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

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Abstract

Incentives conditioned on socially desired acts such as donating blood, departing conflict or mitigating climate change have increased in popularity. Many incentives are targeted, excluding some of the potential participants based upon characteristics or prior actions. We hypothesize that pro-sociality is reduced by exclusion, in of itself (i.e., fixing prices and income), and that the rationale for exclusion influences such 'behavioral spillovers'. To test this, we use a laboratory experiment to study the effects of a subsidy to donations when participants are fully informed about why they are selected, or not, for the subsidy. We study the effects of introducing different selection rules upon changes in donations. Selecting for the subsidy those who initially acted less pro-social (i.e., gave little to start) increased donations, while random subsidies and rewarding greater pro-sociality did not. Yet a selection rule which targets lower prior pro-sociality also intentionally excludes the people who donated more initially, and only that rule reduced donations by the excluded. This shows a tradeoff between losses from excluded participants and gains from selected.

Keywords

monetary incentives; conditional payments;
economic experiments; behavioral economics

JEL classification

C91, D03

1. Introduction

Monetary incentives to promote behaviors associated with societal objectives are increasing. Whether they are called 'conditional cash transfers' for health outcomes, schooling, vaccination or blood donations (Fizsbein and Schady 2009), 'pay for performance' (Miller and Babiarz 2013) or 'performance-based payment' (Pattanayak et al. 2010), such transfers critically are conditioned on some socially approved behaviors done by the target population.[1] Authorities typically decide who qualifies for such an incentive, based upon individuals' characteristics and/or past behaviors.

Examples of such incentives within developing countries include efforts to pay for outcomes to improve the provision of health care (Miller and Babiarz 2013). In Colombia, since 2006 the government has provided cash and social services − education, health, psychosocial care − to those willing to leave groups long involved in conflict (Denissen 2010). In the environment area, increasingly incentives based on private pro-environmental behavior are being used to substitute for public restrictions upon land use, such as protected areas (see, for instance, Pfaff et al. 2009).

Relative to the state prohibiting or restricting private action, transferring public resources to private individuals may increase the acceptance of policies − as is clear in the environment arena. Yet using public funds also may increase the demand for impact from private behavioral change. Either of these concerns − with transfers or impacts − could motivate the targeting of incentives. For instance, incentives might target groups deemed to represent higher societal benefits, such as poorer areas or single mothers. Alternatively, a selection for incentives might at least effectively reward people for 'pro-social' behaviors that they took under their own initiative (pre-incentive). Finally, some have argued (for the environment-forest arena, e.g., see Robalino and Pfaff 2013) for the opposite of a 'reward', i.e., for the targeting of incentives to those who without incentives are not too likely to act pro-social, plus the avoidance of those who already tend to act pro-social.

The latter argument follows directly from a focus upon impacts and it motivates our paper. That argument focuses on the people selected, highlighting for whom incentives matter the most. Given that, we juxtapose any effects upon the selected with *possible effects upon those excluded*. Why would the excluded respond? If the targeting excludes those who were acting pro-socially, they may feel it is unfair and act less pro-socially when excluded as others are bribed to 'reform'.

[1] We note that these examples cover a range between mostly pro-social motivation, associated with blood donations, and settings with strong private motivations, like getting kids to school (see, e.g., Mexico's *Oportunidades* program).

This potential reaction often is not considered in the design of selective incentive programs. Ignoring it might seem reasonable, as prices and income are unchanged: neither is affected by the policy directly, for people who are excluded; while indirect effects on either might well be small. Yet fixed prices and incomes do not rule out individuals reacting to something they see as unfair. We hypothesize that individuals who were voluntarily acting pro-socially might: react negatively to being excluded by what they see as unfair policy design; and, as a result, shift their behavior. Further, we hypothesize that the reasoning on which exclusion was based could affect reactions. Random exclusions, for instance, may not seem as unfair as rewarding prior anti-social behavior.

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 still making use of the control offered by experiments in order to eliminate indirect effects, so that we can be sure any effects we find from *not being paid* are in fact 'behavioral spillovers'. 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.

A large behavioral literature suggests the importance of many non-neoclassical motivations consistent with our hypothesis, including for instance fairness, envy, spite and inequity aversion (Straub and Murnighan 1995, Pillutla and Murnighan 1996, Fehr and Schmidt 1999, Bolton and Ockenfels 2000, Goel and Thakor 2005, Dur and Glazer 2007). We do not attempt to distinguish among potential motivations for negative reactions but, instead, focus on whether being excluded can trigger them, in order to shed light on the optimal design for conditional incentive programs.[2] If such behavioral spillovers from exclusion exist, and if they shift with the selection rationale, those designing such programs should plan around impact on both the selected and the excluded.

Our experiment has the basic structure of the well-known dictator game in which a player, i.e., the dictator, is given a sum of money to allocate between himself/herself and another player, i.e., the receiver (see, e.g., Kahneman et al. 1986, Forsythe et al. 1994, Hoffman et al., 1994). In contrast to the standard dictator game where the recipient is in the room, the recipient here is a governmental conservation program called Bosque Vivo. Bosque Vivo's objective is to conserve key forest ecosystems in Costa Rica, and it is perceived as pursuing a social goal. In this regard, our experiment follows the lead of both Eckel and Grossman (2003) and Carpenter et al. (2008).

[2] Section 2 considers how a focus on exclusion differs from other literatures considering motivations and spillovers.

Subjects made three allocation decisions in sequence with the basic design of dictator games gradually shifting across rounds. Round 1 has no institution. Round 2 introduces an institution in the form of a regulator who chooses the selection rule for allocating the subsidy to contributions in the next round and who benefits from contributions (note that comparing Round 2 to Round 1 finds no effect of the regulator, despite that actor providing somebody participants can 'punish'). Round 3 is played with a new subsidy and full disclosure about which selection rule was chosen: *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. [3] There were also control sessions where no incentive was introduced. To isolate the effects of the selection rules, we use a difference-in-difference approach: for each rule with an incentive, the Round 3 outcomes are compared to allocations in Round 2; and then those differences are compared to the same difference for control sessions without any incentive.

Our results for a subject pool of over 400 students from the University of Costa Rica provide evidence that *pro-social motivations can be reduced by one's exclusion from a pro-social policy*. To start, first we confirm prior hypotheses that the gain from the selected can rise with targeting: only a subsidy that targets those who did not act pro-social on their own increased contributions. However, that targeting rule intentionally and explicitly excludes the more pro-social and does so specifically because of pro-social behavior. As hypothesized, that is the only subsidy that causes significant negative behavioral spillovers. Neither rewarding greater past pro-social behavior nor randomly selecting people for a subsidy generates negative behavioral spillovers from exclusion. Thus, behavioral spillovers did not occur for just any exclusion but depended upon the reasoning.

These results set up a tradeoff in the targeting of incentives to promote pro-social behaviors. Selecting those who require incentives to contribute may raise contributions beyond what would have happened 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 literature on spillovers and motivation. Section 3 describes our experimental design, i.e., the modified dictator game as well as the sample. Section 4 presents our findings. In Section 5, we discuss our results and we consider implications for the design of selective incentive policies.

[3] 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 monetary incentives sometimes lead to less pro-social behavior once the voluntary act is shifted to a market-based relationship (Adamowicz et al. 1998, Deci 1999, Hanley et al. 2000).[4] Sometimes the situation considered involves a incentive being provided and then later removed. However, in all of these situations the individuals in question are faced with new incentives. In contrast, we study the possibility of stakeholders who are excluded from new monetary incentives reducing pro-social behaviors.[5]

Literature on social approval offers a perspective upon why those not directly affected by an incentive still might respond. If private charity donations, e.g., were driven by the desire to be perceived by others as altruistic, then monetary payments or 'bribes' offered to people who make donations could spoil the clarity of type signals to others through donations (Ariely et al. 2009). The lost signaling value of donating might occur irrespective of whether one was planning to donate or not (for economic models incorporating social approval see e.g., Alpizar et al. 2008, Benz and Meier 2008, Bulte et al. 2008, Engel and Palmer 2008, Andreoni and Bernheim 2009). We suspect that the non-public nature of donations in our experiment makes this less likely to be an explanation of our results, although in principle the experimenter forms a type of audience.

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

[4] For empirical evidence see e.g., Gneezy and Rustichini (2000), Cardenas et al. (2000), Frey and Oberholzer-Gee (1997) and Mellström and Johannesson (2008).

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

3. Experimental Design

3.1 Structure & Payoffs

While we introduce some additional institutions across rounds, the underlying structure of our experiment is just a dictator game. A dictator receives 10 tokens, each worth 1000 colones[6], to allocate between herself and a public conservation program called Bosque Vivo (much as in Eckel and Grossman (2003) as well as in Carpenter et al. (2008)). The objective of Bosque Vivo is to conserve forest ecosystems in Costa Rica, which we believe are perceived as public goods. At the end of each session, the contributions to Bosque Vivo were made on site via the internet.

Subjects make allocation decisions 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 the incentive. This was to avoid any income effect, since if paid for each round subjects would become richer and presumably might increase giving. In the first round, there is no institution but only a simple dictator game. For our purposes, this functions to provide information about subject type, i.e., whether the dictator left on her own shares a large amount or a small amount with Bosque Vivo.

Before the second round, regulators are randomly selected from the pool of subjects at a rate of one regulator per ten dictators. The regulator remained anonymous to the dictators throughout; once regulators are chosen, for the rest of the experiment subjects simply remain in their roles. The presence of the regulator provides an important element of realism within our experiments. Incentive programs like the ones described in the introduction require an institutional framework, be it a government agency, a non-governmental institution or an international organization, that administers funds and chooses who receives the incentives. In practice, these institutions become the face of the program and are assumed to share the objective of the program being supported. Thus, our regulators' payoffs depend on the contributions to Bosque Vivo by the dictators they are regulating[7] (though 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.

As explained to dictators before their second-round allocation decisions, the regulator played no role in the second round, even though her payoff also depended on the round's contributions.

[6] 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.
[7] Specifically, the regulator's payoff equaled the average of all of the donations given by the dictators she regulated.

This structure allows us to control for whether the mere presence of a regulator, without a choice that affects dictators in the sense of choosing a selection rule, might influence dictator behavior (comparing Round 2 with Round 1 finds no effect of simply introducing a regulator with payoff even though in principle an altruistic or spiteful dictator would perceive changed total payoffs). At the beginning of the third and last round, the regulator chose a selection rule, decided which dictators get the subsidy that may influence the amount of money contributed to Bosque Vivo.[8] At the end of each session, a round is chosen for payoff and the dictators and regulators are 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.[9] So that all contributions by dictators go to Bosque Vivo, we fund the incentive using research funds additional to the initial allocation of tokens.[10] In dictator games, average giving is around 20% of the endowment (see Camerer 2003 for a review), hence our threshold. An *additionality rule* targeting those who did not contribute much on their own is a standard idea for programs aiming to raise contributions beyond giving without an incentive, i.e., "additionality" (e.g., O´Donoghue and Rabin 1999, Angelsen 2008).

The second rule, here called the *reward rule*, selects subjects who contributed over 5 tokens (i.e., 6,000-10,000 colones) in Round 2. The rest were exluded. Thus, those who contributed over 50% of their Round 2 endowments were rewarded with the same incentive as was just described. We note that in both of these rules the selection for the subsidy was based on one's prior giving.

The third rule, here called the *random rule*, selects subjects for the incentive randomly with a 50% chance. That delinks selection from prior giving behavior, allowing us to test whether such selection and exclusion has the same effects on behaviors than selection based on behavior. For comparison to all these rules, we also had Round 3 sessions with regulator but no incentive.

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[8] As regulators choose 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. We also note that regulators who contributed more than five in the first round, i.e., before chosen as regulator, tend to prefer the reward rule (p-value=0.07; chi-square test).

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

[10] We tried to have subjects link the incentive to their action, i.e., not see it as seed money or matching funds (e.g., List and David Lucking-Reiley 2002). e.g. refunded money went to the dictators (unlike in a matching donation). This may bring us closer incentive programs (see Eckel and Grossman 2003 on the importance of such framing).

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 explore the existence of behavioral spillovers, we use difference-in-differences (DiD). The outcome for each selection rule in Round 3 is compared to the outcome in Round 2, and this difference is compared with the same difference where no incentive at all has been introduced.[11] This across-subject comparison of changes in behavior controls for any decrease in donation in repeated games and the effect that introducing the regulator may have had upon contributions.[12]

3.3 Procedure

We conducted a paper and pen experiment with students at the University of Costa Rica. The students were paid a fixed amount of 2,500 colones (US$5) 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-serve 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 as well as June 2012. We conducted 16 sessions, each lasting 1-1.5 hours and involving 15-30 students.

Instructions were given orally, using Power Point to make them clear and easy to follow.[13] 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.

[11] As noted above, the experimenter forms a type of audience so there could be an "experimenter effect" on behavior (e.g., Hoffman et al. 1996, Zizzo 2010). The difference-in-difference approach should take care of such concerns.
[12] We also consider Round 2 - Round 1 comparisons in considering 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.
[13] The script in Spanish and the English translation of the script are available upon request.

4. Results

Our difference-in-difference approach tests the effects of selection (exclusion) by comparing the changes in donations across rounds for different subsamples: the individuals selected for the subsidy (or those excluded), under any given selection rule; versus the individuals in the control. For any given selection rule, we did not know how many dictators would be selected (excluded). Applying the selection rule chosen by a regulator, selection is determined by Round 2 donations that were made before dictators had learned there would be selection rules used within Round 3 (thus, Round 3 behavior can react to the subsidy and the selection itself, but Round 2 could not). As it turned out, on average 42% of dictators were excluded from the Round 3 subsidy for the additionality rule. For the reward rule, 87% were excluded, while 52% were for the random rule. At no point in time, however, did any dictators know the share of dictators selected (excluded).

We compare the different sets of (Round 3 - Round 2) changes empirically in three ways. First, we simply compute differences and compare them on average across treatment and control. Second, we compare a binary indicator of whether individual donations rose or fell in Round 3. Finally, we do regression analysis of treatment effects upon the individual changes in donations.

As noted above, before those main analyses we determine whether introducing a regulator in of itself changed behavior. Comparing Round 1 without a regulator to Round 2 with a regulator, given no subsidies in either round, we find a small but significant decrease of 0.08 tokens (80 colones) (p-value=0.05; Wilcoxon test) as an initial estimate of a possible 'regulator effect'. Since that result also could be due simply to repetition, however, we also analyze a repeated Round 1, i.e., when the second round has no regulator. We find no effect (p-value=0.30; Wilcoxon test) in the latter test. Further, comparing the regulator effect to just repetition (difference in differences) we find no significant net regulator effect (p-value=0.80; Mann-Whitney test). However, we it is worth noting that some dictators changed contributions in these settings. Thus, Round 2 is a good baseline to compare to Round 3 as both feature regulators while only Round 3 features a subsidy.

4.1 Exclusion's Unintended Behavioral Spillovers

Table 1 presents − by treatment − average Round 2 and Round 3 contributions for Excluded, with differences between rounds computed alongside their differences from control differences. 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).[14] In the absence of changes in income or prices, we call this reaction negative 'behavioral spillovers'. It appears 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 considers the behavior more categorically, breaking the continuous average change in contributions down into whether the dictator raised, lowered or did not change in contribution across these rounds. A large share of individuals do make some kind of a change and not all of those reactions are in the same direction. The distribution of reactions between the excluded and control is, however, not significantly different for any of the treatments (chi2 p-value=0.27)[15]. Further, we find no significant differences across selection rules in the distribution of reactions.[16]

Yet the share of negative responses, which is where we expect to see reactions to exclusion, is significantly larger under the additionality rule compared to control (chi-square p-value=0.05). Further, it is larger compared to both 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 is statistically significantly larger under the additionality rule, at 2,330 colones, than is the average fall of about 1,500 colones for the other two selection rules.

Table 3's OLS regressions explore whether the differences in the change in donations reflect treatment effects, by adding prior donation levels as covariates to control for the dictator's 'type' plus potential donations dynamics across rounds. Column (1) confirms Table 1 while (2) and (3), respectively, add the Round 1 donations linearly and non-linearly, the latter employing dummies. These prior donations are significant and their inclusion raises the behavioral spillovers estimate. Column (4) adds the Round 2 contributions to (2) − noting that on their own they have no effect. Both prior-donations levels are significant and (4) confirms a significant spillover but, given our small sample and a high (~0.8) Rounds-1-and-2 correlation, (4) is not our preferred specification.

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

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

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

4.2 Selection's Intended Additionality

Table 4 presents, by treatment, average Round 2 and Round 3 contributions for all selected, with differences between rounds computed alongside their differences from control differences. The incentive significantly increases contributions for the additionality targeting and the lottery. The insignificant increase (p-value=0.25; Wilcoxon test) for those selected under a reward rule is not surprising, since in that case those who are selected were contributing high amounts already. Thus, funds spent incentivizing subjects using the reward rule yield no increase in contributions.

Under the random rule, those who were lucky enough to be selected to receive 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 indicates a strong significant gain in contributions due to the treatment (p-value<0.01; one-tailed Mann-Whitney test). In fact, the estimated effect upon contributions under random selection is the largest across all of these rules.

Under the additionality rule, i.e., when the subsidy is targeted towards those who had acted less pro-socially in Round 2, we find a significant rise in contributions, on average 0.90 tokens (900 colones) (p-value<0.01; Wilcoxon test). That yields an estimated effect on of the treatment, relative to the control, of 0.78 tokens (780 colones) due to the provision of the donation subsidy. That estimated treatment effect is significant (p-value<0.01; one-tailed Mann-Whitney test). [17]

Table 5's OLS regressions explore whether the differences in the change in donations reflect treatment effects, by adding prior donation levels as covariates to control for the dictator's 'type' plus potential donations dynamics across rounds. Column (1) confirms Table 4 while (2) and (3), respectively, use the Round 1 donations linearly and non-linearly, the latter employing dummies. Round 1 donations are significant in (2) though not in (3) and in neither case does their inclusion change much the estimate of the additionality treatment's effect in raising dictators' contributions. The lottery treatment's effect is also qualitatively robust and it remains the largest subsidy effect. Column (4) adds to (2) the Round 2 contributions, which on their own have no significant effect. The prior donation levels are significant but make little difference to estimated selection effects. For the reasons given above, we suggest that (2) is a preferred specification, compared with (4). In any case, for these effects of being selected for the subsidy Table 5's results are fairly robust.

[17] The performance of the additionality rule for the selected, and lack of results for the reward rule for the selected, matches justifications for encouraging additionality rules. Our goal is to compare effects on selected and 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 find negative behavioral spillovers: targeting higher prior contributors for exclusion leads them to reduce their public contributions. It is interesting and policy relevant that exclusion under a lottery does not generate that spillover.

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

From a program's point of view, the cost of paying incentives also needs to be considered in measuring net gains of targeted incentives. Figure 2 redoes Figure 1 (both based on Tables 1, 4) but with that cost of the incentive now being subtracted from average gains per person selected. The net then becomes an insignificant loss of -0.23 tokens (p-value=0.31; Mann-Whitney test). Thus, in this case, incentives targeting low contributions yield zero impact overall both when the effects on the excluded are considered and when, in addition, so is the cost of the incentive. This overall outcome also is not significant under the reward rule (p-value=0.79; Mann-Whitney test), yet we find a significant overall gain under the random rule (p-value=0.07; Mann-Whitney test). These results emphasize that policy design consider how to maximize the net gains minus losses.

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. But if the share of excluded subjects is large, in principle our results could imply that behavioral spillovers might, on net, significantly decrease donations when an incentive targets additionality. 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. Discussion

This paper 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., is what we call a 'behavioral spillover' − depended upon the selection rule for the incentive. Targeting the incentives toward those who had acted less pro-social increased social donations by those selected for an incentive. Yet only that selection rule reduced donations by the excluded which is not surprising since that selection rule intentionally excluded the prior high contributors.

Neither rewarding past pro-social behavior with the new incentives nor randomly selecting subjects for incentives yields such negative behavioral spillovers among those who are excluded. The empirically popular 'reward rule', which steers new incentives towards those provided high pro-social contributions on their own, also yields no rise in contributions among those selected. While redistribution may be a reason for it, using funds that way does not raise the public good.

Our results demonstrated a tradeoff for targeting of incentives aimed to promote 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 decisions contrary to the social objective. While we find that this does 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 understanding of the net effects of any such targeting by varying the rules so that the share selected (excluded) varies and it is known by all the actors, since we could not say from our experiments whether higher or lower rates of exclusion matter. 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

Excluded & Control Group Average Contribution Changes

	All Controls (no exclusion)	**Additionality (Rd2 > 2)**	**Reward (Rd2 < 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 Leakage (DiD)	**---**	-0.62 tokens*	+0.09 tokens	+0.19 tokens

***=significant at 1%, **=sigopnificant 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.

Table 2

Excluded & Control Group Categorical Contribution Reactions

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

Table 3

Excluded & Control Group Individual Contribution Changes

	(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 ≤ g ≤ 2	---	---	---	---
Rd1 2 < g ≤ 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*
R2	*0.04*	*0.06*	*0.05*	*0.14*

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

Table 4

Selected & Control Group Average Contributions Changes

	All 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***
Targeting's 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

Selected & Control Group Individual Contributions Changes

	(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*
R2	*0.09*	*0.11*	*0.10*	*0.12*

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

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

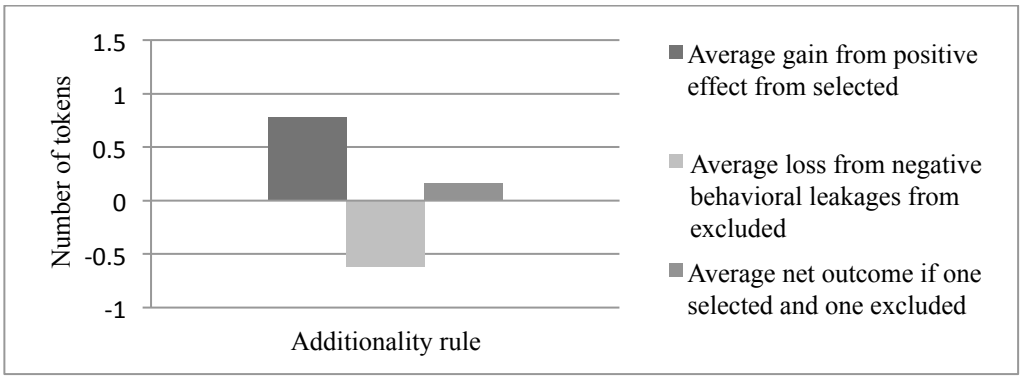

Figure 2. The effect of selection or exclusion under the additionality rule and the net outcome if comparing one selected and one excluded individual accounting for the cost of the incentive. The net outcome under the additionality rule is not significant, according to a Mann-Whitney test p-value=0.31.