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# Push, don't nudge: Behavioral spillovers and policy instruments

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## HIGHLIGHTS

- Policy interventions are generally evaluated only for their direct effectiveness.
- Little is known about their ability to persist over time and spill across contexts.
- We experimentally compare two instances of nudges and two of push measures.
- Push measures result more effective than nudges in promoting fairness directly.
- Their effect also persists over time. However, it does not spill across contexts.

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## ABSTRACT

Policy interventions are generally evaluated for their direct effectiveness. Little is known about their ability to persist over time and spill across contexts. These latter aspects can reinforce or offset the direct impacts depending on the policy instrument choice. Through an online experiment with 1486 subjects, we compare four widely used policy instruments in terms of their ability to enforce a norm of fairness in the Dictator Game, and to persist over time (i.e., to a subsequent untreated Dictator Game) or spill over to a norm of cooperation (i.e., to a subsequent Prisoner's Dilemma). As specific policy interventions, we employed two instances of *nudges*: defaults and social information; and two instances of *push* measures: rebates and a minimum donation rule. Our results show that (i) rebates, the minimum donation rule and social information have a positive direct effect on fairness, although the effect of social information is only marginally significant, and that (ii) the effect of rebates and the minimum donation rule persists in the second game, but only within the same game type. These findings demonstrate that, within our specific design, push measures are more effective than nudges in promoting fairness.



## 1. Introduction

Evaluation of policy instruments is widely recognized as a valuable tool to inform policy design. Among the variables that influence the evaluation of a policy intervention, the extent to which its effects persist over time and spill across contexts plays an important role. For example, a key question in the environmental economics literature is whether interventions that foster pro-social norms in the conservation of a specific environmental resource have positive or negative spillovers to other pro-environmental behaviors (Dolan and Galizzi, 2015; Truelove et al., 2014).

The sign and magnitude of policy spillovers may depend on the instrument used to induce behavioral changes. Here, we focus on two instances of *nudges*, defaults and social information, and two instances of *push* measures, rebates and a minimum donation rule. Previous research has investigated the effect of defaults (Johnson and Goldstein, 2003; Pichert and Katsikopoulos, 2008), social information (Bryan and Test, 1967; Reingen, 1978), rebates (Andreoni and Payne, 2011; List, 2011), and regulation (Cardenas, 2004; Falk and Kosfeld, 2006) in several contexts. Previous studies have also compared the effectiveness of pairs of policy instruments (Ito et al., 2015; Ferraro and Price, 2013; Ashraf et al., 2014). However, little is known about how treatment effects would carry forward and outside the specific area they were designed for (Dolan and Galizzi, 2015; Peysakhovich and Rand, 2016). To the best of our knowledge, this is the first study comparing all these interventions

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within the same design both in terms of their direct effect on a Dictator Game and in terms of their ability to influence subsequent behavior within the same decision context (i.e., other Dictator Game) and across contexts (i.e., Prisoner's Dilemma).

## 2. Experimental design and procedure

We implemented a large scale experiment articulated in two stages in Amazon Mechanical Turk (AMT), using a pool of US workers. Overall, we recruited 1486 participants: 738 as decision makers and 748 as recipients. From the sample of recipients we elicited beliefs on behavior of decision makers in the main experiment.[1]

Subjects received $0.50 as participation fee, in addition to the earnings from the tasks. They participated in a first stage DG (half in the role of dictators and half in the role of recipients), and subsequently played a second game, randomly chosen between another DG and a PD. Subjects playing a second DG kept the same role as in Stage 1.[2]

In the first stage, dictators had an endowment of $0.20 and were asked to decide how much of it (in steps of 2 cents) they wanted to give to the recipient, if any. The recipient had no choice and only got the amount that the dictator allocated to her.

After reading the general rules of the dictator game, all subjects were asked two comprehension questions about the donation maximizing their own and the other participant's payoff. Subjects failing the comprehension questions were automatically excluded from the survey. Those who passed the comprehension questions (about 73% of the total) were randomly assigned to one of four different policy interventions, in addition to the baseline case.

As specific policy interventions, we implemented two instances of nudges (default and social information) and two instances of push measures (rebate and minimum donation rule), all of which with a focal donation point at 50% of the endowment. In the *default* case, donations were pre-set at 50% of the endowment by simply pre-marking the button corresponding to a 50% donation. This is a mild intervention, since the player could select a different donation with very limited effort. Under the *social information* treatment, we informed subjects of the behavior of the subjects who participated in the pilot study: in particular, subjects were told that roughly half of the previous donations were equal or above 50% (this was the actual outcome, no deception was used). The *rebate* treatment rewarded dictators who gave at least 50% of their endowment, by returning half of the donation to them. Finally, a *minimum donation rule*, set at 50% of the endowment, meant that dictators were not allowed to proceed if their donations were strictly below 50%.

The second stage game was either a standard DG or a PD, without any policy intervention. We clarified that the second stage was independent from the first one and played with a different partner. The instruction screen of the second stage DG was identical to the one of the first stage DG, thus no additional comprehension questions were asked. Subjects playing the PD in the second stage had an endowment of $0.10, and were asked how much of it, if any, they wanted to give to the other person (in steps of 1 cent). The amount given would be multiplied by 2 and earned by the other person. Since the Prisoner's Dilemma is different from the Dictator Game, subjects assigned to playing it were asked another set of comprehension questions (success rate of 70%).

While the DG is used to measure individuals' altruistic tendencies (Brañas-Garza, 2006, 2007; Charness and Gneezy, 2008; Engel, 2011; Rand et al., 2016), the PD is used to measure individuals' cooperative tendencies (Nowak, 2006; Perc and Szolnoki, 2010; Capraro, 2013; Rand and Nowak, 2013). Recent experiments have shown that Dictators' allocations are positively correlated with real life altruism in a number of situations (Franzen and Pointner, 2013; Peysakhovich et al., 2014). The PD is a reliable measure of cooperative tendencies (Englmaier and Gebhardt, 2016). Although positively correlated, altruism and cooperation are two different types of behaviors: Capraro et al. (2014) have shown that people who give in the DG also cooperate in the PD, but not the converse.[3]

All subjects participating in the main experiment completed a demographic questionnaire at the end of the second stage.

Table 1 summarizes our treatments, and displays the number of participants for each of them. We adopted a series of checks on IP addresses and Turk IDs to ensure that no subject took the experiment more than once. The sample size in the second stage PD is smaller than in the second stage DG, because of the 30% of subjects assigned to this treatment who failed the additional comprehension questions on the PD (same success rate of the common set of questions). However, this differential attrition has not led to statistically significant differences between the two subsamples along observable characteristics, as shown in Table 1 in the Supplementary Material.

**Table 1**
List of treatments and number of participants per treatment.

| | Stage 1 | Stage 2 | |
|---|---|---|---|
| | DG | DG | PD |
| *Treatment* | | | |
| Baseline | 130 | 80 | 50 |
| Default | 162 | 96 | 66 |
| Social information | 153 | 92 | 61 |
| Rebate | 138 | 81 | 57 |
| Min. donation rule | 155 | 93 | 62 |
| **Total** | 738 | 442 | 296 |

For DG games, we recruited an equal number of subjects to act as receivers. Subjects assigned to the role of receivers in the main experiment faced two belief elicitation tasks. In each treatment, subjects were grouped with two other participants, person A (a dictator) and person B (a recipient). They were shown the screenshots of the instructions received by person A, and were asked to guess person A's donation to Person B. Stage 1 treatments mirrored the ones facing the dictators. Correct guesses were incentivized with a $0.20 reward. This design allows us to elicit recipients' beliefs (Capraro and Kuilder, 2016) and to observe if beliefs on DG donation in stage 1 and 2 varied between subjects depending on how giving was encouraged in the first stage game.

## 3. Results

### 3.1. Direct effects

Column 1 of Table 2 summarizes the direct effects of policy interventions on Stage 1 behavior. Average giving in the first stage DG is 26.7% of the total pie in the baseline treatment, in line with Engel's (2011) meta-analysis of 616 dictator games (28.3%). Also the type of the distribution is in line with Engel's meta-analysis, being, in both cases, a bimodal distribution with one mode at giving nothing and one mode at giving half. Looking at the treatments, average donations are 28.2% of the total pie in the default case, 32.3% in the social information treatment, 46.1% under rebate,

[1] We also conducted a pilot study with 564 participants, consisting in a single DG with two treatments, differing in the size of the participation fee ($0.50 and $0.70). Pilot data are used in the social information treatment and to check for the presence of income effects in DG donations (see Section 3).

[2] Full experimental instructions are reported in the Appendix A.

[3] To confirm this claim, we compare the distribution of decisions in the two games in our sample and find them to be significantly different (Epps-Singleton, $p < .0000$).