Short- and Long-run Effects of External Interventions on Trust.

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Abstract

We experimentally analyze the effects of external interventions such as subsidy and targeting on investment decisions, during the intervention and after. We employ a multi-period version of the trust (investment) game (Berg et al., 1995) introducing either monetary incentives for contribution or providing a suggestion about the level of investment. The results of the experiment indicate that targeting is an effective instrument to promote trustful behavior, whereas subsidy policy is not effective both in the short- and long-run. Therefore we suggest considering a targeting policy as one of the instruments that can foster trustful behavior.

Keywords: Trust Game; Experiment; Policy; Subsidy; Academic Spin-offs

JEL Classification: C92, L50, D80

1. Introduction

In 1998, Stanford University licenses the *PageRank* patent to one of its newly established spin-off companies. This investment initiates the growth of one of the worlds' largest high-tech company *Google* that soon revolutionizes the world markets. Besides public economic impact, this investment brings private financial benefits to Stanford that in large extent include voluntary financing of research scholarships and common projects.[1]

The success of Google explains why governments often intervene aiming to foster academic spin-off creation and knowledge commercialization. Typically,

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[1] For instance, in 2008, Google paid approximately $1,881,400 to Stanford University out of which only $426,950 payments related to the license of patents. The largest part of the payments - about $1,246,000 - was donations for scholarships and other philanthropic endeavors (Wikinvest.com, 2009).

such intervention takes the form of a subsidy policy that comprises two phases: First, a university receives a subsidy if it invests in the spin-off; Secondly, the successful spin-off gains additional finances from the government.[2]

Alternative forms of policy such as targeting are rarely considered, though they may not involve subsidy spending. Moreover, since the policy makers are often focused on immediate consequences of the interventions, the long-term, post-intervention potential costs are not taken into account. We attempt to fill this gap using a controlled laboratory experiment that allows making a direct comparison of different policies' efficiency in the short- and in the long-run.

In this experiment, we analyze the effects of external interventions such as subsidy and targeting on the investment decision during the intervention and after. We employ a multi-period version of the trust (investment) game (Berg et al., 1995) introducing either the monetary incentives for contribution or providing a suggestion about the level of investment. The experiment consists of three blocks with policy intervention in the second one that let us assess immediate as well as post-intervention effects.

In the non-monetary intervention, we exploit experimenter demand effect in line with previous research on the effect of "tax frame" (Cadsby et al., 2006; Karakostas and Zizzo, 2016; Pelligra et al., 2016; Silverman et al., 2014). In the context of our study this approach has considerably higher external validity rather than, for instance, giving authority to a subject, since we aim to model government policy rather than peer pressure effect. It also increases internal validity of the experiment because we can exogenously set up level of suggestion.

In this respect, Pelligra et al. (2016) come closest to our suggestion treatment part of the experiment. However, they study how the experimenter request affects the trustworthiness and find that it systematically decreases trustworthiness. Similarly, Silverman et al. (2014) exploit suggestion but in a public good game, Cadsby et al. (2006) ask to act with respect to *expectations* instead of suggestion, and Karakostas and Zizzo (2016) use requests to induce anti-social behavior.

With respect to monetary interventions, in the paper most closely related to subsidy treatment part of our study Charness et al. (2008) show that a cooperative behavior in trust game increases if a third-party (third player) can simultaneously reward trustor and punish trustee. Naturally, the study raises concern if effect is driven by punishment, reward, monitoring or expectations of reward and punishment.

Fiedler and Haruvy (2017) try to address these issues by providing some evidence that the effects of third player monitoring, reward, and punishment on cooperative behavior are comparable. However, the reward (punishment) rules are undefined in their experiment. Thus, their data barely allow knowing if the behavior changes due to the expectations (threat) of reward (punishment), beliefs about the expected level of investment, or reaction on incentives.

[2] See, for example, programs such as "Small Business Technology Transfer" (SBTT) in the United States and "Existenzgründungen aus der Wissenschaft" (EXIST) in Germany.

We take a different path by exogenously introducing a subsidy and varying the thresholds to get this subsidy (setting up it either above or below average trust level without intervention). Hence, we directly study the effect of subsidies on trust and trustworthiness. This is not only interesting from the theoretical point of view but important from a policy perspective: Subsidy policies rely on defined (written) rules.

The study offers four main original contributions: We are the first who analyze the effect of a non-monetary intervention in form of third-party suggestion on *trust*; Second, we compare the effect of a non-monetary policy to monetary ones and explain the difference in their performance; Third, we provide an analysis of the long-run effects of external interventions on trustful behavior. Finally, to the best of our knowledge, no paper has analyzed the rule-based effect of third-party monetary reward on *trust*.

We aim to answer the next four questions: (1) Does non-monetary intervention such as suggestion increase investment activity during and after it is introduced? (2) Is subsidy policy an efficient mean to foster investment activity in the short-run? (3) Is a low level of investment required to receive a subsidy detrimental for an investment? (4) Does the subsidy policy have a negative impact on investment level after the policy termination?

We find that non-monetary policy in form of suggestion increases investment activity during the intervention and we do not find any detrimental effects afterward. Subsidy policy, instead, does not significantly affect the level of trust, the amount returned, or trustworthiness in the short- or in the long-run. We associate the ineffectiveness of subsidy policy with two regularities: Subjects show low propensity to follow this policy and if subjects follow it, they send mostly the lowest amount required to get the subsidy.

We also find indirect evidence that the monetary policy is ineffective not because of the presence of the subsidy itself, but rather from the fact that the monetary reward is conditioned on a certain behavior: Subjects that unconditionally receive subsidies do not show a significantly different level of trustworthiness. We conclude that targeting policy should be considered as an effective tool to foster investment activity, in other words, to nudge higher investment level.

The rest of the paper proceeds as follows: Section two provides a short review of further relevant literature. Section three describes the theoretical framework and the hypotheses. Section four presents the experimental design. Section five provides the results of the experiment. Section six discusses the findings, followed by some final remarks.

2. Further Related Literature

The paper builds on four different strands of literature. First, it relates to studies on the interaction between intrinsic and extrinsic motivation. From the early research of Titmuss (1970) on blood donations to the experiment of Andreoni (1993) on public good provision, the studies point out the potential detrimental effects of external interventions on intrinsic motivation. For instance, in a meta-analysis of experimental studies on external incentives and intrinsic motivation, Deci et al. (1999) indicate the presence of negative effects that are particularly relevant in the case of tangible rewards.

Bowles and Polania-Reyes (2012), however, come to a different conclusion evaluating the results of fifty experiments on the relation between incentives and social preferences. They note that the effect of the incentives depends on the pre-existing social framework and can be both negative and positive. Gneezy et al. (2011) extend this discussion urging to consider both the potential long-term costs and benefits of external interventions.

The second strand of literature looks at the role that trust plays in investment decisions. Trust is involved in almost every economic transaction (Arrow, 1972) and, indeed, the empirical evidence suggests that the trust is crucial for venture capital investments (Bottazzi et al., 2011), mutual investment decisions (Felli et al., 2010) and has a positive association with the level of investment across countries (Knack and Keefer, 1997).

The trust (investment) game that we employ in the experiment mirrors the investment situation with imperfect contracts. The behavior in this game varies across countries with different economic characteristics (Johnson and Mislin, 2011). Moreover, the trustful behavior in this game correlates with the differences in investment propensity between countries – for instance, Germany and France (Willinger et al., 2003) or Gulf region and Western countries (Bohnet et al., 2010) – that make possible to better understand the variation in the investment rates across nations.

Third, this paper is closely related to the studies of the interaction between external incentives and trustful behavior. Fehr and List (2004) find that not used threat to punish increases trustworthiness, while the punishment crowds out trustworthy behavior. Furthermore, the threat of potential contract enforcement crowds in trustworthiness (Bohnet et al., 2001), but trustworthiness is decreasing when sanctioning is used (Fehr and Rockenbach, 2003). Houser et al. (2008) reconcile these findings by showing that the effect of sanctions depends on the relation between the requested amount and the level of sanctions, not intentions. In addition, Li et al. (2009) show that behavioral change under sanctions can be attributed to a "perception shift" towards more utility-based reasoning.

As concerns specifically subsidies and trustful behavior, additional compensation affects agents' exerted effort not monotonically (Gneezy and Rustichini, 2000) and, if the principal imposes a lower bound for the effort, agents mostly exerts effort at this bound (Falk and Kosfeld, 2006). Interestingly, Gachter et al. (2011) show that exerted effort increases both in the presence of a fine or a bonus, but under the bonus condition subjects tend to choose an effort not higher than a best-reply level condition.

Nevertheless, the effect of the incentives on principal action (trust) remains unclear. Charness et al. (2008) attempt to fill this gap by allowing a third-party (third player) both reward the principal and punish the agent. The experimental results corroborate the hypothesis that the threat of punishment increases trust and trustworthiness. However, the effect of reward on trust (principal's action) stays ambiguous. Fiedler and Haruvy (2017), though, provide experimental evidence that third player monitoring, punishment, or reward all has similar effect on trust and trustworthiness.

Fourth, the effect of non-monetary incentives on trust, in the seminal paper Berg et al. (1995) provide evidence that an aggregated information about previous behavior – information about the average amount sent, returned and net return by other subjects – can strengthen trustful relations. Similarly, Thöni and Gächter (2015) show that peer effects have a significant influence on the trust level and suggest conformism as an explanation of this phenomenon.

Bracht and Feltovich (2009) provide additional evidence that the information about the previous actions of others can enhance cooperation, but reporting that cheap-talk message has little effect. However, Charness and Dufwenberg (2006) show that promise increase cooperation in the one-shot trust game.[3] Schotter and Sopher (2006) show that inter-generational advice decreases the level of trust, but increases trustworthiness in one-shot (per generation) trust game. Finally, Pelligra et al. (2016) directly exploits the effect of experimenter request on trustworthiness and show that request gives a"wiggle room" and, thus, decrease trustworthiness.

3. Theory and Implications

3.1. The Game

We use a version of the trust (investment) game. In the original trust game (Berg et al., 1995), two players interact with each other: player 1 (the trustor) decides which amount of his initial endowment E to send (to give) to player 2 (the trustee). The amount sent s is multiplied by a certain factor m and player 2 receives the multiplied sum. Player 2, in turn, chooses how much to return R of the amount received. See Figure 1 for the structure of the game and a description of the payoffs π of players 1 and 2.

In our version of the investment game, an external intervention is introduced. This intervention is devised alternatively as either a subsidy or a suggestion. The subsidy Z is obtained by both players if the contribution of player 1 is greater than or equal to a certain threshold T (figure 2 describes this version of the game). In the case of suggestion, no subsidy is available but it is suggested to send not less than a threshold level.

The game is played for several periods and consists of three blocks. Blocks 1 and 3 consist of repetitions of the standard trust game, while in the block 2 the interventions are introduced.

In what follows, we outline a simple model to develop the theoretical predictions and hypotheses.

[3] In addition, Duffy and Feltovich (2010) find that the recommendation by third-party affects subjects behavior in the two-player game of Chicken.

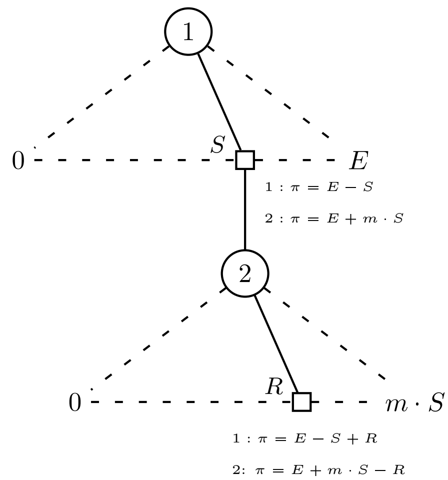

Figure 1: Trust (investment) game.

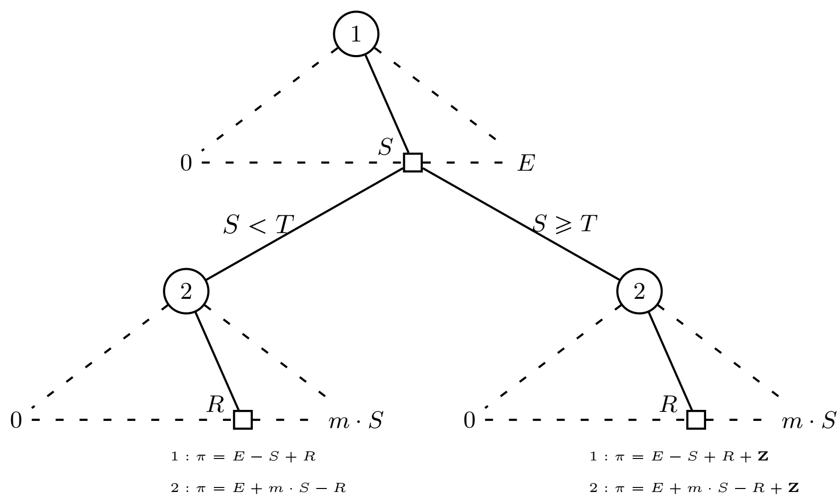

Figure 2: Trust (investment) game with subsidies.

3.2. Trust under External Incentives

To derive the theoretical predictions, we apply backward induction solving the model from the second stage. We denote by v the value that trustor expects to receive back in the second stage of the game. This value is a function of the amount sent s. Thus, the utility function of the trustor takes the form:

$$u = E - c(s) + v(s) + o(s) + I, \tag{1}$$

where E is the player's endowment, c is the individual's cost of sending an amount s, o is the trustor's other-regarding preferences that depend on s, I is the effect of external incentives that can take the form of either a subsidy or a suggestion.

Let's begin the analysis with the subsidy policy. The subsidy policy is characterized by a tuple of parameters (Z, T), indicating the size of the subsidy and the threshold (minimal) amount that the player must send to obtain this subsidy, respectively.

The subsidy offsets the costs of sending but can affect other-regarding preferences as well. We assume that the other-regarding preferences are affected by a measure $\lambda < 0$.[4] Thus, the utility function in the presence of a subsidy policy is

$$u = E - c(s) + v(s) + o(s) + 1_{\{s \geqslant T\}}[Z + \lambda o(s)], \tag{2}$$

where the indicator $1_{\{s \geqslant T\}} = 1$ if $s \geqslant T$ and zero otherwise.

The players maximize their utility so that the marginal costs of sending are equal to the marginal benefits (the values are expressed in discrete terms to account for the discontinuity in $s = T$):

$$\frac{\Delta c(s)}{\Delta s} = \frac{\Delta v(s)}{\Delta s} + \frac{\Delta o(s)}{\Delta s} + \frac{\Delta 1_{\{s \geqslant T\}}[Z + \lambda o(s)]}{\Delta s}, \tag{3}$$

To analyze the effect of a subsidy policy, we compare it to the case where there are no incentives. The subsidy is contingent on the relation between threshold and amount sent. We, therefore, consider two states (1) when the amount to be sent without incentives s_0 is lower than the threshold and (2) when it is higher. We then obtain the following two relations:

$$\frac{\Delta c(s^*)}{\Delta s} = \begin{cases} \frac{\Delta v(s)}{\Delta s} + \frac{\Delta o(s)}{\Delta s} + \frac{Z}{\Delta s} + \frac{\lambda \Delta o(s)}{\Delta s} & \text{if } s_0 < T \\ \frac{\Delta v(s)}{\Delta s} + \frac{\Delta o(s)}{\Delta s} + \frac{\lambda \Delta o(s)}{\Delta s} & \text{if } s_0 \geqslant T \end{cases} \tag{4}$$

One can easily see that it is beneficial to send more whenever the amount to be sent without incentives s_0 is lower than the threshold T and the direct effect of the subsidy $\frac{Z}{\Delta s}$ is larger than the crowding out effect of the subsidy $\frac{\lambda \Delta o(s)}{\Delta s}$. However, if $s_0 < T$, there is no direct subsidy effect (the subsidy is independent from additional sending, $\frac{Z}{\Delta s} = 0$), whereas the negative effect of the subsidy on other-regarding preferences is still present, $\frac{\lambda \Delta o(s)}{\Delta s} < 0$. We are therefore able to formulate the following two hypotheses:

H 1. *The amount sent is higher under external monetary incentives than without them if (1) the threshold level is higher than the amount sent in case without the incentives* $s_0 < T$ *and (2) the direct effect of the subsidy is larger than the crowding out effect* $\frac{Z}{\Delta s} + \frac{\lambda \Delta o(s)}{\Delta s} > 0$.

[4] We make this assumption in line with previous experimental results. See Bowles and Polania-Reyes (2012) for a discussion.

H 2. *The amount sent is lower under external monetary incentives than without them if the threshold level is higher than the amount sent in case without the incentives* $s_0 < T$.

Concerning the targeting policy (suggestion), this policy is also characterized by a threshold level T (the suggested minimal level to be sent). The policy does not use subsidy but players can get an utility complying with authority (Karakostas and Zizzo, 2012).[5] We denote this utility by A (that is independent from s). Thus, the senders' utility is

$$u = E - c(s) + v(s) + o(s) + 1_{\{s \geqslant T\}}(A), \tag{5}$$

Analyzing the players' utility function in the case of targeting policy in the same way as in 3 and 4, we obtain the next relations:

$$\frac{\Delta c(s^*)}{\Delta s} = \begin{cases} \frac{\Delta v(s)}{\Delta s} + \frac{\Delta o(s)}{\Delta s} + \frac{A}{\Delta s} & \text{if } s_0 < T \\ \frac{\Delta v(s)}{\Delta s} + \frac{\Delta o(s)}{\Delta s} & \text{if } s_0 \geqslant T \end{cases} \tag{6}$$

If the amount sent in case without the incentives is lower than the threshold $s_0 < T$ the players benefit by complying with authority. Therefore, they can sacrifice part of their endowment to follow the suggestion. Nevertheless, they do not benefit when $s_0 > T$ since the utility is independent from the amount sent.

H 3. *The amount sent is higher under external non-monetary incentives than without them if the threshold level is higher than the amount sent in the case without the incentives* $s_0 < T$.

Considering the long-run (post-intervention) effect of incentives, we assume that preferences are endogenous (Bowles, 1998), meaning that the preferences learned under certain circumstances stay present afterwards. Given this, we can derive from 4 the following relations for the period after the subsidy policy:

$$\frac{\Delta c(s^*)}{\Delta s} = \begin{cases} \frac{\Delta v(s)}{\Delta s} + \frac{\Delta o(s)}{\Delta s} + \frac{\lambda \Delta o(s)}{\Delta s} & \text{if } s_0 < T \\ \frac{\Delta v(s)}{\Delta s} + \frac{\Delta o(s)}{\Delta s} + \frac{\lambda \Delta o(s)}{\Delta s} & \text{if } s_0 \geqslant T \end{cases} \tag{7}$$

There is no direct effect of the subsidy Z since the subsidy policy is absent now. However, other-regarding preferences are still negatively affected $\frac{\lambda \Delta o(s)}{\Delta s} < 0$. Thus, we formulate:

H 4. *The amount sent is lower after experiencing external monetary incentives than without them.*

[5] In Karakostas and Zizzo (2012), the information communicated by a third-party affects the behavior of subjects. They attribute this effect to compliance to authority. We suppose that the suggestion have a similar effect.

In a similar vein, we derive from 5 the next relations for the period after the targeting policy:

$$\frac{\Delta c(s^*)}{\Delta s} = \begin{cases} \frac{\Delta v(s)}{\Delta s} + \frac{\Delta o(s)}{\Delta s} + \frac{A}{\Delta s} & \text{if } s_0 < T \\ \frac{\Delta v(s)}{\Delta s} + \frac{\Delta o(s)}{\Delta s} & \text{if } s_0 \geqslant T \end{cases} \tag{8}$$

When the threshold level is higher than the amount sent in case without the incentives $s_0 < T$, the players send more after the targeting policy since they continue to gain utility complying to the authority $\frac{A}{\Delta s} > 0$.

H 5. *The amount sent is higher after experiencing external non-monetary incentives than without them if the threshold level is higher than the amount sent in case without the incentives $s_0 < T$.*

3.3. Trustworthiness under External Incentives

We represent the utility function of the trustee in the following way:

$$u = 1 - c(r) + o(r) + I, \tag{9}$$

where $c(r)$ is the trustee's cost of returning the ratio $r = \frac{R}{m \cdot s}$, o is the other regarding preferences that changes with r[6], I is the effect of external intervention (subsidy or suggestion).

We assume that trustees maximize their utility. Since external intervention depends on the behavior of trustor but not on trustee's choice we obtain the following relation:

$$\frac{\partial c(r^*)}{\partial r} = \frac{\partial o(r)}{\partial r}, \tag{10}$$

We know from previous studies (Johnson and Mislin, 2011) that $\frac{\partial c(r^*)}{\partial r \partial s} = \frac{\partial o(r)}{\partial r \partial s} > 0$. Therefore, we can formulate the following hypothesis:

H 6. *The trustworthiness rate r is not different during and after the external intervention as compared to the case without it when conditioned on the amount sent by the trustor s.*

4. Experimental Design

The experiment was conducted at the laboratory of the Max Planck Institute of Economics in Jena (Germany) in April 2013. Seven sessions were run, each of them lasting about 60 minutes and employing 32 experimental subjects. Experimental subjects were recruited using the ORSEE system (Greiner, 2004), and the experiment was programmed and implemented with the help of z-Tree software (Fischbacher, 2007).

[6] We assume that o is independent from Z since (1) subsidy is provided by a third-party and (2) both players receive it.

In the experiment, subjects play various versions of the trust game for 30 periods. In each period, they have an endowment of 100 points, $E = 100$, and the sum that they send is tripled, $m = 3$. The experiment is subdivided into three blocks of 10 periods each. The first and the third blocks are the same for all subjects – they face the standard trust game. However, in the second block, subjects play different versions of the trust game depending on the treatment to which they are randomly assigned: SUBLOW, SUBHIGH, SUGGEST, CONTROL.

In the second block of the SUBLOW treatment, subjects can gain a subsidy of 20 points, $Z = 20$, if the amount sent by the trustor exceeds a (low) threshold of 30, $T = 30$. See the game flow for the SUBLOW treatment in Figure 3.

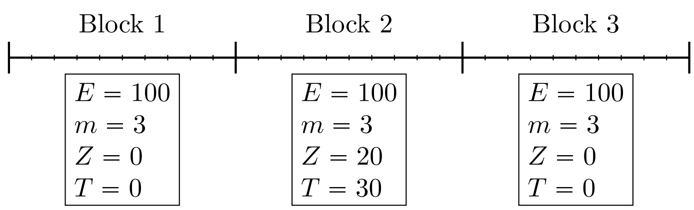

Figure 3: SUBLOW treatment parameters.

The SUBHIGH treatment differs from the SUBLOW treatment only in the threshold level: To gain the subsidy the trustor needs to send not less then 70, $T = 70$. See Figure 4.

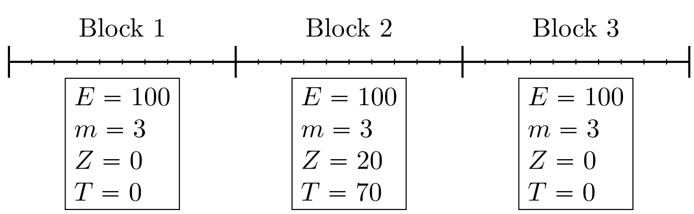

Figure 4: SUBHIGH treatment parameters.

In the SUGGEST treatment – the case of targeting policy – the subsidy is absent in all blocks, but in block 2 it is suggested by the experimenter to send not less then 70, so $T = 70$ (like in SUBHIGH treatment). See Figure 5.

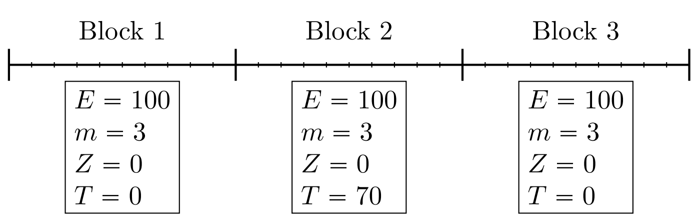

Figure 5: SUGGEST treatment parameters.

In the CONTROL treatment, the standard trust game, without any subsidies or suggestions, is played for all three blocks. See Figure 6.

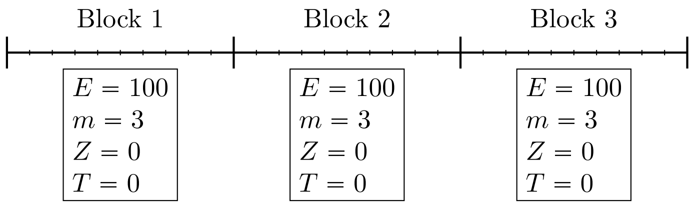

Figure 6: CONTROL treatment parameters.

We run all four treatments within same session to control for the session specific effects. Subjects are randomly assigned to the treatment and to the role of trustor or trustee. They keep their role throughout the whole experiment and are randomly matched with the other players from the same treatment in each period of the experiment (stranger matching design).[7] We keep the roles constant and use stranger matching because this design represents in our view a situation of repeated but independent decisions of the university to engage in spin-off activities.

The subjects privately receive payments at the end of the experiment according to the points they gained in one randomly selected period of the game.[8] Points are converted to Euros at the rate of 10 points for €0.35. Including a participation fee of €2.50, the subjects earned on average €6.81 with minimum €2.5 and maximum €15.5.

Table 1: Participants characteristics

Statistic	N	Mean	St. Dev.	Min	Max
Age	224	24.147	4.040	18	48
Share of Females (♀)	224	0.491	0.501	0	1
Exp. Interesting	224	2.536	1.249	1	5
Exp. Length	224	2.304	0.871	1	5
Exp. Understandable	224	4.143	1.174	1	5
Task difficulty	224	2.268	1.556	1	8

Table 1 summarizes the descriptive data about the subjects and their perception of the experiment obtained through the questionnaire given at the end of

[7] Though the order of matching is random, it is identical in all four treatments within the same session. That allows us to reduce the potential effects resulting from the history of the interaction.

[8] We use this scheme to avoid the endowment effect. See Azrieli et al. (2012) for the analysis of incentive schemes in experiments.

each experimental session. We almost perfectly balanced the sample on gender across the experiment (ratio of female participants: 0.49) and across sessions (ratio of female participants per session: 0.47, 0.5, 0.5, 0.47, 0.47, 0.53, 0.5). Also, we covered a wide range of age groups from 18 to 48 though most of the participants are relatively young (median age: 23.5).

As concerns the complexity of the experiment, subjects report a fairly high understanding of instructions with average value of 4.14 on a scale from 1 to 5 and the task difficulty as low, with mean 2.27 on a scale from 1 to 10.

5. Results

5.1. Descriptive Analysis of Trust, Amount Returned, and Trustworthiness.

To assess subject's behavior, we first compare the average amount sent in each round across the treatments. Figure 7a plots the average amounts sent over the game. The average amount sent across all the treatments in block 1 is similar to what other studies find[9] and equals to 40.24. From visual inspection, no evident difference in trust level shows up in block 1 across the four treatments. This is to be expected since subjects play the same standard trust game in all four treatments.

Now let's consider the behavior during the policy intervention, the block 2. It is clear that subjects send more on average in the treatment SUGGEST than in any other treatment. The suggestion effect is especially strong in the first period of the intervention: The average amount sent in in the first period of the intervention tends to the suggested level of 70 points, $\bar{s}_{SG} = 68.46$, which is far above average amount sent in control group, $\bar{s}_{CL} = 46.61$ (Standardized Effect Size, Cohen's d_{11}=0.581). The difference stays positive during the intervention, however, it decays reaching a level comparable to the control group in the last period of the intervention ($\bar{s}_{SG} = 46.04$ vs. $\bar{s}_{CL} = 35.82$; Standartized Effect Size, Cohen's d_{20}=0.239). Thus, the suggestion policy seems to be very effective but its effect is decreasing in time. We will take into account this fact in the empirical analysis (see section 5.2)

One can also observe inthat the curve of the average amount sent in the treatment with suggestion is always above the similar curve for the treatments with subsidy during the intervention. However, the plot does not show a difference between the amount sent in treatments with subsidy and the control treatment.

An interesting pattern emerges after the policy intervention. In block 3 the average amount sent in the SUGGEST treatment continues to exceed the corresponding value in the CONTROL treatment until the last periods of the game. On the contrary, the amount sent in the SUBHIGH treatment is lower than for CONTROL. The average sending in SUBLOW treatment is similar to the corresponding value in CONTROL treatment.

[9]See Johnson and Mislin (2011) for a meta-analysis of experiments based on the trust games.

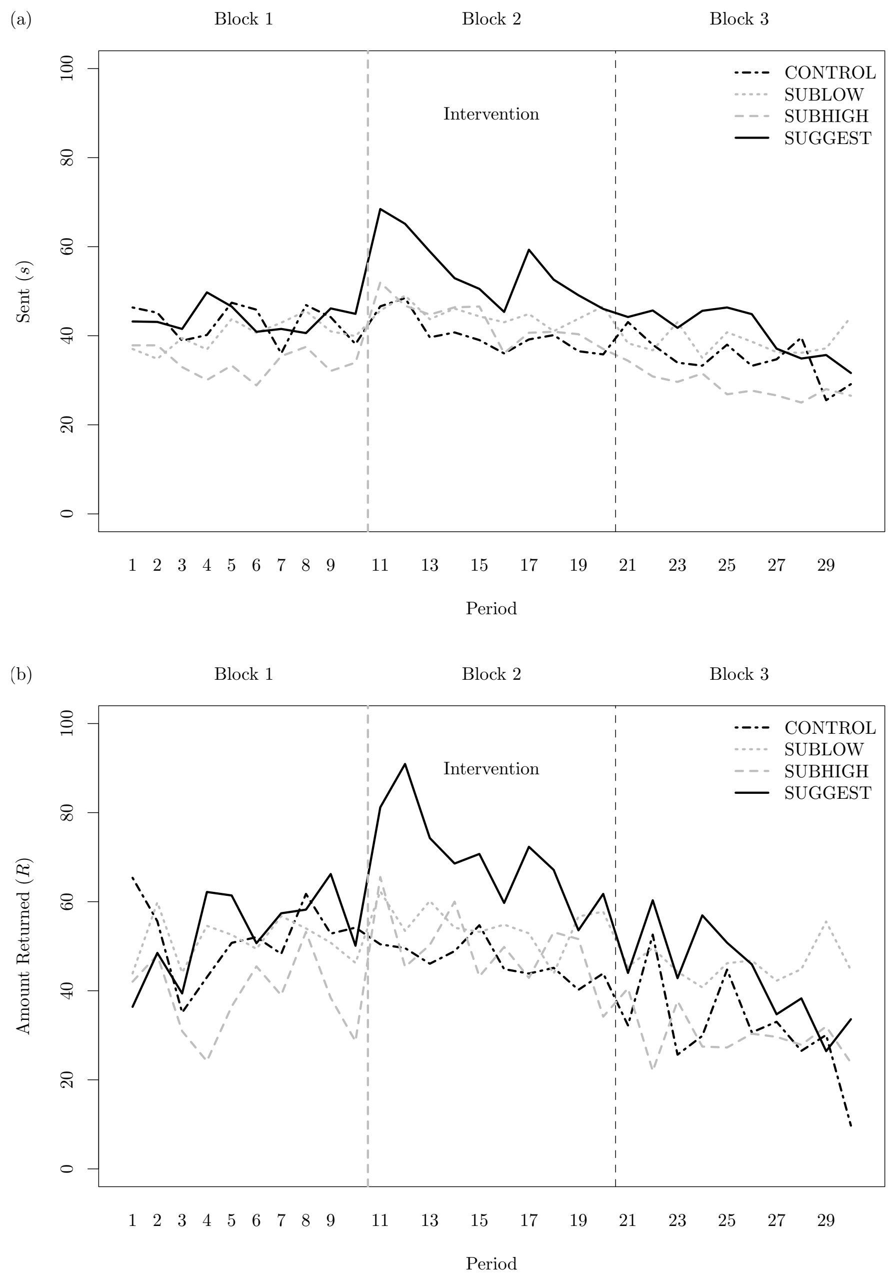

Figure 7: Average amount sent (a) and amount returned (b) by treatment.

To have a more clear picture of the difference between the treatments, we plot the cumulative distribution functions (CDF) for each of the three blocks (see Figure 8). The CDFs indicate the proportion of cases where the amount sent is smaller than a certain value, allowing us to have a detailed view of the distribution of the amount sent.

Again, we do not see any substantial difference between treatments in block 1 but we observe a very different shape of the distributions in block 2. One can easily identify discontinuities in correspondence to the values of the low threshold ($T = 30$) for the treatment SUBLOW, the high threshold ($T = 70$) for the treatment SUBHIGH and the suggested amount to be sent ($T = 70$) for the treatment SUGGEST. Indeed, we observe changes related to the policy intervention.

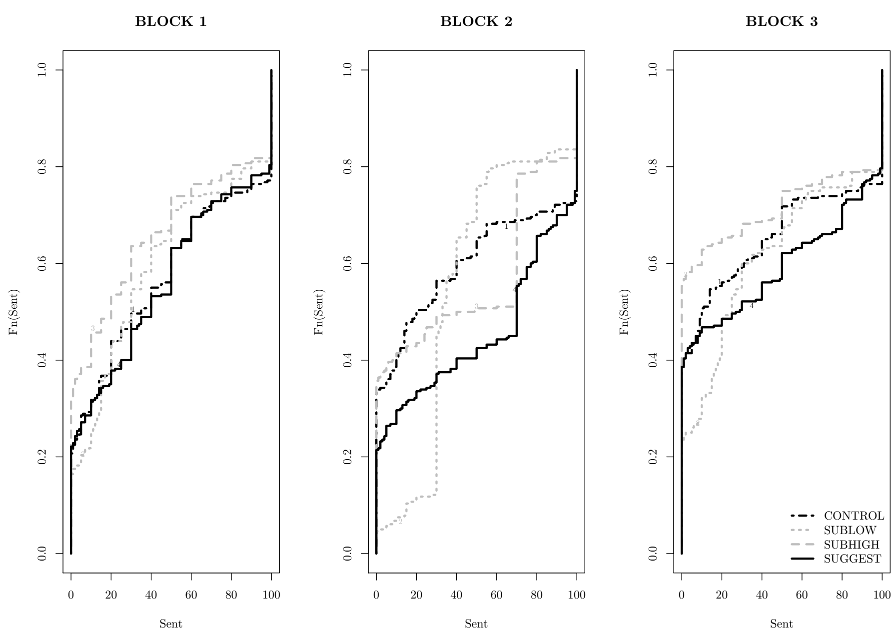

Figure 8: Cumulative distribution of amount sent by treatment.

Interestingly, we see very different distributions of the amount sent for the SUBHIGH and SUGGEST treatments if we look at the values that exceed 70 (the high threshold level or the suggested amount to send). While in the SUGGEST treatment subjects do not simply send the minimal level suggested, but continue to send higher values as well, in the SUBHIGH treatment almost no one provides contributions that are higher than that required for the subsidy. This pattern can be potentially explained by a crowding out effect and we will discuss it in more details in section 5.4.

As concerns block 3, one can observe that the curve of the cumulative distribution function for SUBHIGH treatment lies above the one of the CONTROL treatment and, on the contrary, the curve for SUGGEST treatment is below the one of CONTROL treatment.

We conclude the descriptive analysis by discussing how much player 2 (trustee) sends back (see Figure 7b). We see that the average amount returned mirrors the amount sent across all treatments. More importanly, it reflects the amount sent during the intervention as well. That is, under the subsidy policy trustees do not decrease return level and under suggestion policy subjects return a larger amount in correspondence with amount they receive. Trustees continue to reciprocate trust despite external intervention.

To see if the rate of return proportionally correspondence to amount sent, we calculate the ratio between the amount sent back by player 2 and the amount received by the same player - the trustworthiness rate, $r = \frac{R}{3 \cdot s}$. As expected, we do not observe any difference in trustworthiness between treatments (see Figure B.9 in Appendix B.2). The stability of trustworthiness across the treatments makes it possible to focus on the aim of our study, the analysis of the effects of external interventions on trust and overall efficiency of the policy.

5.2. Regression Analysis of Trust, Amount Returned, and Trustworthiness

To assess significance of our results, we provide a regression analysis using a mixed effects model with random effect for individual subject. We split the analysis by five periods to capture dynamic effects (see previous subsection). We estimate the difference in amount sent (trust level) across treatments by running the following regression:[10]

$$s = \beta_0 + \beta_{SG} SUGGEST + \beta_{SL} SUBLOW + \beta_{SH} SUBHIGH + \upsilon_i + \epsilon_{i,t}, \qquad (11)$$

where $SUGGEST$, $SUBLOW$, $SUBHIGH$ are dummy variables that are equal to 1 for the corresponding treatments. υ_i is the random effect for subject i and $\epsilon_{i,t}$ is the error term for subject i in period t. The results are reported in Table 2.

In line with expectations and the observed pattern in Figure 7 we do not find a significant difference at any conventional level in the first ten periods. The behavior should not differ since there is no intervention in the first ten periods (block 1).

Now let's consider the effect of the intervention. We observe that subjects send significantly more in the SUGGEST treatment than in the CONTROL treatment during the first 5 periods of block 2 ($p = 0.064$; $\beta_{SG} = 16.3$). During the next 5 periods of block 2 this difference remains positive, however, it is no longer significant ($p = 0.184$; $\beta_{SG} = 12.943$). To understand the dynamic effect of suggestion policy we take a closer look at the first five periods of intervention. Namely, we run non-parametric exact Wilcoxon test across aggregated averages

[10]As robustness check, we also estimate linear regression with robust standard errors clustered on subjects (for all linear mixed-effects specifications). The results holds if use this specification as well.

Table 2: Determinants of Sending by five periods – estimation of equation 11

	Sent (s)					
	Periods					
	1-5	5-10	11-15	16-20	21-25	26-30
SUGGEST	1.2	0.6	16.3*	12.9	7.5	4.4
	(8.4)	(10.1)	(8.7)	(9.7)	(10.2)	(10.1)
SUBHIGH	−9.2	−8.7	4.4	1.5	−6.6	−5.7
	(8.4)	(10.1)	(8.8)	(9.7)	(10.2)	(10.0)
SUBLOW	−5.2	−0.2	2.9	6.3	1.5	6.1
	(8.4)	(10.1)	(8.6)	(9.6)	(10.1)	(10.0)
Constant	43.6***	42.2***	42.9***	37.5***	37.3***	32.5***
	(5.9)	(7.2)	(6.2)	(6.8)	(7.2)	(7.2)
Observations	560	560	560	560	560	560
Akaike Inf. Crit.	5,206.9	5,068.8	5,219.4	5,094.5	5,059.2	5,208.0
Bayesian Inf. Crit.	5,245.8	5,107.7	5,258.3	5,133.4	5,098.1	5,246.9

Note: Robust standard errors are in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

of the amount sent over the sessions in the SUGGEST and CONTROL treatment for each of those periods (Period 11-15).[11] We see that in the first period of intervention the suggestion policy has a large statistically significant effect (Period 11: $p = 0.041, r = 0.547$), but then this effect gradually fades away (Period 12: $p = 0.09; r = 0.453$; 13: $p = 0.157; r = 0.378$; 14: $p = 0.402; r = 0.224$; 15: $p = 0.365; r = 0.242$).[12] Thus, we conclude that the targeting policy reaches its goal and positively affects the level of sending though only in the short-run.

As concerns the subsidy-policy, its effect is less evident. We cannot reject the null-hypothesis that the average amount sent in the treatments with subsidies is the same as the average amount sent in the control treatment neither in the first five periods of block 2 (SUBHIGH: $p = 0.738$; $\beta_{SH} = 2.871$; SUBLOW: $p = 0.618$; $\beta_{SL} = 4.386$) nor for the next five periods (SUBHIGH: $p = 0.512$; $\beta_{SH} = 6.336$; SUBLOW:$p = 0.88$; $\beta_{SL} = 1.471$). If we compare amount sent in subsidy treatments with the control treatment, applying non-parametric Wilcoxon test across aggregated averages over the sessions to each period, we do not see that this policy is effective in the short run as well.[13] Put it differently, we do not find an evidence that subsidy policy is an effective mean to promote trustful behavior in the short-run.

In the last ten periods of the game, we do not find any significant post-

[11] We estimate the exact Wilcoxon test based on the Shift Algorithm by Streitberg and Röhmel (1986) throughout the paper.

[12] If we test the difference between SUGGEST and CONTROL for the next periods (Periods 16-20) using the same Wilcoxon test, the effect always stays positive but we can not reject the null-hypothesis at any conventional level of significance for any of those periods.

[13] SUBHIGH VS. CONTROL, Period 11: $p = 0.644, r = 0.124; 12 : p = 0.931; r = 0.023$; 13: $p = 0.71; r = 0.099$; 14: $p = 0.533; r = 0.167$; 15: $p = 0.513; r = 0.175$. SUBHLOW VS. CONTROL, Period 11: $p = 0.597, r = 0.141$; 12: $p = 1, r = 0$; 13: $p = 0.71; r = 0.099$; 14: $p = 0.646, r = 0.123$; 15: $p = 0.692; r = 0.106$

intervention effects. The amount sent in the control treatment does not significantly differ from the one in any other treatment.[14] We observe, however, that the coefficient associated with the dummy for SUGGEST treatment is positive and larger than in the first periods of the game. This result suggests that there can be a long-lasting effect of the targeting policy, though a further investigation is necessary.

We conclude this section by analyzing the evolution of the amount returned R and trustworthiness rate r. At first, similarly to (11) we estimate the following regression for the amount returned:

$$R = \beta_0 + \beta^R_{SG} SUGGEST + \beta^R_{SL} SUBLOW + \beta^R_{SH} SUBHIGH + \upsilon_i + \epsilon_{i,t} \quad (12)$$

We do not control for the amount sent in the regression 12 to asses if the policies affect the amount returned under the intervention in absolute terms (the results are reported in table B.7 in Appendix B.1). Indeed, we see that amount returned is significantly higher in the SUGGEST treatment than in the CONTROL treatment during the first 5 periods of block 2 ($p = 0.048$; $\beta^R_{SG} = 27.2$). In the next 5 periods of block 2 this difference remains positive, but loose significance ($p = 0.146$; $\beta^R_{SG} = 19.307$). In subsidy treatments we do not find evidence that trustees change their behavior during the intervention.[15] Thus, trustees reciprocate trust in all treatments during the intervention and send more in absolute terms under suggestion policy.

Perhaps, trustees return a disproportional amount to what they receive e.g. subsidy or suggestion crowd-out trustworthiness, that can, in turn, decrease trustors contribution. To address this concern, we assess determinants of trustworthiness ($r = \frac{R}{3 \cdot s}$) using the next regression:[16]

$$r = \beta_0 + \beta^r_{SG} SUGGEST + \beta^r_{SL} SUBLOW + \beta^r_{SH} SUBHIGH + s + \upsilon_i + \epsilon_{i,t} \quad (13)$$

In line with the theoretical predictions we do not find a significant difference over the entire experiment in trustworthiness rate between CONTROL, SUBHIGH, and SUGGEST treatments (see table B.8 in Appendix B.2).[17] No difference in trustworthiness during the intervention provide evidence that the policy does not crowd out trustworthiness: Trustees return a proprtional amount

[14]We as well do not find any significant difference comparing each of the treatments to each other.

[15]The amount returned is significantly higher in the last five periods of the game for SUBLOW treatment ($p = 0.072$; $\beta^r_{SL} = 20.814$). This difference might be driven by subjects idiosyncratic characteristics. To avoid interpretation of potentially biased results in the SUBLOW treatment we focus on the CONTROL, SUBHIGH and SUGGEST treatments, though we report the analysis of subjects behavior in SUBLOW treatment as well.

[16]We control for amount sent (s) in regression 13 since not just amount returned, but trustworthiness (proportion returned) as well depends on amount sent (see meta-analysis of trust game by Johnson and Mislin, 2011). However, the results are robust if we do not include it (see Table B.9 in Appendix B.2)

[17]Trustworthiness is significantly different both during the first five periods ($p = 0.046$; $\beta^r_{SL} = 0.11$) and the last five periods of the game for the SUBLOW treatment ($p = 0.068$; $\beta^r_{SL} = 0.124$). This indicates that subject idiosyncratic characteristics drives this difference.

(reciprocate trust) under policy intervention as under control condition. It is especially interesting to see no difference in trustworthiness between the treatments with subsidy and control during the intervention period: The subjects that are exposed to subsidy still do not significantly change their behavior. This indirectly points out that unconditional subsidy does not produce crowding out effect.

To sum up, the suggestion (targeting) policy increases the amount sent in the short-run, but we do not find evidence that the subsidy policies alter the amount sent. Thus, given that trustees return a proportional amount to what they receive in all treatments, the suggestion policy seems to be an attractive tool from the social welfare perspective. We assess if the suggestion, indeed, increases social welfare in the next subsection.

5.3. Net Payoff

Now, we consider how the reaction on different policies is reflected in the variation of net payoffs. Specifically, we evaluate the effect of each policy on the average net payoff π_N, that is, the difference between the subject's payoff and the value of the subsidy (s)he gets: $\pi_N = \pi - Z$. We subtract the value of a subsidy to account for the costs of the third party since we focus on social welfare effect of the policy, but not individual benefit[18]. The following mixed-effect model is estimated:

$$\pi_N = \beta_0 + \beta^{\pi}_{SG} SUGGEST + \beta^{\pi}_{SL} SUBLOW + \beta^{\pi}_{SH} SUBHIGH + P + \upsilon_i + \epsilon_{i,t}, \quad (14)$$

where $SUGGEST$, $SUBLOW$, $SUBHIGH$ are dummy variables that are equal to 1 for the corresponding treatments. P is a dummy variable that is equal to 1 if the player is a trustor and 0 if the player is a trustee. υ_i is the random effect for subject i and $\epsilon_{i,t}$ is the error term for subject i in period t.

As expected we find a significant increase in net payoffs during the first five periods of targeting policy ($p = 0.048$;$\beta^{\pi}_{SG} = 16.3$) as well as during the next five ($p = 0.108$; $\beta^{\pi}_{SG} = 12.943$). On the contrary, we still do not find significant effect of subsidy policy: The subsidy policy is ineffective both during the first five periods of intervention (SUBHIGH: $p = 0.598$; $\beta^{\pi}_{SH} = 4.386$; SUBLOW: $p = 0.71$; $\beta^{\pi}_{SL} = 2.871$) and during the next five (SUBHIGH: $p = 0.854$; $\beta^{\pi}_{SH} = 1.471$; SUBLOW: $p = 0.395$; $\beta^{\pi}_{SL} = 6.336$). To shed light on the reasons of these results, we provide further analysis in the next subsection.

5.4. Crowding Out and Effect of Threshold

We wish to understand the potential cause of inefficiency of subsidy policy. To do that we focus on the distribution of the amount sent in treatments with different policy but with identical threshold level: SUBHIGH and SUGGEST.

[18]The effect of policies on gross payoff are reported in Table B.10 in Appendix B.3. One can see that even if do not account for third-party costs, the individual payoffs under subsidy policies are comparable to the payoffs under the suggestion intervention. However, under subsidy policies subject get this additional payoff simply by exploiting the subsidy.

Table 3: Determinants of Net Payoff (π_N) by five periods – estimation of equation 14

	Dependent variable:					
	Net Payoff (π_N)					
	1-5	5-10	11-15	16-20	21-25	26-30
SUGGEST	1.2	0.6	16.3**	12.9	7.5	4.4
	(6.8)	(6.9)	(8.2)	(8.0)	(8.6)	(8.8)
SUBHIGH	−5.2	−0.2	2.9	6.3	1.5	6.1
	(6.7)	(6.9)	(7.7)	(7.4)	(8.2)	(8.3)
SUBLOW	−9.2	−8.7	4.4	1.5	−6.6	−5.7
	(6.9)	(7.0)	(8.3)	(8.0)	(8.7)	(8.7)
Player (P)	−67.4***	−59.4***	−76.3***	−68.0***	−69.2***	−65.2***
	(4.8)	(4.9)	(5.6)	(5.4)	(6.0)	(5.9)
Constant	177.3***	171.9***	181.0***	171.5***	171.8***	165.0***
	(5.4)	(5.5)	(6.4)	(6.3)	(6.8)	(7.0)
Observations	1,120	1,120	1,120	1,120	1,120	1,120
Akaike Inf. Crit.	12,327.5	12,497.2	12,408.9	12,418.8	12,554.6	12,634.5
Bayesian Inf. Crit.	12,377.7	12,547.3	12,459.0	12,469.0	12,604.8	12,684.7

Note: Robust standard errors are in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

At first we look at the subject's general propensity to follow the subsidy and the targeting policy. We compare the probability that subjects send an amount that is greater or equal to 70 in the SUBHIGH and SUGGEST treatments as opposed to CONTROL treatment. We do this by estimating the following regression:

$$Pr(s \geqslant 70) = \mathcal{L}(\beta_0 + \beta_{SG}^{\geqslant} SUGGEST + \beta_{SH}^{\geqslant} SUBHIGH + v_i), \qquad (15)$$

where $\mathcal{L}$ is a standard logistic function. The results are reported in Table 4.

Table 4: Determinants $Pr(s \geqslant 70)$ by five periods – estimation of equation 15

	$Pr(s \geqslant 70)$					
	Periods					
	1-5	5-10	11-15	16-20	21-25	26-30
SUGGEST	0.8	−0.02	3.0**	5.4**	0.8	0.1
	(1.6)	(1.7)	(1.2)	(2.3)	(1.8)	(1.6)
SUBHIGH	−0.2	−0.6	2.1*	4.2**	−0.4	−0.4
	(1.5)	(1.8)	(1.1)	(2.0)	(1.8)	(1.6)
Constant	−6.0***	−8.4***	−1.9**	−5.3***	−8.6***	−7.7***
	(2.0)	(1.5)	(0.8)	(1.6)	(1.5)	(1.5)
Observations	420	420	420	420	420	420
Akaike Inf. Crit.	338.9	266.9	399.4	329.1	274.3	285.8
Bayesian Inf. Crit.	355.1	283.1	415.6	345.3	290.5	301.9

Note: Standard errors are in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

Of course, we find a significant increase in propensity to follow the targeting policy for the first five ($p = 0.0118; \beta_{SG}^{\geqslant} = 3.02; e^{\beta_{SG}^{\geqslant}} = 20.56$) as well as for the next five periods of block 2 ($p = 0.0167; \beta_{SG}^{\geqslant} = 5.4; e^{\beta_{SG}^{\geqslant}} = 222.45$). It is, however, more surprising to observe that subjects are significantly more likely to send the required amount during the subsidy policy as well (for periods 10–15: $p = 0.0633; \beta^{\gtrless}_{SH} = 2.09; e^{\beta^{\gtrless}_{SH}} = 8.04$; for periods 16–20: $p = 0.0402; \beta^{\gtrless}_{SH} = 4.2; e^{\beta^{\gtrless}_{SH}} = 67$).

This result is puzzling since we do not observe that subjects send significantly more on average in the SUBHIGH than in the CONTROL treatment in block 2 (see Table 2 in section 5.2).[19] We can partially explain it by the fact that subjects' propensity to follow the policy tends to be lower in case of the SUBHIGH than in the SUGGEST treatment (for periods 10–15: $\beta^{\gtrless}_{SH} = 2.09 < \beta^{\gtrless}_{SG} = 3.02$; for periods 16–20: $\beta^{\gtrless}_{SH} = 4.2 < \beta^{\gtrless}_{SG} = 5.4$). Thus, given the sample size, we may not capture the effect directly.

The observed pattern points out that subsidy policy significantly affects the subjects' behavior but it is not that effective as the targeting policy because subjects avoid to follow the subsidy policy. This explanation can be partially accepted, however, one needs to compare whether the propensity to follow the policy is, indeed, significantly lower in case of subsidy than in case of suggestion. To do that we estimate the following regression using the SUBHIGH treatment as a reference category:

$$Pr(s \geqslant 70) = \mathcal{L}(\beta_0 + \beta^{\gtrless}_{SG} SUGGEST + \upsilon_i) \tag{16}$$

Nonetheless we do not find a significant difference in propensity to follow the policy between the SUGGEST and SUBHIGH treatments neither in the first five periods ($p = 0.3963; \beta^{\gtrless}_{SG} = 0.89; e^{\beta^{\gtrless}_{SG}} = 2.43$) nor in the next five periods of block 2 ($p = 0.6082; \beta^{\gtrless}_{SG} = 0.73; e^{\beta^{\gtrless}_{SG}} = 2.08$). The results are reported in Table B.11 in Appendix B.4. It suggests that another source of inefficiency is possibly at work and to find it we have a closer look at the distributions of the sending in the SUBHIGH and SUGGEST treatments.

We have mentioned in Section 5.1 that the distribution of the sending is different for the SUBHIGH and SUGGEST treatments in block 2. Subjects tend to send not more than the minimal amount 70 required to get the subsidy in the SUBHIGH treatment, while in the SUGGEST treatment the subjects also send more than the minimal level suggested (see Figure 8). If this difference is significant it explains why the effect of the subsidy policy is not as large as the effect of the targeting policy.

To assess the significance of the observed disparity we evaluate whether the probabilities to send an amount that is greater than 70 or equal to 70 are different between the SUBHIGH and SUGGEST treatments. We estimate the following two logistic regressions using the SUBHIGH treatment as a reference category:

$$Pr(s = 70) = \mathcal{L}(\beta_0 + \beta^{=}_{SG} SUGGEST + \upsilon_i) \tag{17}$$

$$Pr(s > 70) = \mathcal{L}(\beta_0 + \beta^{>}_{SG} SUGGEST + \upsilon_i) \tag{18}$$

[19] As well as given that we do not observe significant growth in net payoffs during the subsidy policy (see Table 3 in section 5.3).

We report the results in the Tables 5 and 6. One can see that the probability of sending exactly 70 is significantly lower in the SUGGEST treatment as compared to the SUBHIGH treatment during the first five periods of block 2 ($p = 0.0234; \beta^{=}_{SG} = -1.71; e^{\beta^{=}_{SG}} = 0.18$). On the contrary, the probability of sending more than 70 is significantly higher in the SUGGEST treatment (than in the SUBHIGH treatment) also during the first five periods of block 2 ($p = 0.0254; \beta^{>}_{SG} = 3.44; e^{\beta^{>}_{SG}} = 31.29$).

Moreover, applying the non-parametric exact Wilcoxon test across aggregated averages over the sessions, we reject the null-hypothesis that there is no difference between the SUGGEST and SUBHIGH treatments in probability to send exactly 70 ($p = 0.0076$) and more than 70 ($p = 0.046$) during the first five periods of block 2.

That is, in the SUGGEST treatment subjects tend to send more than 70 and, hence, contribute to the growth of the average amount sent. However, in the SUBHIGH treatment subjects tend to fulfill the requirement to get the subsidy but not to send more, diminishing the average level of contribution. Thus, the specific reaction on the subsidy policy decreases its effectiveness.

Table 5: Determinants of $Pr(s = 70)$ by five periods – estimation of equation 17

	$Pr(s = 70)$					
	Periods					
	1-5	5-10	11-15	16-20	21-25	26-30
SUGGEST	1.1	0.7	−1.7**	−1.7	−0.7	−0.51
	(1.2)	(1.2)	(0.8)	(1.6)	(1.2)	(2.2)
Constant	−4.9***	−4.9***	−1.6***	−5.3**	−4.2***	−11.6***
	(1.0)	(1.0)	(0.5)	(2.6)	(0.7)	(3.5)
Observations	280	280	280	280	280	280
Akaike Inf. Crit.	46.9	38.8	234.8	186.3	38.8	27.6
Bayesian Inf. Crit.	57.8	49.7	245.7	197.2	49.7	38.5
Note:	Standard errors are in parentheses.*$p < 0.1$,** $p < 0.05$,*** $p < 0.01$					

Table 6: Determinants of $Pr(s > 70)$, in block 2 – estimation of equation 18

	Dependent variable:					
	$Pr(s > 70)$					
	1-5	5-10	11-15	16-20	21-25	26-30
SUGGEST	0.7	0.4	3.4**	2.0	1.2	0.7
	(1.1)	(1.9)	(1.5)	(2.0)	(1.8)	(1.7)
Constant	−3.2***	−9.3***	−3.7***	−8.5***	−8.8***	−8.8***
	(1.1)	(1.7)	(1.3)	(1.8)	(1.7)	(1.6)
Observations	280	280	280	280	280	280
Akaike Inf. Crit.	243.1	158.7	241.1	193.9	187.7	177.5
Bayesian Inf. Crit.	254.0	169.6	252.0	204.8	198.6	188.4
Note:	Standard errors are in parentheses.*$p < 0.1$,** $p < 0.05$,*** $p < 0.01$					

6. Discussion and Conclusion

Our analysis falls under the broad rubric of studies on monetary and non-monetary incentives and social preferences. We develop a model that predicts that the policy that involves monetary incentives can be ineffective as this type of incentives crowd-out other-regarding preferences if subjects comply with the policy. We assume that preferences are endogenous (Bowles, 1998) – the preference once learned stays unchanged for some time. Therefore, the monetary-based policy that eradicates social preferences negatively affects the subjects' pro-social behavior after the intervention. On the contrary, the policy that uses non-monetary incentives is effective during the intervention and does not have detrimental consequences in the long-run because it does not influence other-regarding preferences.

The experimental results, indeed, show that the non-monetary incentives in form of suggestion are an effective tool to foster pro-social behavior. They "nudge" (Thaler and Sunstein, 2008) people into trustful behavior (higher investment and absolute returns) in the short-run, while there is no evidence of detrimental effects of this type of incentives in the long-run. In turn, monetary incentives do not show their effectiveness in the short- as well as in the long-run though the policy significantly affects the subjects' behavior during the intervention. To interpret this fact we turn to the taxonomy of incentive effects on preferences provided by Bowles and Polania-Reyes (2012).

According to their taxonomy, there are three mechanisms linking interventions and preferences: "bad news" – incentives provide information about interests of a principal; "control aversion" – incentives jeopardize self-determination; "moral disengagement" – incentives activate a switch from pro-social to own payoff maximization mode of thought. We do not consider here the first one ("bad news") since the incentives are provided by the third-party and, hence, should not affect the subjects' behavior. However, the last two –"control aversion" and "moral disengagement" – can explain the specific pattern of subjects reaction on the subsidy policy.

Subjects react to the monetary policy but (1) their propensity to follow this policy is low and (2) those who follow the policy send the minimal amount required to get the subsidy. We attribute the low propensity to follow the policy to the mechanism of "control aversion": Subjects perceive the policy as controlling and avoid following it. The "moral disengagement" can explain the fact that subjects send mostly the minimal amount: They switch their way of thinking to own-payoff maximization, thence, if they decide to follow the policy they simply minimize their costs by sending the minimal amount.

As concerns the post-intervention effect of the policies, despite the fact that we do not find a significant difference between treatments after the policy interventions, we observe that subjects tend to send a high amount after the targeting policy. This is an interesting observation since it suggests that a targeting policy can have a potentially long-lasting effect. Nevertheless, further research is needed to test this observation.

It is also interesting to observe that the trustworthiness rate is not affected during the intervention as well as it does not change afterward. On the one hand, this goes in line with theoretical expectations – the trustee's behavior should remain the same since the policies incentivize only trustors. On the other hand, given that trustees also receive subsidies, this fact suggests that the presence of a subsidy is insufficient to crowd out other-regarding preferences. It is rather likely that the crowding out occurs when the monetary incentives are conditioned on a certain behavior.

To sum up and conclude, in this study, we aim to understand how subsidy and targeting policies affect an investment decision. We employ a multi-period trust (investment) game where we introduce an external intervention either in form of subsidy or suggestion and analyze the level of trustful behavior during and after the intervention.

We find that targeting (suggestion) is an effective instrument to promote trustful behavior (investment and absolute returns) in the short-run whereas subsidy policy is not effective both in the short- and long-run: Subjects follow the targeting policy and send even more than minimal level suggested, while under the subsidy policy they exhibit low propensity to follow the policy and send mostly the minimal amount needed to get the subsidy. We, therefore, recommend the targeting (suggestion) policy as an instrument to foster investments in the short-run, put it differently, to "nudge" investment activity.

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Appendix A. Instructions

Appendix A.1. Player 1, Trustor.

Welcome to the experiment!

Thank you very much for participating. We hope that you feel comfortable. We ask you to remain quiet and do not communicate with any other player. Please understand that in case you communicate with other players we will have to exclude you from the experiment without payment. If you have any questions please raise your hand and wait for the experimenter to come to you.

We guarantee that all information collected during the experiment undergoes a strict anonymity process. It ensures anonymity among players and that you stay anonymous to the experimenter.

During the experiment you will see information about other players. We have ensured that you cannot identify them personally as well as they cannot identify you.

The experiment is on decision-making. Your earnings will depend partly on your decisions and partly on the decisions of other players. You will have to make one decision in each round of a simple game which consists of 30 rounds.

In each round of the game the earnings will be calculated in points. At the end of the experiment **one round** will be randomly chosen. The points gained during this round will be converted to Euros with the following rate:

10 points = 0.35 Euro

In addition, you will receive 2.50 euro as a compensation for showing up on time. The game you will play is divided **into three blocks** (A, B and C), with 10 rounds in each block.

In each round of any block you will be matched with another randomly chosen player among other participants. There will be a new random pair each round.

The information about your previous decisions will not be revealed to other players at any round of the experiment.

In each round you and the other player both will be endowed with 100 points. **You can send any amount** to the other player. **Each point you send is tripled.** The other player will decide how many points to send back to you and how many points to keep (from zero to the tripled sum you sent).

[For the SUBHIGH and SUBLOW treatment we add the following paragraph]

Also, in some blocks if you send **not less than a certain minimum**, you and the other player will receive **an additional payment**. The amount of the **additional payment** and the required minimum sent to receive it will be specified in the beginning of each block.

[For the SUGGEST treatment we add the following paragraph]

In some blocks it will be suggested to send not less than a certain amount. The amount suggested is specified at the beginning of each block.

Appendix A.2. Player 2, Trustee.

Welcome to the experiment!

Thank you very much for participating. We hope that you feel comfortable. We ask you to remain quiet and do not communicate with any other player. Please understand that in case you communicate with other players we will have to exclude you from the experiment without payment. If you have any questions please raise your hand and wait for the experimenter to come to you.

We guarantee that all information collected during the experiment undergoes a strict anonymity process. It ensures anonymity among players and that you stay anonymous to the experimenter.

During the experiment you will see information about other players. We have ensured that you cannot identify them personally as well as they cannot identify you.

The experiment is on decision-making. Your earnings will depend partly on your decisions and partly on the decisions of other players. You will have to make one decision in each round of a simple game which consists of 30 rounds.

In each round of the game the earnings will be calculated in points. At the end of the experiment **one round** will be randomly chosen. The points gained during this round will be converted to Euros with the following rate:

10 points = 0.35 Euro

In addition, you will receive 2.50 euro as a compensation for showing up on time. The game you will play is divided **into three blocks** (A, B and C), with 10 rounds in each block.

In each round of any block you will be matched with another randomly chosen player among other participants. There will be a new random pair each round.

The information about your previous decisions will not be revealed to other players at any round of the experiment.

In each round you and the other player both will be endowed with 100 points. You will receive some amount of points from the other player. **Each point sent by the other player is tripled. You can decide** how many points to send back to him and how many points to keep (from zero to the tripled sum of points the other player sent).

[For the SUBHIGH and SUBLOW treatment we add the following paragraph.]

Also, in some blocks if the other player sends **not less than a certain minimum**, you and the other player will receive **an additional payment**. The amount of **the additional payment** and the required minimum sent to receive it will be specified in the beginning of each block.

[For the SUGGEST treatment we add the following paragraph]

In some blocks, it will be suggested to other player to send not less than a certain amount. The amount suggested is specified at the beginning of each block.

Appendix B. Additional Estimations

Appendix B.1. Amount Returned

Table B.7: Determinants of the amount Returned (R) by five periods without control variable amount sent (s) – estimation of equation 12

	Returned (R)					
	Periods					
	1-5	5-10	11-15	16-20	21-25	26-30
SUGGEST	−0.40	2.71	27.20**	19.31	13.96	9.80
	(11.03)	(12.70)	(13.60)	(13.20)	(11.59)	(11.93)
SUBHIGH	−13.64	−12.87	2.94	2.74	−6.04	2.74
	(10.72)	(12.52)	(13.48)	(13.12)	(11.27)	(11.83)
SUBLOW	1.02	−2.38	6.72	9.61	8.21	20.81*
	(10.67)	(12.07)	(12.88)	(12.59)	(10.99)	(11.47)
Constant	49.97***	53.81***	49.94***	43.59***	37.01***	25.99***
	(7.71)	(8.79)	(9.46)	(9.08)	(7.81)	(8.15)
Observations	560	560	560	560	560	560
Log Likelihood	−3,071.29	−3,126.01	−3,055.48	−3,050.39	−3,070.61	−3,037.68
Akaike Inf. Crit.	6,160.59	6,270.02	6,128.96	6,118.78	6,159.23	6,093.37
Bayesian Inf. Crit.	6,199.48	6,308.91	6,167.84	6,157.67	6,198.12	6,132.26
Note:	Robust standard errors are in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$					

Appendix B.2. Trustworthiness

Table B.8: Determinants of trustworthiness by five periods – estimation of equation 13

	Trustworthiness (r)					
	Periods					
	1-5	5-10	11-15	16-20	21-25	26-30
SUGGEST	−0.004	0.05	0.04	0.05	0.06	0.05
	(0.05)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
SUBHIGH	−0.01	0.01	0.03	0.04	0.02	0.09
	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
SUBLOW	0.11**	0.05	0.09	0.08	0.09	0.12*
	(0.05)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
Sent (s)	0.002***	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.0003)	(0.0002)	(0.0003)	(0.0003)	(0.0002)	(0.0003)
Constant	0.25***	0.27***	0.25***	0.24***	0.24***	0.19***
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Observations	459	405	455	398	371	312
Log Likelihood	59.95	62.03	110.61	109.44	94.13	38.31
Akaike Inf. Crit.	−99.91	−104.06	−201.22	−198.87	−168.26	−56.62
Bayesian Inf. Crit.	−58.73	−64.15	−160.13	−159.13	−129.23	−19.35
Note:	Robust standard errors are in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$					

$$r = \beta_0 + \beta^r_{SG} SUGGEST + \beta^r_{SL} SUBLOW + \beta^r_{SH} SUBHIGH + \upsilon_i + \epsilon_{i,t} \quad \text{(B.1)}$$

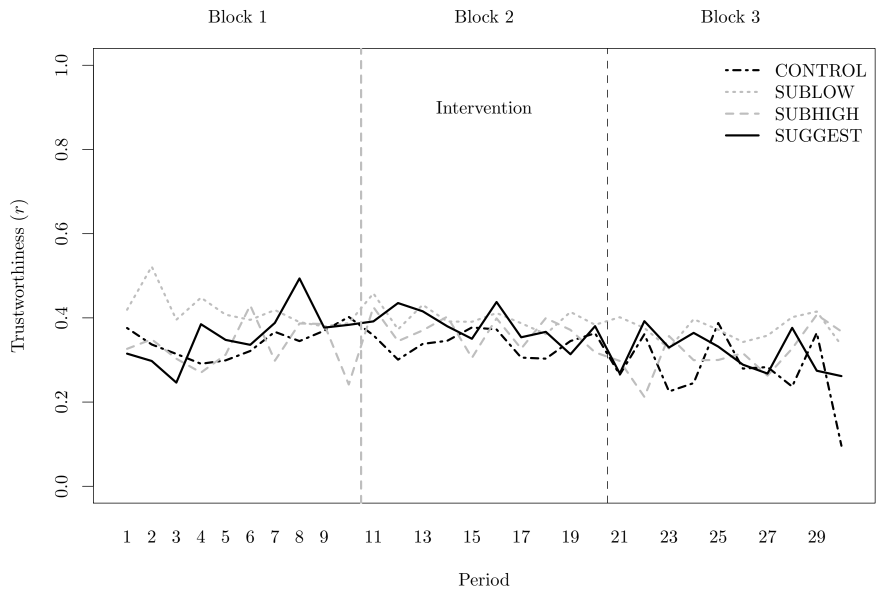

Figure B.9: Average trustworthiness by treatment.

Table B.9: Determinants of trustworthiness by five periods without control variable amount sent (s) – estimation of equation B.1

	Trustworthiness (r)					
	Periods					
	1-5	5-10	11-15	16-20	21-25	26-30
SUGGEST	0.001	0.06	0.05	0.06	0.07	0.06
	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
SUBHIGH	−0.02	0.01	0.04	0.06	0.02	0.10
	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
SUBLOW	0.10*	0.03	0.07	0.07	0.08	0.12*
	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
Constant	0.33***	0.35***	0.34***	0.31***	0.28***	0.24***
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Observations	459	405	455	398	371	312
Log Likelihood	51.64	55.23	104.88	107.39	94.55	41.72
Akaike Inf. Crit.	−85.29	−92.45	−191.75	−196.77	−171.10	−65.44
Bayesian Inf. Crit.	−48.20	−56.51	−154.75	−160.99	−135.95	−31.87

Note: Robust standard errors are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix B.3. Gross Payoff

$$\pi = \beta_0 + \beta^{\pi}_{SG} SUGGEST + \beta^{\pi}_{SL} SUBLOW + \beta^{\pi}_{SH} SUBHIGH + P + \upsilon_i + \epsilon_{i,t}, \quad \text{(B.2)}$$

Table B.10: Determinants of Gross Payoff (π) by five periods – estimation of equation B.2

	Dependent variable:					
	Gross Payoff (π)					
	1-5	5-10	11-15	16-20	21-25	26-30
SUGGEST	1.2	0.6	16.3**	12.9	7.5	4.4
	(6.8)	(6.9)	(8.2)	(8.2)	(8.6)	(8.8)
SUBHIGH	−9.2	−8.7	14.8*	10.6	−6.6	−5.7
	(6.9)	(7.0)	(8.5)	(8.3)	(8.7)	(8.7)
SUBLOW	−5.2	−0.2	21.2***	23.2***	1.5	6.1
	(6.7)	(6.9)	(7.8)	(7.7)	(8.2)	(8.3)
Player (P)	−67.4***	−59.4***	−76.2***	−68.0***	−69.2***	−65.2***
	(4.8)	(4.9)	(5.8)	(5.6)	(6.0)	(5.9)
Constant	177.3***	171.9***	181.0***	171.6***	171.8***	165.0***
	(5.4)	(5.5)	(6.5)	(6.4)	(6.8)	(7.0)
Observations	1,120	1,120	1,120	1,120	1,120	1,120
Akaike Inf. Crit.	12,327.5	12,497.2	12,468.5	12,493.4	12,554.6	12,634.5
Bayesian Inf. Crit.	12,377.7	12,547.3	12,518.7	12,543.5	12,604.8	12,684.7
Note:	Robust standard errors are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$					

Appendix B.4. Probability to Follow the Policy (SUGGEST VS. SUBHIGH)

Table B.11: Determinants $Pr(s \geqslant 70)$ by five periods – estimation of equation 16

	$Pr(s \geqslant 70)$					
	Periods					
	1-5	5-10	11-15	16-20	21-25	26-30
SUGGEST	0.8	0.6	0.9	0.7	1.2	0.5
	(1.1)	(1.8)	(1.0)	(1.4)	(1.8)	(1.7)
Constant	−3.3***	−9.0***	0.2	−0.7	−9.1***	−8.6***
	(1.1)	(1.7)	(0.7)	(1.0)	(1.7)	(1.6)
Observations	280	280	280	280	280	280
Akaike Inf. Crit.	248.9	172.1	280.0	250.6	185.4	180.9
Bayesian Inf. Crit.	259.8	183.0	290.9	261.5	196.3	191.8
Note:	Standard errors are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$					