective is to conserve forest ecosystems in Costa Rica, which we believe are perceived as public goods. At the end of each session, all the contributions to Bosque Vivo made by the participants were actually made on site, via the internet, in order to make the credibility of the structure very clear.

The subjects, who are always the dictators, made allocation decisions between themselves and Bosque Vivo in each of three rounds. At the beginning of the game, they were instructed that one of the three rounds would be randomly selected for payment. This was to avoid any income effect since, if paid for each round, the subjects, in principle, could perceive themselves as richer and change their giving behavior. In the first round, the institution was the simple dictator game. For our purposes, this initial allocation functioned to provide information about subject type, i.e., whether the dictator, left on her own, gave a larger amount or a smaller amount to Bosque Vivo.

Before the second round, regulators were randomly selected from the participants at a rate of one regulator per ten dictators. The regulator remained anonymous to the dictators throughout; once regulators were chosen, for the rest of the experiment, subjects remained in their roles. The presence of the regulator provided an important element of realism within our experiments. Incentive programs require an institutional framework, be that within a government agency, non-governmental institution or international organization; this institution administers the funds and determines who receives the incentives. In practice, these institutions become the face of the program and are assumed to share the objective of the program that is being supported. Thus, our regulators' payoffs depend upon all the contributions to Bosque Vivo by the dictators they are regulating[8] (although the donations all go to Bosque Vivo, as we pay the regulator ourselves). These payoffs give dictators a mechanism to 'punish' or 'reward' a regulator, via their donations.

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[7] The exchange rate at this time was 500 colones/US$. Each dictator received $US20, substantial for a Costa Rican university student (about 5 lunches at the university cafeteria). High stakes were used to increase saliency. However, we note that Kocher et al. (2008) did not find any significant stake effect in a study of contributions to public goods.
[8] Specifically, the regulator's payoff equaled the average of all of the donations given by the dictators she regulated.

As explained to dictators before their second-round allocation decision, the regulator played no role in the second round, even though her payoff also depended on the round's contributions. This allowed us to test whether the mere presence of a paid regulator, even when not affecting the dictators, influenced dictator behavior (however, comparing Round 2 with Round 1 found no effect of simply introducing 'the presence of the state' or of the fact that having regulator payoffs based on the total contributions increased the total societal gains from the contributions). At the beginning of the third, and last, round, the regulator chose a selection rule that determined which sub-set of the dictators received the subsidy to their contributions made to Bosque Vivo.[9] At the end of each session, a round was chosen for payoff and all the dictators and regulators were paid.

3.2 Selection Rules

Three selection rules were tested. The first, which we call here − but not in the script − the *additionality rule*, selected for the subsidy subjects who gave 2 or fewer tokens (≤ 2,000 colones) in Round 2. All others were excluded. The selected faced an incentive equal to 50% of their contribution in Round 3, paid after they contributed.[10] So that all contributions by dictators go to Bosque Vivo, we funded the incentive and the initial allocation of tokens using research funds.[11] In dictator games, average giving is around 20% of the endowment (Camerer 2003 for a review), hence our threshold of 2,000. An *additionality rule* targeting those who did not contribute much is a standard idea for programs that aim to raise contributions beyond the giving that occurs without an incentive, i.e., aim for 'additionality' (see Rawlings and Rubio 2005, Angelsen 2008).

The second rule, which we call here the *reward rule*, selected for the subsidy subjects who contributed 5 or more tokens (i.e., 6,000-10,000 colones) to Bosque Vivo in Round 2. The rest of the subjects were excluded from the subsidy to donations. Thus, those who contributed over 50% of their Round 2 endowments were rewarded with the same incentive as was just described. We note that within both of these rules, the selection for the subsidy was based on one's prior giving.

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[9] As regulators chose the selection rule, the rules chosen reflect their preferences: 41% chose the additionality rule; 41% chose the reward rule; and 18% chose the random rule. Regulators who contributed more than five tokens in the first round, i.e., before being chosen as regulator, tended to prefer a reward rule (p-value=0.07; chi-square test).

[10] Thus, if the dictators' payoff from giving G is (10-G) without incentives, now it is effectively (10-G/2). Given the timing or mechanics of giving, perhaps the payoff would be perceived as the equivalent (10-G+P), where $P = G/2$.

[11] We were trying to have the subjects link the incentive directly to their choice of donation, 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 back to the dictators (unlike what is done within a matching-donation setup). This may bring us closer to the operational feel or framing of incentive programs (see Eckel and Grossman 2003 on importance of such framing).

The third rule, here called the *random rule*, selected subjects for the subsidy to donations using a random draw with a 50% chance of each subject being selected. That de-links selection from prior behavior, which allows us to test whether selection and exclusion *per se* have the same effects upon behaviors as selection based upon prior behaviors. For comparison to all these rules, we also had Round 3 sessions with a regulator but no incentive, i.e., the control treatment.

Importantly, participants knew why they were selected or rejected. Before Round 3 giving, each subject learned the selection rule for her case and whether she would get the subsidy. The subjects were also asked to carefully read the text explaining why they were selected or rejected.

To isolate the effects of introducing selection rules, we used difference-in-differences (DiD). For each selection rule, the outcome in Round 3 was compared with that for Round 2, and this difference was compared to the same difference where no incentive at all had been introduced.[12] This across-subject comparison of changes in behavior controls for any decrease in donation in repeated games plus the effect that introducing the regulator may have had upon contributions.[13]

3.3 Procedure

We conducted a paper-and-pen experiment with students at the University of Costa Rica. The students were paid 2,500 colones (about US$5) simply for participating in the experiment. We recruited the subjects by distributing flyers and then had them sign up on a participation list for each session on a first-come, first-served basis. Each session had room for, at most, 30 subjects. All subjects had to show university identification to ensure that they had not participated before.

The laboratory experiment was conducted with 392 students in total, combining sessions done at two different campuses of the University of Costa Rica during March-June 2011 and June 2012. We conducted 16 sessions, each lasting about one hour and involving 15-30 students.

Instructions were given orally, using Power Point to make them clear and easy to follow.[14] Before the session started, students were informed that the session would take about one hour. Subjects were asked for informed consent and given the option to leave the room. The decisions were anonymous. At the end of the experiment, subjects were asked to complete a questionnaire.

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[12] The experimenter forms a type of audience, so there could be an "experimenter effect" on behavior (see, e.g., Hoffman et al. 1996 and Zizzo 2010). The difference-in-difference approach should take care of such concerns.

[13] We also considered Round 2 - Round 1 comparisons to consider the effect of introducing the regulator, by doing some sessions with only two rounds of the basic dictator game, i.e., Round 1, to isolate the effect of repetition alone.

[14] The original script, which is in Spanish, as well as the English translation of the script, are available upon request.

4. Results

We tested exclusion and selection effects by comparing donations across rounds for controls and for those selected or excluded under any given selection rule. Selection was random or based on Round 2 donations, which were made before the dictators knew that there would be selections made in Round 3. Thus, Round 2 did not react to the subsidy or the exclusion. For any selection rule, we did not know in advance how many of the dictators would be selected. As it turned out, on average, for the additionality rule 42% of dictators were excluded from the Round 3 subsidy. For the reward rule, 87% were excluded on average and 52% were excluded for the random rule. At no point in time did any of the dictators know the shares of dictators selected or excluded.

We compared in three ways the {Round3 - Round2} changes in donations. First, we simply computed differences and compared treatment with control on average. Second, for the excluded subjects who were the novel focus of this paper, we examined donation change categorically too, i.e., we went beyond the average change to place the changes in contributions into bins based on the dictators' having raised, lowered or not changed their contribution across these two rounds. Finally, we did regression analysis of treatment effects upon the individual changes in donations.

Before those main analyses, we determined whether introducing a regulator in and of itself changed behavior. Comparing Round 1 without a regulator to Round 2 with a regulator, given no subsidies in either round, we found a small but significant decrease of 0.08 tokens (80 colones) (p-value=0.05; Wilcoxon test) as an initial 'regulator effect' estimate. Since that result also could be due simply to repetition, we also analyzed a repeated Round 1 in which the second round had no regulator either. We found no effect (p-value=0.30; Wilcoxon test) in the latter and, further, comparing the regulator effect to just repetition alone (difference in differences) we found no significant net regulator effect (p-value=0.80; Mann-Whitney test). Still, it is worth noting that some dictators did change their contributions in these settings. Thus, Round 2 was the baseline to compare with Round 3, since both featured regulators, while only Round 3 featured the subsidy.

4.1 Exclusion's Unintended Behavioral Spillovers

Table 1 presents average contributions by controls in Round 2 and Round 3. Then to compare with controls, the table also presents the averages by round for those excluded in each treatment. Subjects in the control treatment, where no incentive was introduced, on average did not change their contributions significantly between Round 2 and Round 3 (p-value=0.47; Wilcoxon test).

Excluded subjects under the additionality rule − who initially gave larger contributions − on average reacted negatively, showing a significant overall reduction of 0.62 tokens (620 colones) (p-value=0.07; one-tailed Mann-Whitney test).[15] In the absence of changes in income or prices, we call this negative 'behavioral spillovers'. This appeared to depend upon the rationale for exclusion, since no significant spillovers are seen here for exclusion via reward or random rules (p-value=0.42, one-tailed Mann-Whitney test and p-value=0.11, one-tailed Mann-Whitney test).

Table 2 goes beyond the average change to break changes into whether the excluded dictator raised, lowered or did not shift the contribution. Many individuals shifted donations even though changes were not all in the same direction. The distribution of reactions by the excluded was not significantly different from the control for any of the treatments (chi2 p-value=0.27).[16] Further, we found no significant differences across selection rules in the distributions of these reactions.[17] Yet the share of negative responses, i.e., where we would expect to see any reaction to exclusion, was significantly larger under the additionality rule versus the control (chi-square p-value=0.05). Further, the share was larger than the reward and the random rules (chi-square p-value=0.05 and p-value=0.06 respectively). In addition, looking among those who had negative reactions, the magnitude of the average fall was statistically significantly larger under the additionality rule, at 2,330 colones, than was the average fall of about 1,500 colones for the other two selection rules.

Table 3's OLS regressions explore whether these observed differences in donation changes reflect the effects of our treatment, i.e., introduction of selection rules, by adding prior donation levels as covariates to control for 'dictator type' and any dynamics of donations across rounds. Column (1) confirms Table 1, while Columns (2) and (3) add the Round 1 donations linearly and with dummies to allow non-linearity. The Round 1 donations were significant and their inclusion raises the behavioral spillovers estimate. Column (4) adds the Round 2 donations as a covariate to Column (2). We note that on their own the Round 2 donations have no significant effect, yet both of the prior rounds' donations are found to be significant in Column (4). This specification confirms significant spillovers but given a small sample and a high (~0.8) correlation between the donations in Rounds 1 and 2, we prefer to Column (4) the Columns (2) and (3) specifications.

[15] We use non-parametric tests given the small sample size, but t-tests were also used with unchanged results.

[16] 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 got 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.

[17] Comparing distributions of reactions between selection-rule treatments, we get: additionality rule to reward rule, chi2 p-value=0.13; additionality versus random, chi2 p-value=0.16; reward versus random, chi2 p-value=0.49.

4.2 Selection's Intended Additionality

Table 4 presents average contributions by controls in Round 2 and Round 3 and, to compare with control differences across those rounds, round averages for the selected in each treatment. The incentive significantly increased contributions for the additionality targeting and the lottery. The insignificant increase (p-value=0.25; Wilcoxon test) for those selected under a reward rule was not surprising; in that case, those selected were contributing high amounts already. Thus, the funds spent incentivizing the subjects using the reward rule yielded no increase in contributions.

Under the random rule, those lucky enough to be selected to get the subsidy increased their contributions, on average, by 1.33 tokens (1,330 colones) between rounds 2 and 3 (p-value<0.01, Wilcoxon test). Comparing that to the control treatment indicated a strong significant gain in contributions due to the treatment (p-value<0.01; one-tailed Mann-Whitney test). The estimated effect upon contributions under the random selection rule was the largest across all of these rules.

Under the additionality rule − i.e., when the subsidy was targeted at those who had acted less pro-socially in Round 2 − we found a significant rise in contributions, on average 0.90 tokens (900 colones) (p-value<0.01; Wilcoxon test). That generated an estimated effect, relative to the control, of 0.78 tokens (780 colones) due to the influence of the subsidy to donations, and this estimated treatment effect also was significant (p-value<0.01; one-tailed Mann-Whitney test).[18]

Table 5's OLS regressions explore whether these observed differences in donations changes reflect the effects of our treatment, i.e., introduction of selection rules, by adding prior donation levels as covariates to control for 'dictator type' and any dynamics of donations across rounds. Column (1) confirms Table 4, while Columns (2) and (3) add the Round 1 donations linearly and with dummies to allow non-linearity. Round 1 donations levels were significant in Column (2), although not in Column (3); in neither case did their inclusion change very much the estimate of the additionality treatment's effect on the selected dictators' contributions. The lottery treatment's effect was also qualitatively robust and it remained the largest subsidy effect. Column (4) adds to Column (2) contributions in Round 2 which as in Table 3 have no significant effect on their own; both prior donations levels were significant but changed little the estimated impact of treatment. For the reasons given above, we prefer Column (2) to adding Round 2 donations in Column (4). In any case, Table 5's results are fairly robust concerning effects of being selected for a subsidy.

[18] These results match common justifications for additionality rules. Our goal is to juxtapose selected with excluded.

4.3 Net Effects of Targeting?

Our results suggest positive treatment effects for selected subjects under two rules: a random assignment of the donations subsidy; and the additionality rule that targets prior low contributors. However, only one of those rules excludes subjects based explicitly on prior pro-social behavior and that rule − additionality − is the only one for which we found negative behavioral spillovers: targeting higher prior contributors for exclusion led them to reduce their public contributions. It is both interesting and policy relevant that exclusion under a lottery did not generate a spillover.

Only the additionality rule had significant effects in both directions, increasing contributions for the selected and decreasing them for the excluded. For that rule, in Figure 1, we compare gain per person selected with loss per person excluded. The losses for the excluded counter gains for the selected under additionality: the average net rise of 0.16 is not significant (p-value=0.55; Mann-Whitney test). Thus, in evaluating selection rules, responses by those excluded can matter.

There was no significant effect of exclusion for the other rules. Hence, the average effect was zero for the reward rule and was significantly positive for the random rule (p-value=0.61,0.07 respectively; Mann-Whitney test). Comparing net effects, we found the random rule performed bettercompared to additionality (p-value=0.01; Mann-Whitney test). There was no difference between the net effects of the additionality and reward rules (p-value=0.38; Mann-Whitney test).

From a program's point of view, the cost of paying incentives also needs to be considered in measuring net gains of the incentives. Figure 2 redoes Figure 1 (both based upon Tables 1 and 4) but with that incentive cost now being subtracted from average gains per person selected. The net for additionality was an insignificant loss of -0.23 (p-value=0.31; Mann-Whitney test) and the net also was not significant under the reward rule (p-value=0.79; Mann-Whitney test), yet we still found a significant overall gain under the random rule (p-value=0.07; Mann-Whitney test).

The net effect of any selection rule clearly could depend on the share selected or excluded. At the extreme, if almost all subjects are included, then a few disgruntled individuals empirically are not likely to significantly reduce a program's net gains in contributions under additionality. However, if the share of excluded subjects were large, then our results could imply, in principle, that the occurrence of negative behavioral spillovers − in reaction to exclusion − might, on net, significantly decrease donations if an incentive has targeted additionality (low prior donations).

Since our subjects were not informed about the share of subjects who were excluded, while in a public policy they might be, and as knowledge of that share might affect behavioral spillovers, we cannot say what might have happened were more, or fewer, to be excluded in a public policy.

5. Conclusions

We have provided empirical evidence that stakeholders *excluded* from monetary incentives that benefit others may choose to act less pro-socially than before any incentive was introduced. This unintended effect of exclusion, which occurred even without changes in prices or incomes − i.e., what we call a 'behavioral spillover' − depended upon the selection rule for the incentive. While targeting the incentives to those who had acted less pro-socially increased social donations by those selected, only that selection rule *reduced* the donations made by those excluded, which we had hypothesized since that selection rule intentionally excluded the high prior contributors.

Neither rewarding past pro-social behavior with payments nor randomly selecting subjects for incentives yielded such negative behavioral spillovers among those who were excluded. The empirically popular 'reward rule', which steers new incentives towards those who provided high pro-social contributions on their own, also yielded no rise in contributions among those selected. While redistribution could motivate this, as hypothesized this did not increase the public good.

Our results demonstrated a tradeoff for the targeting of incentives that are intended to attain social goals through private actions. The standard recommendation from an efficiency standpoint has been to give the incentive to those who in its absence make choices contrary to social goals. While we found that this did increase the contributions by those selected through such targeting, our results provided new evidence that there is a downside from the alienation of those excluded *when they are excluded based on past pro-social behavior* (as opposed to via a random lottery). Such effects, as well as the role of the rationale provided, have implications for program designs.

Further research certainly could extend our understanding of the various possible net effects of such attempts to target social incentives. One way would be to vary the rules so that the share selected (excluded) from the program varies, and that share is known by all of the actors, since we could not say from our experiments whether higher or lower rates of exclusion matter at all. Other selection rules could also be considered, as well as hybrids designed in order to attempt to maximize gains in social contributions from the selected, and minimize losses from the excluded.

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Table 1

Contribution Changes by those Excluded -- Averages

	Control (no exclusion)	Additionality (Round2>2)	Reward (Round2<6)	Random (lottery)
# obs.	99	50	107	26
Round 2	1.58 tokens	5.20 tokens	2.40 tokens	1.69 tokens
Round 3	1.70 tokens	4.70 tokens	2.62 tokens	2.00 tokens
Rd3 - Rd2	+0.12 tokens	-0.50 tokens	+0.22 tokens	+0.31 tokens
Behavioral spillovers (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 of the exclusion.

<u>**Table 2**</u>

Contribution Changes by those Excluded -- Categories

		Control (99)	**Additionality (50)**	**Reward (107)**	**Random (26)**
Rise	**%**	25% (25 obs)	26% (13 obs)	27% (29 obs)	39% (10 obs)
	#	+1.52 tokens	+1.31 tokens	+2.00 tokens	+1.50 tokens
Fall	**%**	21% (21 obs)	36% (18 obs)	22% (23 obs)	15% (4 obs)
	#	-1.24 tokens	-2.33 tokens	-1.52 tokens	-1.75 tokens
None	**%**	54% (53 obs)	38% (19 obs)	51% (55 obs)	46% (12 obs)
	#	0 token	0 token	0 token	0 token

Table 3

Contribution Changes by those Excluded -- OLS Regression

	(1)	(2)	(3)	(4)
Additionality	-0.62** (0.01)	-1.01*** (0.00)	-0.89*** (0.00)	-0.47* (0.10)
Reward	0.09 (0.64)	-0.04 (0.85)	-0.01 (0.98)	0.01 (0.98)
Random	0.18 (0.55)	0.16 (0.61)	0.19 (0.55)	0.15 (0.62)
Round 1	---	0.13* (0.01)	---	0.36*** (0.00)
Rd1 $0 \leq g \leq 2$	---	---	---	---
Rd1 $2 < g \leq 5$	---	---	0.36* (0.06)	---
Rd1 $g > 5$	---	---	0.60* (0.09)	---
Round 2	---	---	---	-0.35*** (0.00)
constant	*0.12 (0.40)*	*-0.09 (0.58)*	*0.01 (0.93)*	*-0.07 (0.66)*
#obs	*282*	*282*	*282*	*282*
R^2	*0.04*	*0.06*	*0.05*	*0.14*

p-values in parentheses; ***=significant at 1%, **=significant at 5% , *=significant at 10%

<u>**Table 4**</u>

Contributions Changes by those Selected -- Averages

	Controls (no selection)	**Additionality (Rd2 ≤ 2)**	**Reward (Rd2 ≥ 6)**	**Random (lottery)**
# obs.	99	70	16	24
Round 2	1.58 tokens	0.87 tokens	6.81 tokens	3.13 tokens
Round 3	1.70 tokens	1.77 tokens	7.31 tokens	4.46 tokens
Rd3 - Rd2	+0.12 tokens	+0.90 tokens***	+0.50 tokens	+1.33 tokens***
Additionality (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 of the selection.

Table 5

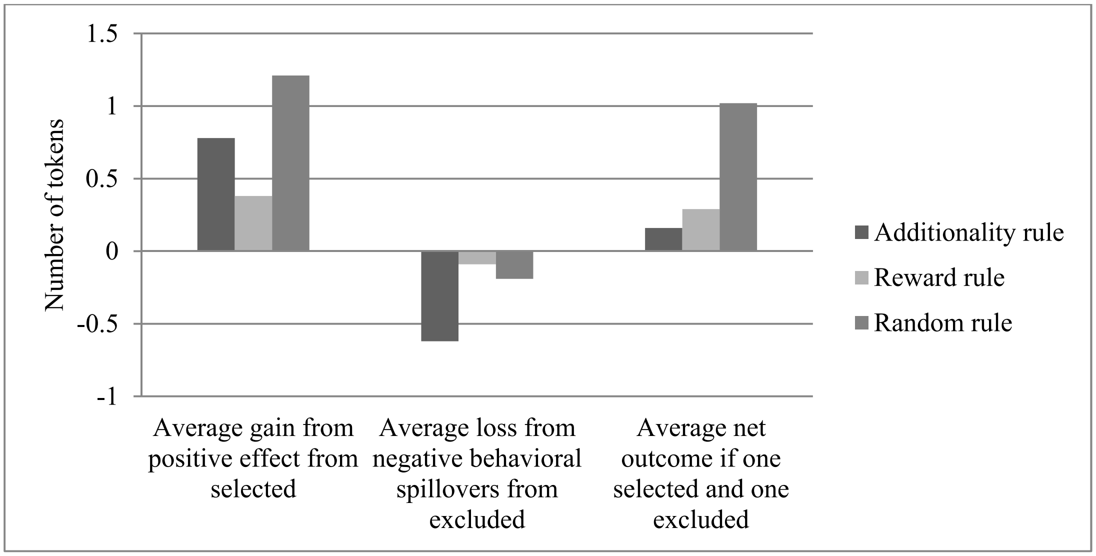
Contribution Changes by those Selected -- OLS Regression

	(1)	(2)	(3)	(4)
Additionality	0.78*** (0.00)	0.82*** (0.00)	0.81*** (0.00)	0.75*** (0.00)
Reward	0.38 (0.64)	-0.05 (0.91)	0.06 (0.89)	0.38 (0.45)
Random	1.21*** (0.00)	1.05*** (0.00)	1.14*** (0.00)	1.15*** (0.00)
Round 1	---	0.11** (0.05)	---	0.22*** (0.01)
Rd1 $0 \leq g \leq 2$	---	---	---	---
Rd1 $2 < g \leq 5$	---	---	0.17 (0.51)	---
Rd1 $g > 5$	---	---	0.60 (0.18)	---
Round 2	---	---	---	-0.16*** (0.08)
constant	*0.12 (0.40)*	*-0.07 (0.70)*	*0.06 (0.72)*	*0.01 (0.96)*
#obs	*209*	*209*	*209*	*209*
R^2	*0.09*	*0.11*	*0.10*	*0.12*

p-values in parentheses; ***=significant at 1%, **=significant at 5% , *=significant at 10%

Figure 1

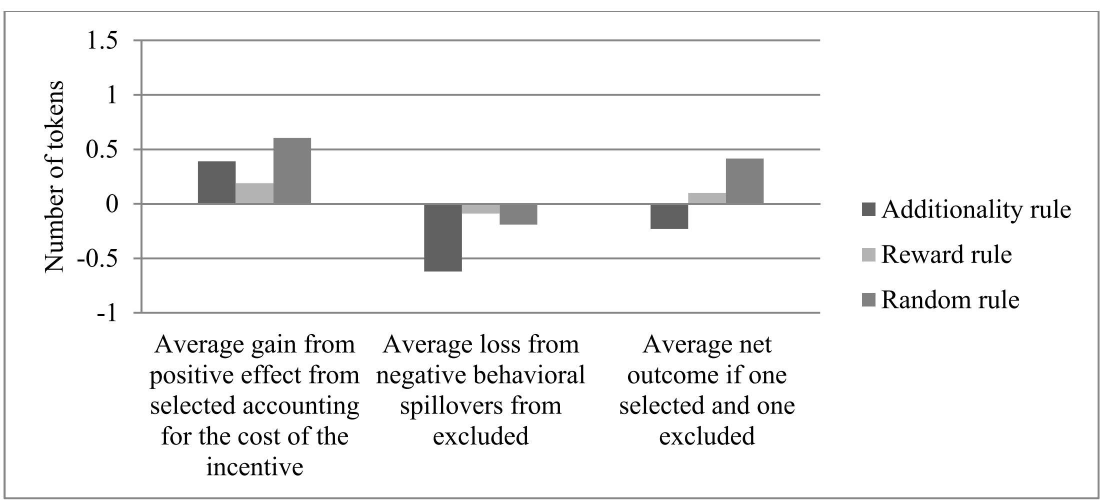
Comparing Selection and Exclusion Effects



Net outcomes if comparing one selected and one excluded individual.
They are not significant under the additionality and reward rules
but the net outcome is significantly positive for the random rule
(Mann-Whitney tests p-value=0.55, 0.61 and 0.07, respectively).

Figure 2

Comparing Selection and Exclusion Effects including Costs



Net outcomes if comparing one selected and one excluded individual
and subtracting from contribution changes the cost of the incentive.
They are not significant under the additionality and reward rules
but the net outcome is significantly positive for the random rule
(Mann-Whitney tests p-value=0.31, 0.79 and 0.07, respectively).