Short- and long-run effects of external interventions on trust

Article (Accepted Version)

Example Journal, XXXX, XX: 1–46

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

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ABSTRACT

We analyze experimentally the effects of external interventions such as subsidy and targeting on investment decisions, during an 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 while subsidy policy is effective in neither the short- or the long-run. Therefore, we suggest a targeting policy should be considered as an instrument to foster trustful behavior.

JEL Classification: C92, L50, D80

1 Introduction

In 1998, Stanford University licensed its *PageRank* patent to one of its newly established spin-off companies. This investment resulted in the emergence of one of the worlds' largest high-tech companies *Google*

ISSN XXXX-XXXX; DOI XXXXXXXX

which quickly revolutionized the world market. In addition to its public economic impact, this investment has brought private financial benefits to Stanford involving to a large extent voluntary financing of research scholarships and collaborative projects.[1]

The success of Google illustrates why governments often intervene to try to foster academic spin-off creation and knowledge commercialization. This intervention typically takes the form of a two-phase subsidy policy: (i) a subsidy for university initial spin-off investment, and (ii) additional government funding if the spin-off proves successful.[2]

Alternative interventions such as targeting are rarely considered even though they may not involve subsidy spending. Also, since policy makers tend to focus on the immediate effects of policy, potential long-term post-intervention costs are also not considered. In this paper we fill this gap conducting a controlled laboratory experiment which allows a direct comparison of the short- and long-run efficiency of different policies.

In this experiment, we analyze the effects of external interventions such as subsidies and targeting on investment decision both during and after the intervention. We employ a multi-period version of the trust (investment) game (Berg *et al.*, 1995) introducing monetary incentives or suggesting a level of investment. The experiment consists of three blocks, the second of which features policy intervention. This allows the assessment of the immediate and the post-intervention effects.

In the non-monetary intervention, we exploit the experimenter demand effect in line with previous research on the influence of “tax frame" (Sugawara and Nikaido, 2014; 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 than, for instance, assigning authority to a subject, since our aim is to model government policy rather than the peer pressure effect. Also, it increases internal validity because we can set up the level of suggestion

[1]For instance, in 2008, Google paid approximately $1,881,400 to Stanford University of which only $426,950 related to licensing of patents. The largest part — some $1,246,000 — took the form of donations for scholarships and other philanthropic endeavors (Wikinvest.com, 2009).

[2] See, e.g., programs such as “Small Business Technology Transfer” (SBTT) in the United States and “Existenzgründungen aus der Wissenschaft” (EXIST) in Germany.

exogenously.

In this respect, Pelligra *et al.* (2016) most resembles the suggestion treatment part of our experiment. However, Pelligra and colleagues are interested in how the experimenter request affects trustworthiness and find that it systematically decreases it. Silverman *et al.* (2014) also exploit suggestion but in a public good game, while Sugawara and Nikaido (2014) ask to act with respect to *expectations* rather than suggestion and Karakostas and Zizzo (2016) use requests to induce anti-social behavior.

With respect to monetary interventions, in a paper most closely related to the subsidy treatment part of our study Charness *et al.* (2008) show that cooperative behavior in trust games increases if a third-party (third player) is able simultaneously to reward the trustor and punish the trustee. This raises concerns if the effect is driven by punishment, reward, monitoring or expectations of reward or 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, in their experiment they do not define the reward (punishment) rules, thus, their data provide little information on whether behavior changes as the results of expectations (threat) of reward (punishment), beliefs about the expected level of investment, or reaction to incentives.

We take a different line: We introduce a subsidy exogenously and vary the thresholds for receiving the subsidy (setting it either above or below the average trust level without intervention). Hence, we study the effect of subsidies on trust and trustworthiness directly. This is both interesting from a theoretical viewpoint and important from a policy perspective: Subsidy policies rely on defined (written) rules.

The study provide four main original contributions. It is the first to analyze the effect of non-monetary intervention in form of third-party suggestion about *trust*; Second, it compares the effect of non-monetary and monetary interventions and explains the difference in how they perform; Third, it analyzes the long-run effects of external interventions on trustful behavior; and fourth, it is to our knowledge the first paper to analyze the rule-based effect of a third-party monetary reward on *trust*.

We address the following research questions: (1) Does a non-monetary intervention such as suggestion increase investment activity during and after its implementation? (2) Is subsidy policy an efficient means to foster investment activity in the short-run? (3) Is a low level of investment required to receive a subsidy detrimental to an investment? (4) Does a subsidy policy have a negative impact on investment levels after the policy termination?

We find that non-monetary interventions in the form of suggestion increase investment activity during the intervention and even though this effect might be short lived, we find no indication of subsequent detrimental effects. We find also that a subsidy policy does not significantly affect the level of trust, the amount returned, or the level of trustworthiness in either the short- or the long-run. We associate the ineffectiveness of subsidy policy to two regularities: Subjects show low propensity to follow this policy and if subjects follow it, they mostly send the lowest amount required to get the subsidy. From a welfare perspective (net payoffs), however, we find that targeting policy increases net payoff, but no evidence that the targeting policy outperform the subsidy policy.

We provide indirect evidence of the ineffectiveness of monetary intervention due not to the presence of the subsidy itself, but rather to the fact that the monetary reward is conditioned on a certain behavior: Subjects that unconditionally receive subsidies do not show significantly different levels of trustworthiness. We conclude that a targeting policy should be considered an effective tool to foster investment activity or, in other words, to nudge higher levels of investment.

2 Further Related Literature

The paper builds on four literature strands. First, it is related to work on the interaction between intrinsic and extrinsic motivation. From Titmuss (1970) early research on blood donation to Andreoni (1993) experiment on public good provision, these 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.

However, Bowles and Polania-Reyes (2012) come to a different conclusion based on the evaluation of the results of 50 experiments on the relation between incentives and social preferences. They find that the effect of 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 literature strand looks at the role played by trust in investment decisions. Trust is involved in almost every economic transaction (Arrow, 1972) and the empirical evidence suggests that it is crucial for venture capital investments (Bottazzi *et al.*, 2011) and mutual investment decisions (Felli *et al.*, 2010), and is associated positively to the level of investment across countries (Knack and Keefer, 1997).

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

The thid stream of work includes studies of the interaction between external incentives and trustful behavior. Fehr and List (2004) find that a not used threat to punish increases trustworthiness, whereas punishment crowds out trustworthy behavior. It has been shown also, that the threat of potential contract enforcement crowds in trustworthiness (Bohnet *et al.*, 2001), but trustworthiness is decreasing in presence of sanctioning (Fehr and Rockenbach, 2003). Houser *et al.* (2008) reconcile these findings by showing that the effect of sanction depends on the relation between the amount requested and the level of the sanction, not the 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.

In the specific case of subsidies and trustful behavior, the effort exerted by agents is affected non-monotonically by additional compensation (Gneezy and Rustichini, 2000), and if the principal imposes a lower bound on the effort, agents mostly exerts effort at this bound (Falk and Kosfeld, 2006). Interestingly, Gachter *et al.* (2011) show that the effort exerted increases in the presence of both a fine or a bonus, but that in the latter case subjects tend to choose an effort that is not higher than the best-reply level condition.

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

The fourth set of studies looks at the effect of non-monetary incentives on trust. A seminal paper by Berg *et al.* (1995) provides evidence that an aggregated information on previous behavior — average amount sent, returned and net return from 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 information on others' previous actions can enhance cooperation, but they report little effect of cheap-talk messages. However, Charness and Dufwenberg (2006) show that promises increase cooperation in a one-shot trust game.[3] Schotter and Sopher (2006) show that intergenerational advice decreases trust levels, but increases trustworthiness in one-shot (per generation) trust game. Finally, Pelligra *et al.* (2016) directly exploit the effect of experimenter's request on trustworthiness and show that request provide some "wiggle room" and thus decreases 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: player 1 (trustor) de-

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

cides how much of his initial endowment E to send (give) to player 2 (trustee). The amount sent s is multiplied by a certain factor m and player 2 receives the multiplied sum. In turn, player 2 chooses how much of the amount received to return R. See figure 1 for a depiction of the structure of the game and the payoffs π of players 1 and 2.

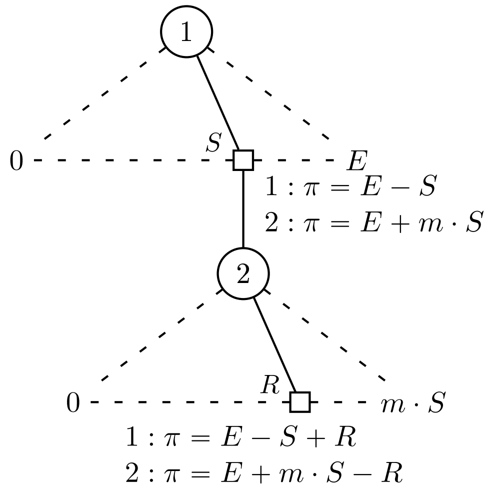

Figure 1: Trust (investment) game.

In our version of the investment game, we introduce an external intervention devised alternately as 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 depicts this version of the game). In the suggestion case, no subsidy is available but the suggestion is to send not less than a threshold level.

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

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

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 the

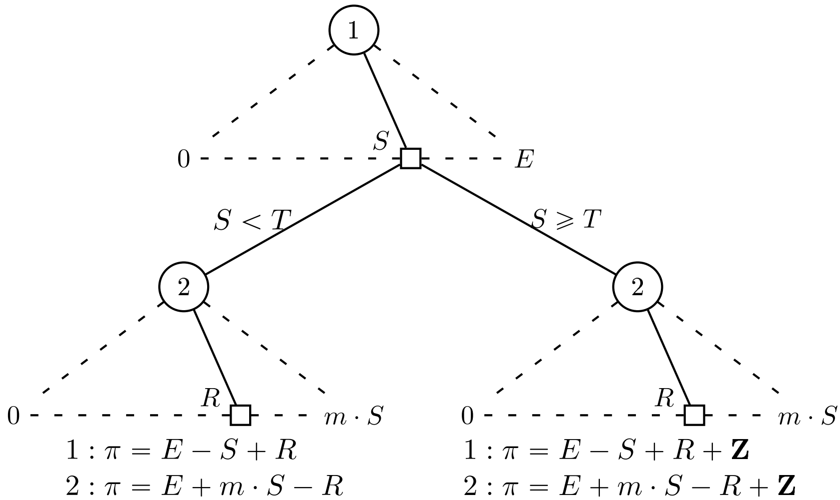

Figure 2: Trust (investment) game with subsidies.

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 which depend on s, I is the effect of external incentives which can take the form of either subsidy or suggestion.

Let us begin the analysis with the subsidy policy which is characterized by a tuple of parameters (Z, T), indicating respectively the size of the subsidy and the threshold (minimal) amount that the player must send to obtain this subsidy.

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

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

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 (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 of no incentives. The subsidy is contingent on the relation between the threshold and the amount sent. Therefore, we 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 than the threshold. We obtain the following two relations:

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

It is easy to see that it is beneficial to send more if 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, $\frac{Z}{\Delta s} = 0$), while the negative effect of the subsidy on other-regarding preferences persists, $\frac{\lambda \Delta o(s)}{\Delta s} < 0$. Therefore, we ca formulate the following hypotheses:

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

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

Concerning the targeting policy (suggestion), this also is characterized by a threshold level T (the suggested minimal amount to be sent). The policy does not use subsidy but players obtain an utility by complying with the authority (Karakostas and Zizzo, 2012).[5] We denote this utility by A (which 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 the case without the incentives is lower than the threshold $s_0 < T$ the players benefit from complying with authority. Therefore, they can sacrifice part of their endowment to follow the suggestion. However, when $s_0 > T$ they do not benefit since the utility is independent of the amount sent.

H 3. *The amount sent is higher in the presence rather than the absence of external non-monetary incentives 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 persist. 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 no longer present. However, other-regarding preferences remain negatively affected $\frac{\lambda \Delta o(s)}{\Delta s} < 0$. Thus, we formulate:

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

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

Similarly, from 5 we derive the next relations for the period after the targeting policy:

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

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

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

3.3 Trustworthiness under External Incentives

We can represent the trustee's utility function 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 which change with r, and I is the effect of external intervention (subsidy or suggestion).[6]

We assume that trustees maximize their utility. Since external intervention depends on the trustor's behavior but not on the 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 compared to the case without it when conditioned on the amount sent by the trustor s.*

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

4 Experimental Design

The experiment was conducted in the Max Planck Institute of Economics laboratory in Jena (Germany) in April 2013. Seven sessions were run, each lasting about 60 minutes and employing 32 experimental subjects. Experimental subjects were recruited using the ORSEE system (Greiner, 2015), and the experiment was programmed and implemented with the help of z-Tree software (Fischbacher, 2007).

During 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 were the same for all subjects — they faced the standard trust game. However, in the second block, subjects played different versions of the trust game depending on the treatment to which they were randomly assigned: SUBLOW, SUBHIGH, SUGGEST, CONTROL.

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

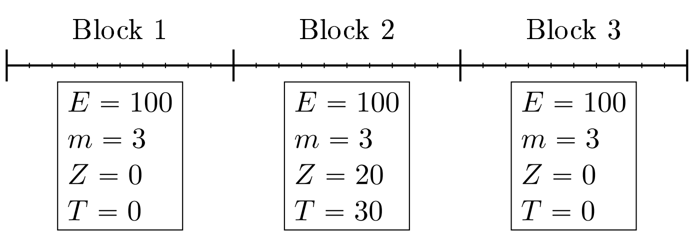

Figure 3: SUBLOW treatment parameters.

The SUBHIGH treatment differs from the SUBLOW treatment only in the threshold level: To obtain the subsidy the trustor needs to send not less than 70, $T = 70$ (see figure 4).

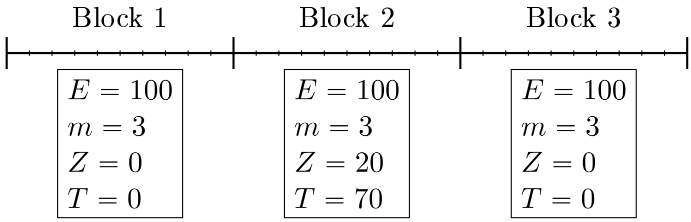

Figure 4: SUBHIGH treatment parameters.

In the SUGGEST treatment — the case of the targeting policy — the subsidy is absent in all blocks but in block 2 the experimenter suggests the player to send not less then 70, so $T = 70$ as in the SUBHIGH treatment (see figure 5).

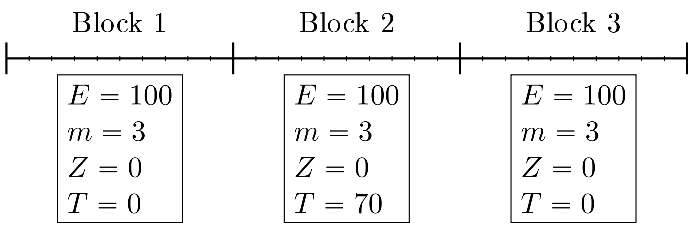

Figure 5: SUGGEST treatment parameters.

In the CONTROL treatment, the standard trust game without any subsidies or suggestions is played for all three blocks (see figure 6).

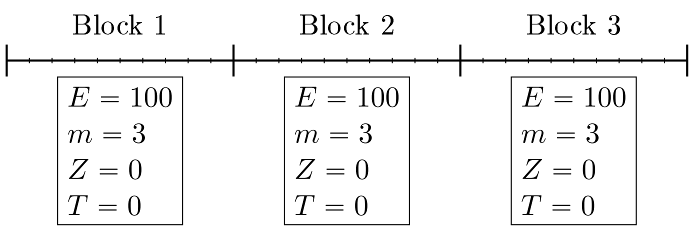

Figure 6: CONTROL treatment parameters.

We ran all four treatments (varied between subjects) during the same session to control for session-specific effects. We displayed treatment-specific information (amount of subsidy and threshold level) on the computer screen, together with information about both players endowment; this information was shown in each period.[7] Subjects were randomly assigned to the treatment, and to the roles of trustor or trustee. They retained their roles throughout the whole experiment and were randomly matched to other players in the same treatment in each period of the experiment (stranger matching design). Subjects were informed that they would be randomly matched to a new player in each period (the matching algorithm was implemented accordingly), but were given no information on the number of treatments in the session. Thus, they could not predict to whom they would be matched except that it would be a different player in each period. The order of matching was random but identical in all four treatments within the same session. That allows us to reduce the potential effects of the history of the interaction. We kept the roles constant and used stranger matching because, in our view, this design captures the situation of a university's repeated but independent decisions to engage in spin-off activities.

The subjects received payments privately at the end of the experiment according to the points they gained in one randomly selected period of the game.[8] Points were converted to Euros at the rate of 10 points for €0.35.[9] Subjects could send (back) any amount of points. In the control questions section of the experiment we showed an example situation with a random number of amount sent (back), which can take any possible value within the endowment. There could be a concern that the conversion rate might have distorted incentives (for the amount of fifty Euro cents). However, we observed that subjects sent any value in the range 0 to 100 at least once, and returned (sent back) almost any value in the range 0 to 300 (subjects were allowed to send only whole points). Including a participation fee of €2.50, subjects earned on average €6.81 with a minimum €2.5 and a maximum €15.5.

[7] Additionally, we provided on the screen a game customized calculator, so that a subject could see for a given sending how many points (s)he keeps and how much the other player will receive and have conditional on the amount returned. Also, the screen showed an history of an individual's choices and payoffs.

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

[9] The experimental points were rounded up to nearest 50 cents in final payments.

Table 1: Participants characteristics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Age	224	24.15	4.04	18	22	26	48
Share of Females (♀)	224	0.49	0.50	0	0	1	1
Exp. Interesting	224	2.54	1.25	1	2	3	5
Exp. Length	224	2.30	0.87	1	2	3	5
Exp. Understandable	224	4.14	1.17	1	4	5	5
Task difficulty	224	2.27	1.56	1	1	3	8

Table 1 summarizes the descriptive data on the subjects and their perception of the experiment obtained via the questionnaire administered at the end of each experimental session. We almost perfectly balanced the sample with respect to 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). Our sample also covers a wide range of age groups from 18 to 48 although most participants were relatively young (median age: 23.5).

In terms of experiment complexity, subjects reported a fairly good understanding of instructions with an average value of 4.14 on a 1 to 5 scale and low task difficulty, with mean 2.27 on a 1 to 10 scale.

5 Results

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

To assess subjects' behavior, we first compare the average amount sent in each round across 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 other studies and equals to 40.24.[10] A visual inspection shows no evident differences in trust levels in block 1 across the four treatments. This is expected since subjects play the same standard trust game in all four treatments.

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

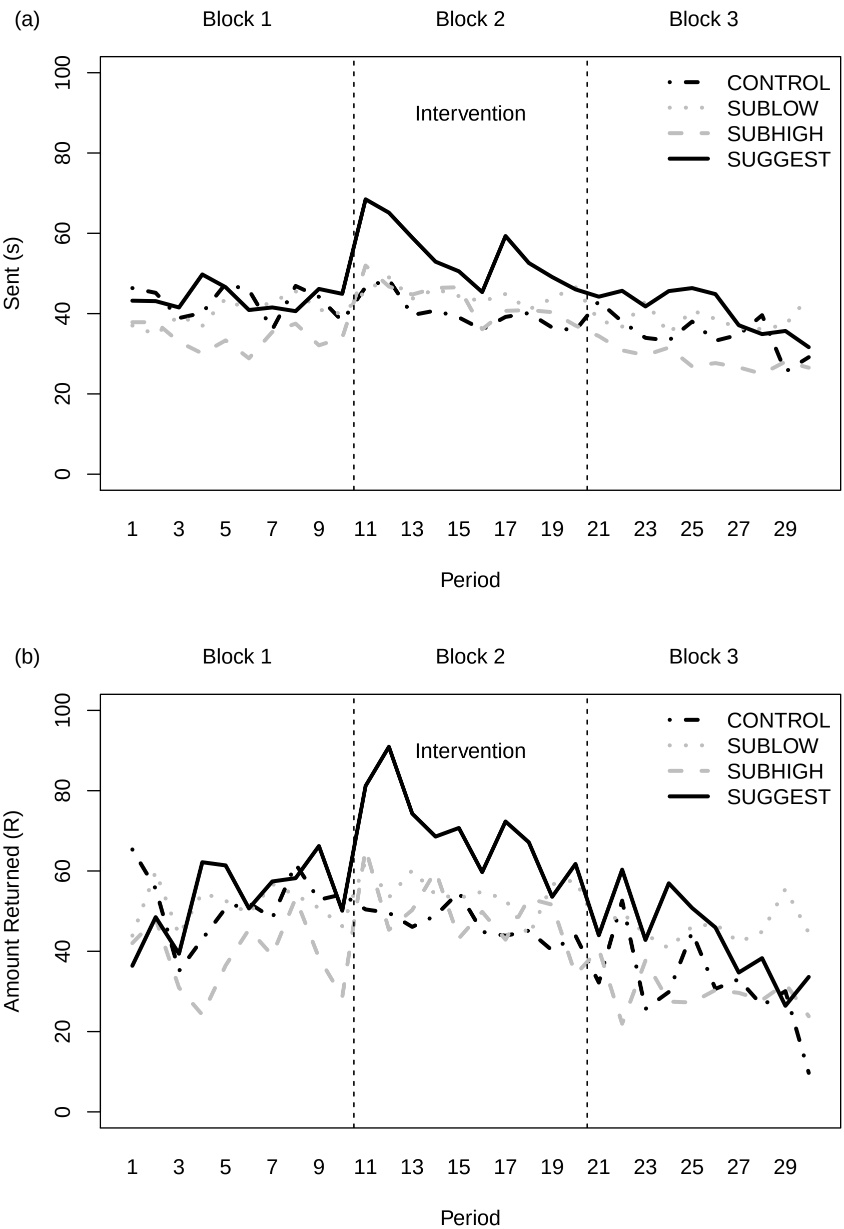

Figure 7: Average amount sent (a) and amount returned (b) by treatment.

Let us now consider behavior during the policy intervention — 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 the first period of the intervention tends towards the suggested level of 70 points, $\bar{s}_{SG} = 68.46$, which is far above the average amount sent in the control group, $\bar{s}_{CL} = 46.61$ (Standardized Effect Size, Cohen's d_{11}=0.581). The difference stays positive during the intervention but in the last period of the intervention it reduces, reaching a level comparable to the control group ($\bar{s}_{SG} = 46.04$ vs. $\bar{s}_{CL} = 35.82$; Standardized Effect Size, Cohen's d_{20}=0.239). Thus, the suggestion policy seems to be very effective although its effect is decreasing over time.

It can be seen that the curve of the average amount sent in the treatment with suggestion is always above the corresponding curve for the treatments with subsidy during the intervention. However, the plot does not show any difference between the amount sent in the 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. However, the amount sent in the SUBHIGH treatment is lower than in the CONTROL treatment. The average sent in SUBLOW treatment is similar to the corresponding amount in CONTROL treatment.

To obtain a clearer picture of the differences among 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 lower than a certain value, allowing a detailed view of the distribution of the amounts sent.

Again, we observe no substantial difference between the treatments in block 1 but we observe a very different shape of the distributions in block 2. It is not difficult to identify discontinuities corresponding 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. The changes observed are related to the policy intervention.

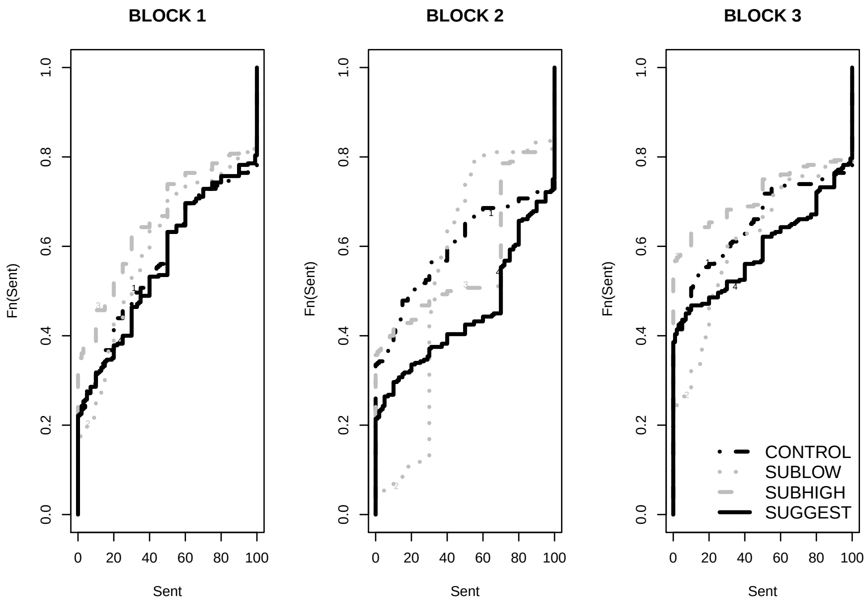

Figure 8: Cumulative distribution of amount sent by treatment.

Interestingly, we observe 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). In the SUGGEST treatment subjects do not send just the suggested minimal level, but continue also to send higher values, while among subjects in the SUBHIGH treatment only a few send contributions higher than that required for the subsidy. A potential explanation for this pattern is a crowding out effect (appendix B.4 provides some supporting evidence).

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

To further understand the effect of the different policies during the intervention, we analyze how the amount sent changes in different treatments depending on the original level of trust. To do this, we plot the average amount sent in block 1 (unaffected by any policy) against the average amount sent in block 2 for each trustor (see figure 10a).

The plot shows a strong relationship between the average amounts sent in blocks 1 and 2. More importantly, we can see that subjects send more under the suggestion treatment (the crosses on the plot) across all levels of initial trust. This is reflected in an upward shift of the SUGGEST treatment regression line (black line) which lies parallel to the CONTROL regression line (black dashed-dotted line).

However, we do not observe a similar uniform reaction to the SUBHIGH (grey squares) and SUBLOW (grey triangles) treatments across different levels of initial trust. Instead, we observe a shift of the regression line on the plot for the subsidy treatments (grey lines) but the gradual slope of this line indicates that the behavior of subjects with an initial high level of trust changes to a lower extent than in other treatments and that it is not necessarily homogeneous across initial levels of trust.

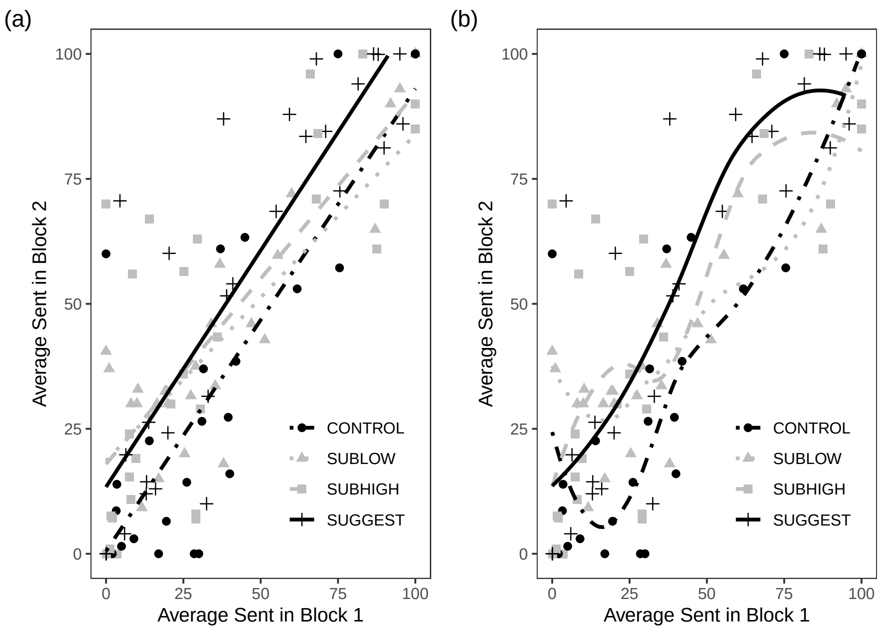

Figure 9: Relation between average amount sent ($\bar{s}$) in Block 1 and Block 2 with (a) linear and (b) loess smoothed conditional means.

We investigate the non-homogeneous effect of the interventions conditional on the difference in the initial trust level by plotting loess smoothed conditional means (see figure 10b). Loess curves indicate a similar pattern: Subjects send more under the SUGGEST (black curve) than the CONTROL treatment across different levels of initial trust (taking into account that they are constrained by the endowment of 100 points). However, the plot additionally shows the non-linear reaction in case of the subsidy (SUBHIGH), with a spike occurring at around the initial level of trust of 25 points.

Taken together, these observations indicate that under the SUGGEST treatment subjects send more across all levels of initial trust and send more than the minimum requested amount, whereas under the subsidy policy only a few subjects provide a contribution that is higher than requested. This pattern explains why the average amount sent under the SUGGEST treatment is higher than the average amount sent in the CONTROL treatment while we do not observe a higher average contribution under the subsidy policy compared to the control condition (see figure 7).

We conclude the descriptive analysis by discussing how much players 2 (trustee) send back (see figure 7b). We can see that the average amount returned mirrors the amount sent across all treatments. More importantly, the amount returned reflects the amount sent also during the intervention. That is, under the subsidy policy trustees do not decrease the return level and under the suggestion policy subjects return a higher amount in correspondence to the amount they receive. Thus, trustees continue to reciprocate trust despite the external intervention.[11]

To assess whether the rate of return corresponds proportionally to the amount sent, we calculate the ratio of the amount sent back by player 2 to the amount received by that player — the trustworthiness rate, $r = \frac{R}{3 \cdot s}$. As expected, we observe no difference in trustworthiness between treatments (see Appendix B.2). The stability of trustworthiness across the treatments allow us to focus on analyzing the effects of external interventions on trust and the overall efficiency of the policy.

[11] We also study if the amount returned depends on the level of initial propensity to return (see appendix B.1). Again, we observe that the amount returned mirrors the amount sent across different levels of initial propensity to return. In the SUBHIGH treatment it appears that subjects with an initial high level of propensity to return tend to send less, even though they continue to reciprocate.

5.2 Regression Analysis of Trust, Amount Returned, and Trustworthiness

To assess the significance of our results, we provide a regression analysis using a mixed effects model with random effects for individual subjects. We estimate the difference in the amount sent (trust level) across treatments by running the following regression in block 1:

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

where $SUGGEST$, $SUBLOW$, $SUBHIGH$ are dummy variables equal to 1 for the corresponding treatments, υ_i is the random effect for subject i, m_p is random effect for matching pair, and $\epsilon_{i,t}$ is the error term for subject i in period t.[12] For blocks 2 and 3 we run a similar regression to 11 but add the average amount sent in the block 1 per subject ($\bar{s}_{i,B1}$) to account for variance in subjects' propensity to sent:

$$s = \beta_0 + \beta_{SG} SUGGEST + \beta_{SL} SUBLOW + \beta_{SH} SUBHIGH + \\ + \beta_{\bar{s}} \bar{s}_{i,B1} + \upsilon_i + m_p + \epsilon_{i,t}, \quad (12)$$

Adding the average amount sent in the block 1 (baseline measure) improves the statistical power and increases the precision of the estimates.[13] The results are reported in table 2.

In line with our expectations and the pattern observed in figure 7 we find no 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 us consider the effect of the intervention. We observe that during block 2 subjects send significantly more in the SUGGEST than in the CONTROL treatment ($p = 0.02$ corrected for multiple comparisons with Dunnet procedure — MPC corrected; $\beta_{SG} = 13.873$). To understand the effect of the suggestion policy in more detail we scrutinize the intervention by period by running a non-parametric exact Wilcoxon test across aggregated averages of the amounts sent over

[12] As a robustness check, we estimated a linear regression with robust standard errors clustered on subjects and matching pairs (for all linear mixed-effects specifications). Our results hold also using this specification.

[13] We thank an anonymous referee for suggesting this idea.

Table 2: Determinants of Sending (s) – estimation of equations 11, 12

	Sent (s)		
	Block 1	Block 2	Block 3
SUGGEST	0.91	13.87**	5.02
	(8.87)	(5.17)	(4.36)
SUBHIGH	−8.92	10.23	2.88
	(8.85)	(5.26)	(4.34)
SUBLOW	−2.70	6.82	6.49
	(8.85)	(5.08)	(4.25)
Average Sent in Block 1 ($\bar{s}_{i,B1}$)		0.82***	1.01***
		(0.06)	(0.05)
Constant	42.91***	5.09	−8.49**
	(6.27)	(4.37)	(3.68)
Observations	1,120	1,120	1,120
Akaike Inf. Crit.	10,263.60	10,139.00	9,902.80
Bayesian Inf. Crit.	10,313.80	10,194.20	9,957.98

Notes: Robust standard errors are in parentheses. Dunnet adjusted p-values for multiple comparisons: $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

the sessions in the SUGGEST and CONTROL treatments for each period in block 2.[14] We see that in the first period of intervention the suggestion policy has a large statistically significant effect (period 11: $p = 0.041, r = \frac{z}{\sqrt{N}} = 0.547$) but that this effect fades gradually (period 12: $r = 0.453$; 13: $r = 0.378$; 14: $r = 0.224$; 15: $r = 0.242$) and loses statistical significance (Period 12: $p = 0.09$; 13: $p = 0.157$; 14: $p = 0.402$; 15: $p = 0.365$). When we test the difference between the SUGGEST and CONTROL treatments for the succeeding periods (16-20) using the same Wilcoxon test, the effect remains positive; however, we cannot reject the null-hypothesis at any conventional level of significance for any of those periods. Thus, we conclude that the targeting policy achieves its goal and positively affects the level of sending although only in the short-run.

The effect of the subsidy policy 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 in block 2 (SUBHIGH: $p = 0.129$ corrected for MPC with Dunnet procedure; $\beta_{SH} = 10.233$; SUBLOW: $p = 0.396$ MPC corrected; $\beta_{SL} = 6.817$). If we compare the amount sent in the subsidy treatments to the control treatment applying to each period the non-parametric Wilcoxon test across aggregated averages over the sessions, we also see no effect of this policy in the short-run.[15] Put differently, we find no evidence of subsidy policy being an effective means of promoting trustful behavior in the short-run.

As a final note related to the effects of interventions on trust, we point out that these effects can be influenced by senders' heterogeneous response. We find some suggestive trace of non-linearities in the descriptive analysis. As the mass of sendings seems to move towards the treshold levels during the external intervention, we investigate this issue in the appendix B.4. In any case, our evidence on the average treatment effect – the generalised "lift-up" in sendings following the policies – remains the focus of this paper.

[14]Throughout the paper we estimate the exact Wilcoxon test based on the Shift Algorithm by Streitberg and Röhmel (1986).

[15]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: $p = 0.692; r = 0.106$

In block 3, we find no significant post-intervention effects. The amount sent in the control treatment does not differ significantly from the amounts in any other treatment.[16] However, the coefficients of all three treatment are positive which suggests no detrimental long-lasting effects of any of the policies we tested.

We conclude this section by analyzing the evolution of the amount returned R in absolute terms, and the trustworthiness rate r. At first, similar to (11) we estimate the following regression for the amount returned in block 1:

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

Similar to (12) we add the average amount returned in block 1 ($\bar{R}_{i,B1}$) to the estimations for blocks 2 and 3:

$$R = \beta_0 + \beta^R_{SG} SUGGEST + \beta^R_{SL} SUBLOW + \beta^R_{SH} SUBHIGH + \\ + \beta_{\bar{R}} \bar{R}_{i,B1} + \upsilon_i + m_p + \epsilon_{i,t} \quad (14)$$

We see that in block 2 the amount returned is significantly higher in the SUGGEST compared to the CONTROL treatment ($p = 0.009$ MPC corrected; $\beta^R_{SG} = 22.33$). In the case of the subsidy treatments we find no evidence that trustees change their behavior during the intervention. The amount returned is significantly higher in block 3 SUBLOW treatment ($p = 0.06$ MPC corrected; $\beta^r_{SL} = 15.649$). In sum, trustees reciprocate trust in all treatments both during and after the intervention, but send more in absolute terms under the suggestion policy.

[16] We also find no significant differences if we compare the treatments to each other.

Table 3: Determinants of the amount Returned (R) – estimation of equations 13, 14

	Returned (R)		
	Block 1	Block 2	Block 3
SUGGEST	1.28	22.33***	12.00
	(10.34)	(7.60)	(7.09)
SUBHIGH	−12.74	15.25	7.24
	(10.27)	(7.85)	(7.01)
SUBLOW	0.36	8.32	15.65*
	(10.23)	(7.50)	(6.86)
Av. Returned in B. 1 ($\bar{R}_{i,B1}$)		0.89***	0.78***
		(0.07)	(0.06)
Constant	51.27***	0.28	−8.49
	(7.28)	(6.51)	(5.91)
Observations	1,120	1,120	1,120
Log Likelihood	−5,995.61	−5,841.07	−5,744.68
Akaike Inf. Crit.	12,011.20	11,704.10	11,511.40
Bayesian Inf. Crit.	12,061.40	11,759.30	11,566.50

Notes: Robust standard errors are in parentheses. Dunnet adjusted p-values for multiple comparisons: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

It is possible that trustees return a disproportionate amount compared to what they receive (e.g. this might apply to the case where subsidy or suggestion crowd out trustworthiness). This, in turn, can reduce the trustor's contribution. To address this concern, in the next regression we assess the determinants of trustworthiness ($r = \frac{R}{3 \cdot s}$):[17]

$$r = \beta_0 + \beta^r_{SG} SUGGEST + \beta^r_{SL} SUBLOW + \beta^r_{SH} SUBHIGH + \\ + \beta_s s + \upsilon_i + \epsilon_{i,t} \quad (15)$$

In line with the theoretical predictions, over the entire experiment we find no significant differences in trustworthiness rate among the CONTROL, SUBHIGH, and SUGGEST treatments (see appendix B.2).

[17] In regression 15 we control for the amount sent (s) since not only the amount returned but also trustworthiness (proportion returned) depend on the amount sent (see the meta-analysis of the trust game in Johnson and Mislin, 2011). However, the results remain robust even if the amount sent is not included.

Lack of difference in trustworthiness during the intervention is evidence that the policy does not crowd out trustworthiness: Trustees return a proportional amount (reciprocate trust) under policy intervention as under control condition. It is especially interesting that we observe no difference in trustworthiness between the treatments with subsidy and control during the intervention period: The subjects exposed to subsidy do not change their behavior significantly. This suggests indirectly that unconditional subsidies do not produce a crowding out effect.

To sum up, the suggestion (targeting) policy increases the amount sent in the short-run, while we find no evidence that subsidy policies alter the amount sent in a statistically significant way. Given that in all treatments trustees return an amount that is proportional to the amount they received, a suggestion policy seems to be a more attractive tool from a social welfare perspective. In the next subsection, we examine this finding in more depth focusing on the welfare effect of the interventions.

5.3 Net Payoff

To obtain insights into the welfare impact of our interventions, we consider how the reaction to different policies is reflected in the variation in net payoffs. More precisely, 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 the subject receives: $\pi_N = \pi - Z$. We subtract the value of the subsidy to account for the third party's cost, given that our focus is on the social welfare effect of the policy, not on the individual benefit.[18] The following mixed-effect model is estimated for block 1:

$$\pi_N = \beta_0 + \beta^{\pi}_{SG} SUGGEST + \beta^{\pi}_{SL} SUBLOW + \beta^{\pi}_{SH} SUBHIGH + \\ + \beta_{TR} TR + \upsilon_i + m_p + \epsilon_{i,t}, \quad (16)$$

where $SUGGEST$, $SUBLOW$, $SUBHIGH$ are dummy variables that are equal to 1 for the corresponding treatments. TR is a dummy vari-

[18]The effects of policies on gross payoffs are reported in appendix B.3. Results show that even if we do not account for third-party costs, the individual payoffs under the subsidy policies are comparable to the payoffs under the suggestion. However, under the subsidy policies subjects receive an additional payoff simply by exploiting the subsidy.

able that is equal to 1 for the trustor and 0 for the trustee, υ_i is the random effect for subject i, m_p is the random effect for matching pair, and $\epsilon_{i,t}$ is the error term for subject i in period t. We estimate a regression similar to (16) for blocks 2 and 3 and add the average net profit from block 1 ($\pi_{Ni,B1}$) to account for the variance in profits across subjects:

$$\pi_N = \beta_0 + \beta^{\pi}_{SG}SUGGEST + \beta^{\pi}_{SL}SUBLOW + \beta^{\pi}_{SH}SUBHIGH + \\ + \beta_{TR}TR + \beta_{pi}\pi_{Ni,B1} + m_i + \upsilon_i + \epsilon_{i,t}, \quad (17)$$

The results are reported in table 4, panel A.

As expected, for block 2 we find a significant increase in net payoffs as a result of the targeting policy ($p = 0.028$ MPC corrected; $\beta^{\pi}_{SG} = 13.254$). However, we find no significant effect of subsidy during the intervention compared to the control, although both coefficients for this type of intervention are positive (SUBHIGH: $p = 0.169$ MPC corrected; $\beta^{\pi}_{SH} = 9.566$; SUBLOW: $p = 0.507$ MPC corrected; $\beta^{\pi}_{SL} = 5.809$). As a robustness check, we estimate a similar regression to (16) and (17), but, now, we interact player type (Trustor, Trustee) and the treatment dummy to account for the heterogeneous reaction of different types of players to the treatment (table 4, Panel B). The results remain unchanged ($p = 0.041$ MPC corrected; $\beta^{\pi}_{SG} = 17.693$). We cannot reject the null-hypothesis that different types of players receive different net payoffs depending on the treatment.

Finally, to understand whether the targeting policy outperforms any of the subsidy policies in the short-run (during block 2) in terms of net payoffs, we test $H0 : \beta^{\pi}_{SH} = \beta^{\pi}_{SG} \vee \beta^{\pi}_{SL} = \beta^{\pi}_{SG}$. However, we cannot reject the null-hypothesis at any conventional significance level ($p = 0.329$).

In sum, from a welfare perspective, we find evidence of the benefit derived from a targeting policy in the short-run compared to no intervention, but no evidence of benefit from a subsidy policy.[19] However, we find no evidence to support the idea that the suggestion (targeting) policy outperforms the subsidy policies.

[19] We provide a potential explanation in appendix B.4.

Table 4: Determinants of Net Payoff (π_N) by block – estimation of equation 16, 17

	Net Payoff (π_N)		
	Block 1	Block 2	Block 3
Panel A: Assuming Homogeneous Reaction of Trustor and Trustee			
SUGGEST	0.61	13.25**	6.47
	(5.98)	(5.15)	(5.73)
SUBHIGH	−2.20	5.81	5.97
	(5.95)	(4.97)	(5.57)
SUBLOW	−8.75	9.57	2.33
	(5.96)	(5.26)	(5.77)
Trustor (TR)	−64.38***	−19.64***	−12.18**
	(4.21)	(5.13)	(5.66)
Av. Net Payoff in B.1 ($\pi_{Ni,B1}$)		0.83***	0.85***
		(0.06)	(0.06)
Constant	175.19***	32.16***	19.34
	(4.72)	(10.81)	(11.99)
Panel B: Assuming Heterogeneous Reaction of Trustor and Trustee			
SUGGEST	0.80	17.69**	7.81
	(7.51)	(7.29)	(8.11)
SUBHIGH	−6.17	9.37	2.54
	(7.46)	(7.05)	(7.90)
SUBLOW	−12.88	15.00	−0.46
	(7.47)	(7.45)	(8.18)
SUGGEST × Trustor (TR)	−0.29	−8.88	−2.67
	(10.62)	(10.31)	(11.47)
SUBHIGH × Trustor (TR)	8.27	−7.10	6.84
	(10.56)	(9.96)	(11.17)
SUBLOW × Trustor (TR)	8.37	−10.82	5.51
	(10.56)	(10.49)	(11.51)
Trustor (TR)	−68.53***	−12.88	−15.05
	(7.49)	(8.22)	(9.25)
Av. Net Payoff in B.1 ($\pi_{Ni,B1}$)		0.83***	0.84***
		(0.06)	(0.06)
Constant	177.21***	28.46**	21.44*
	(5.30)	(11.39)	(12.68)
Observations	2,240	2,240	2,240

Notes: Robust standard errors are in parentheses. Dunnet adjusted p-values for multiple comparisons: $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

6 Discussion and Conclusion

Our analysis falls under the broad rubric of studies on monetary and non-monetary incentives and social preferences. We developed a model that predicts that a policy involving monetary incentives may be ineffective, as such crowd out other-regarding preferences for those subjects that comply with the policy. We assume that preferences are endogenous (Bowles, 1998) — once learned, a preference persists for some time. Therefore, a monetary intervention which eradicates social preferences has a negative effect on subjects' pro-social behavior after the intervention. In contrast, a policy using non-monetary incentives is momentary beneficial and also has no long run detrimental consequences because it does not affect other-regarding preferences.

The experimental results show that non-monetary incentives in form of suggestion are effective to foster pro-social behavior in the very short-run. 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 any detrimental effects in the long-run. In contrast, monetary incentives do not show effectiveness in either the short- or the long-run although the policy significantly affects the subjects' behavior during the intervention. To interpret this we turn to the taxonomy of incentive effects on preferences proposed by Bowles and Polania-Reyes (2012).

Their categorization refers to three mechanisms linking interventions and preferences: "bad news" — incentives provide information about a principal's interests, "control aversion" — incentives jeopardize self-determination, and "moral disengagement" — incentives activate a switch from pro-social to own payoff maximization mode of thinking. While we cannot completely rule out the role played by bad news, we focus on how control aversion and moral disengagement might explain the specific pattern of subjects' reactions to the subsidy policy.

Subjects respond to the monetary intervention but (1) their propensity to follow it is low and (2) those who follow the policy send the minimal amount required to obtain 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. Moral disengagement could explain why subjects mostly send the minimal amount: They switch their mode of thinking to maximization of their own payoff, hence, if they decide to follow the policy they minimize their costs by sending the minimum amount. Moral disengagement can occur also if subjects consider the subsidy as too low (Gneezy *et al.*, 2011). Although we designed the incentive scheme in such a way that the subsidy covers half of the average amount sent (or more than half of difference between average amount sent and the minimum amount required to receive it), we cannot rule out the low response to the subsidy policy being due to the level of incentive provided.

In relation to the post-intervention effect of the policies, despite finding no significant differences among treatments after the policy interventions, we observe that the targeting policy has the persisting effect of inducing subjects to send high amounts. This is interesting since it suggests that a targeting policy is likely potentially to have a long-lasting effect. However, this needs to be tested in further research.

It is interesting also that the trustworthiness rate is not affected either during the intervention or afterward. On the one hand, this is in line with the theory — the trustee's behavior should remain the same since the policies incentivize only trustors. On the other hand, given that trustees also receive the subsidies, this suggests that the presence of a subsidy is insufficient to crowd out other-regarding preferences. Rather, it is likely that crowding out occurs if the monetary incentives are conditioned on a certain behavior.

To conclude, we studied how subsidy and targeting (suggestion) policies affect trust (investment) in a trust (investment) game. We provided suggestive evidence that targeting is an effective instrument to promote trustful behavior, but only in the short-run; therefore, we recommend that a targeting (suggestion) policy should be considered as an instrument to foster (at least momentary) trustful behaviour.

Conflict of Interest: The authors declare that they have no conflicts of interest.

Acknowledgments: We would like to thank the German Science foundation (DFG) for the financial support of this project. We are grateful to Oliver Kirchkamp, Uwe Cantner, Arno Riedl for their useful suggestions and support. We would like thank Tobias Regner, Alexia Gaudel, Paolo Crosetto, the participants to the BEEW 2013, the 15th ISS conference 2014, and the Max Planck Institute of Economics seminars in Jena for their insightful comments. Finally, we would also like to thank an anonymous referee for the comments that helped improving the paper substantially.

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A Instructions

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:

$$10 \text{ points} = 0.35 \text{ Euro}$$

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.

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:

$$10 \text{ points} = 0.35 \text{ Euro}$$

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.

B Additional Analysis

B.1 Sent Back (Returned)

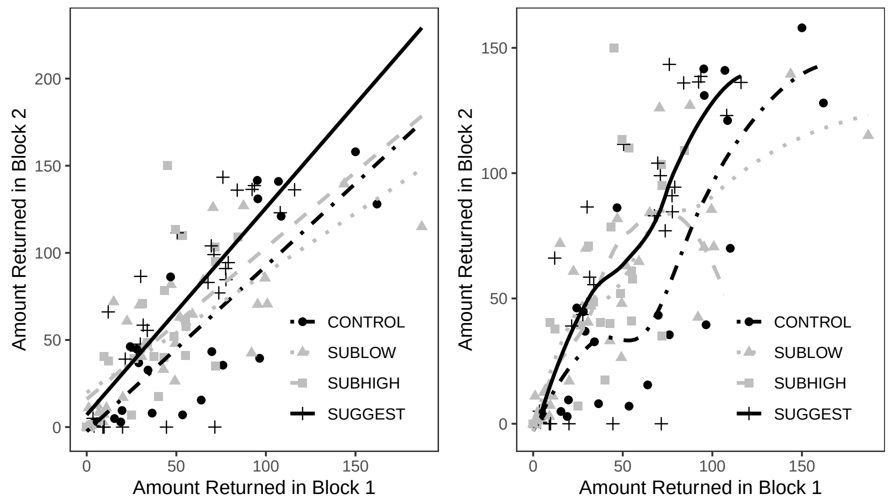

Figure 10: Relation between average amount returned ($\bar{R}$) in Block 1 and Block 2 with (a) linear and (b) loess smoothed conditional means.

B.2 Trustworthiness

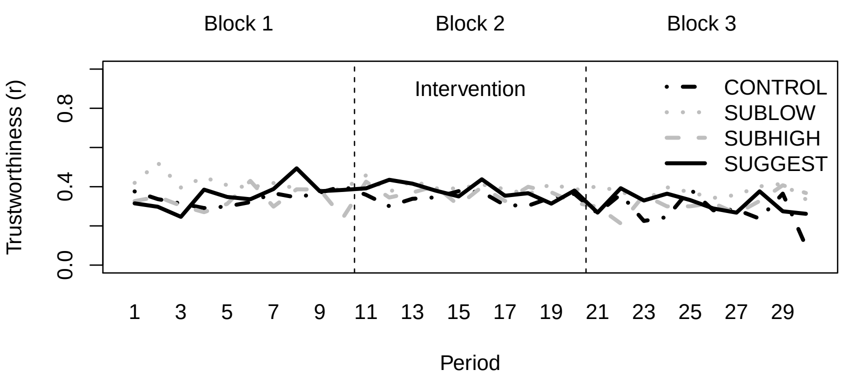

Figure 11: Average trustworthiness by treatment.

Table 5: Determinants of trustworthiness – estimation of equation 15

	Trustworthiness (r)		
	Block1	Block 2	Block 3
SUGGEST	0.01	0.04	0.06
	(0.05)	(0.06)	(0.07)
SUBHIGH	−0.004	0.03	0.05
	(0.05)	(0.06)	(0.07)
SUBLOW	0.08	0.09	0.10
	(0.05)	(0.06)	(0.07)
Sent (s)	0.001	0.001***	0.001***
	(0.0002)	(0.0002)	(0.0002)
Constant	0.26***	0.26***	0.22***
	(0.04)	(0.05)	(0.05)
Observations	864	853	683
Log Likelihood	170.50	309.72	214.27
Akaike Inf. Crit.	−319.01	−597.45	−406.55
Bayesian Inf. Crit.	−266.69	−545.27	−356.84

Notes: Robust standard errors are in parentheses. Dunnet adjusted p-values for multiple comparisons: $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

$$r = \beta_0 + \beta^r_{SG} SUGGEST + \beta^r_{SL} SUBLOW + \beta^r_{SH} SUBHIGH + v_i + \epsilon_{i,t} \quad (18)$$

Table 6: Determinants of trustworthiness – estimation of equation 18

	Trustworthiness (r)		
	Block1	Block 2	Block 3
SUGGEST	0.02	0.06	0.07
	(0.06)	(0.06)	(0.07)
SUBHIGH	−0.01	0.04	0.06
	(0.06)	(0.07)	(0.07)
SUBLOW	0.07	0.07	0.09
	(0.05)	(0.06)	(0.07)
Constant	0.34***	0.32***	0.26***
	(0.04)	(0.05)	(0.05)
Observations	864	853	683
Log Likelihood	146.59	293.83	203.46
Akaike Inf. Crit.	−275.17	−569.66	−388.93
Bayesian Inf. Crit.	−232.36	−526.97	−348.24

Notes: Robust standard errors are in parentheses. Dunnet adjusted p-values for multiple comparisons: $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

B.3 Gross Payoff

$$\pi = \beta_0 + \beta^{\pi}_{SG} SUGGEST + \beta^{\pi}_{SL} SUBLOW + \beta^{\pi}_{SH} SUBHIGH + \beta_{TR} TR + v_i + m_p + \epsilon_{i,t}, \quad (19)$$

Table 7: Determinants of Gross Payoff (π) – estimation of equation 19

	Gross Payoff (π)		
	Block 1	Block 2	Block 3
SUGGEST	0.61	6.50	6.47
	(5.98)	(4.46)	(5.73)
SUBHIGH	−2.20	10.27*	5.97
	(5.95)	(4.43)	(5.57)
SUBLOW	−8.75	7.01	2.33
	(5.96)	(4.50)	(5.77)
Player (P)	−64.38***	−12.41***	−12.18**
	(4.21)	(4.50)	(5.66)
Av. Gross Payoff in B1 ($\bar{\pi}_{i,B1}$)		0.87***	0.85***
		(0.05)	(0.06)
Constant	175.19***	21.35**	19.34
	(4.72)	(9.50)	(11.99)
Observations	2,240	6,720	2,240
Akaike Inf. Crit.	24,340.70	71,806.50	24,005.00
Bayesian Inf. Crit.	24,403.50	71,888.30	24,073.60

Notes: Robust standard errors are in parentheses. Dunnet adjusted p-values for multiple comparisons: $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

B.4 Crowding Out and Effect of Threshold

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

At first, we look at the subjects' 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 compared to the CONTROL treatment. We do this by estimating the following regression:

$$Pr(s \geqslant 70) = \mathcal{L}(\beta_0 + \beta^{\gtrless}_{SG} SUGGEST + \beta^{\gtrless}_{SH} SUBHIGH + \\ + \bar{Pr}_{i,B1}(s \geqslant 70) + \upsilon_i), \quad (20)$$

where $\mathcal{L}$ is a standard logistic function. The results are reported in Table 8.

Table 8: Determinants $Pr(s \geqslant 70)$ by block – estimation of equation 20

	$Pr(s \geqslant 70)$		
	Block1	Block 2	Block 3
SUGGEST	0.10	2.54***	0.79
	(1.53)	(0.90)	(0.77)
SUBHIGH	−1.74	2.19**	−0.17
	(1.54)	(0.87)	(0.82)
$\bar{Pr}_{i,B1}(s \geqslant 70)$		8.53	9.91
		(1.26)	(1.32)
Constant	−3.24**	−4.27***	−5.46***
	(1.32)	(0.78)	(0.83)
Observations	840	840	840
Akaike Inf. Crit.	563.58	576.62	390.38
Bayesian Inf. Crit.	582.51	600.29	414.05

Notes: Robust standard errors are in parentheses. Dunnet adjusted p-values for multiple comparisons: $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

We find a significant increase in propensity to follow the targeting policy in block 2 ($p = 0.0118$ MPC corrected; $\beta^{\gtrless}_{SG} = 2.54; e^{\beta^{\gtrless}_{SG}} = 12.62$). However, it is surprising to observe that subjects are significantly more likely to send the required amount also during the subsidy policy ($p = 0.0094; \beta^{\gtrless}_{SH} = 2.19; e^{\beta^{\gtrless}_{SH}} = 8.95;$).

This result is puzzling since in block 2 we observe that subjects do not send significantly more on average in the SUBHIGH than in the CONTROL treatment (see table 2 in section 5.2).[20] We can partially

[20] As well as given that we observe no significant growth in net payoffs during the subsidy policy (see table 4 in section 5.3).

explain this by the fact that subjects' propensity to follow the policy tends to be lower in the case of the SUBHIGH than in the SUGGEST treatment ($\beta^{\geqslant}_{SH} = 2.19 < \beta^{\geqslant}_{SG} = 2.54$; $\beta^{\geqslant}_{SH} = 2.19 < \beta^{\geqslant}_{SG} = 2.54$). Thus, given the sample size, we may not capture the effect directly.

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

$$Pr(s \geqslant 70) = \mathcal{L}(\beta_0 + \beta^{\geqslant}_{SG} SUGGEST + \bar{P}r_{i,B1}(s \geqslant 70) + \upsilon_i) \quad (21)$$

We find no significant difference in propensity to follow the policy between the SUGGEST and SUBHIGH treatments in block 2 ($p = 0.6346; \beta^{\geqslant}_{SG} = 0.4; e^{\beta^{\geqslant}_{SG}} = 1.49$). This suggests that another source of inefficiency is possibly at work. To find it we give 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 in block 2 is different for the SUBHIGH and SUGGEST treatments. Subjects tend to send not more than the minimum amount 70 required to obtain the subsidy in the SUBHIGH treatment, while in the SUGGEST treatment subjects also send more than the minimum suggested level (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:

$$Pr(s = 70) = \mathcal{L}(\beta_0 + \beta^{=}_{SG} SUGGEST + \bar{P}r_{i,B1}(s = 70) + \upsilon_i) \quad (22)$$

$$Pr(s > 70) = \mathcal{L}(\beta_0 + \beta^{>}_{SG} SUGGEST + \bar{P}r_{i,B1}(s > 70) + \upsilon_i) \quad (23)$$

Table 9: Determinants of $Pr(s = 70)$ by block – estimation of equation 22

	$Pr(s = 70)$		
	Block1	Block 2	Block 3
SUGGEST	1.27	−2.02***	−1.45
	(1.29)	(0.72)	(1.13)
$\bar{Pr}_{i,B1}(s = 70)$		15.51*	13.31**
		(8.48)	(5.43)
Constant	−6.81***	−1.84***	−4.59***
	(2.22)	(0.49)	(0.60)
Observations	560	560	560
Akaike Inf. Crit.	77.32	403.08	58.98
Bayesian Inf. Crit.	90.30	420.39	76.29

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

Table 10: Determinants of $Pr(s > 70)$ by block – estimation of equation 22

	$Pr(s > 70)$		
	Block 1	Block 2	Block 3
SUGGEST	1.47	2.47***	1.49
	(1.50)	(0.84)	(0.98)
$\bar{Pr}_{i,B1}(s > 70)$		8.05***	11.54***
		(1.39)	(2.13)
Constant	−4.90***	−5.20***	−6.66***
	(1.68)	(0.87)	(1.26)
Observations	560	560	560
Akaike Inf. Crit.	376.11	346.89	255.87
Bayesian Inf. Crit.	389.10	364.20	273.19

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

We report the results in tables 9 and 10. The probability of sending exactly 70 is significantly lower in the SUGGEST treatment compared to the SUBHIGH treatment during block 2 ($p = 0.0049; \beta^{=}_{SG} = -2.02; e^{\beta^{=}_{SG}} = 0.13$). In contrast, the probability of sending more than 70 is significantly higher in the SUGGEST than in the SUBHIGH treatment ($p = 0.0033; \beta^{>}_{SG} = 2.47; e^{\beta^{>}_{SG}} = 11.82$).

Moreover, applying the non-parametric exact Wilcoxon test across aggregated averages over the sessions, we reject the null-hypothesis that there is no difference in probability to send exactly 70 ($p = 0.0484$) and more than 70 ($p = 0.0484$) between the SUGGEST and SUBHIGH treatments during block 2.

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